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PHILOSOPHICAL TRANSACTIONS.

Giving Some

ACCOUNT

OF THE

Preſent Undertakings, Studies and Labours

OF THE

INGENIOUS,

In Many

Conſiderable Parts of the WORLD.

VOL. XXXI. For the Years, 1720, 1721.

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TO

Sir *Iſaac Newton*, Kt.

PRESIDENT,

AND TO THE

COUNCIL and FELLOWS

OF THE

Royal Society of *London*,

FOR

Improving Natural KNOWLEDGE,

This THIRTY FIRST Volume

OF

Philoſophical Tranſactions

Is Humbly Dedicated

BY

Their moſt Obliged,

and moſt Obedient Servant,

James Jurin, R. S. Secr.

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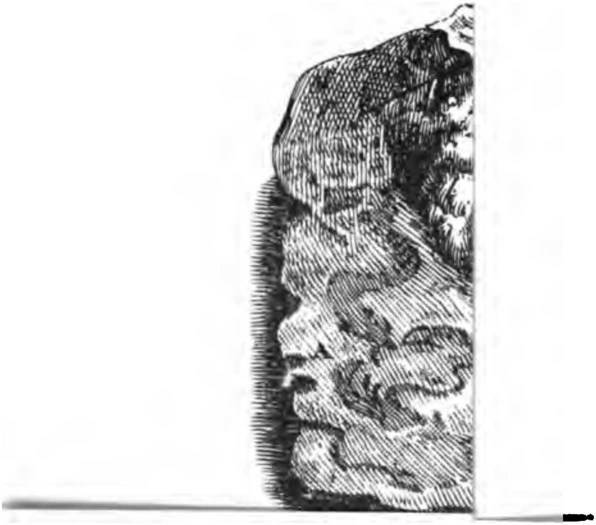

Numb. 364.

Beginning the Thirty Firſt Volume.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *January, Feb. March* and *April.* 1720.

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I. *Some*

I. *Some Remarks on a late Eſſay of Mr.* Caſſini, *wherein he propoſes to find, by Obſervation, the Parallax and Magnitude of* Sirius. *By* Edmund Halley, LL.D. R. S. S.

IN the *Memoires* of the *Royal Academie* of *Paris*, for the Year 1717. but now very lately publiſhed, there is one very remarkable Eſſay, by Mr. *Caſſini*, concerning the *Annual Parallax* of the *Fix'd Stars*, and particularly of *Sirius*; and in concluſion, he determines the Diameter of *Sirius* to be as much bigger than that of the *Sun*, as the *Sun*'s is greater than that of the *Earth*, which he ſuppoſes to be 100 times: And the diſtance from the *Sun* to the *Earth* being certainly about 100 Diameters of the *Sun*, it will follow, that the Globe of *Sirius* muſt be a Sphere, whoſe Diameter muſt equal the diſtance between the *Earth* and *Sun*.

To prove this, he tells us that he made uſe of an excellent Teleſcope of 34 *French* Feet, or 36 *Engliſh*, leaving an Aperture of but an Inch and half, to take off the ſpurious Rays of the *Star*, which then appeared round, and ſufficiently well defined; and comparing his Body to that of *Jupiter*, which he ſays, was then 50 Seconds Diameter, he found that the Diameter of *Jupiter* was ten times greater than that of the Star, which by conſequence was ſeen under an Angle of about 5 Seconds; which is his firſt Poſition.

Then he tells us, that to make the Obſervations of the Parallax of this Star with all the exactneſs poſſible, he employed a Teleſcope of *three Feet*, in a Copper

Tube, having fixed in the common *Focus* of the two Glaſſes, four threads croſſing one another in the Center, under Angles of 45 Degrees. This Tube he firmly fix'd to the Plain of a *Mural Arch*, which had been for above 30 Years immoveably cemented to the Wall of the *Royal Obſervatory*, to which he choſe to fix it, becauſe of the great Solidity thereof, and its being therefore the leſs liable to ſhake; and that after having ſtood 30 Years, there was no fear of its ſettling any further in the ſpace of one Year; beſides, that it was eaſy to perceive if any ſuch alteration ſhould happen to it.

Having therefore fix'd his three Foot Tube as above, ſo that, about the beginning of *April*, 1714. *New Stile*, (I ſuppoſe, becauſe then *Sirius* was in *Square* to the *Sun*) the Star being exactly in the *Meridian*, paſt over the Center of the Tube, he obſerved that on the 20th of *April* the Star touched the Horizontal Thread with its under edge, being apparently all above it, in the inverting Tube, but really below. On the 15th of *May*, and 6th of *June*, it paſt again by the Center. On *June* the 27th it appeared a little under, and on *July* the 9th it was found to touch the under part of the Thread. On *October* the 5th it again paſt by the Center; but on *December* the 29th, it touched the upper part of the Thread. *January* the 18th, 1715. being the coldeſt Day of that Winter, it paſt exactly by the Center; and on the 27th of *March*, and the 1ſt of *April*, it almoſt touched the upper ſide of the Horizontal Thread, from which it ſeem'd a little ſeparated. But on *June* the 7th, it paſt a little under the Center; and on *June* the 29th, the *Sun* being then in conjunction with *Sirius*, it paſt under the Thread, ſo as to touch it with its upper edge. Whence it appears, that in the ſpace of the whole Year, there had been no other variation of the *Meridian* Altitude of *Sirius*, than the breadth of the Thread, which appear'd equal to the Diameter of the Star, which he takes to be five, or at moſt ſix Seconds.

Suppoſing this to be ſo, he then ſhews that the whole Diameter of the annual Orb is to the diſtance of *Sirius*, as the Sine of 6″ to the Sine of 39° 33 the Latitude of the Star, whence the aforeſaid immenſe magnitude of the Body thereof, is a neceſſary Conſequence.

But before this obtain a full aſſent, it may not perhaps be amiſs to enquire whether the ſuppos'd viſible Diameter of *Sirius*, were not an Optick Fallacy, occaſioned by the great contraction of the *Aperture* of the *Object Glaſs:* For we all know that the Diameters of *Aldebaran* and *Spica Virginis*, are ſo ſmall, that when they happen to immerge on the dark Limb of the *Moon*, they are ſo far from looſing their Light gradually, as they muſt do were they of any ſenſible magnitude, that they vaniſh at once with their utmoſt Luſtre; and emerge likewiſe in a Moment, not ſmall at firſt, but at once appear with their full Light, even tho' the Emerſion happen very near the *Cuſp*; where, if they were four Seconds in Diameter, they would be many Seconds of Time in getting entirely ſeparated from the Limb. But the contrary appears to all thoſe, that have obſerved the Occultations of theſe bright Stars. And tho' *Sirius* be bigger than either of them, yet he is by far leſs than two of them; and conſequently his Diameter to theirs is leſs than the Square Root of 2 to 1, or than 14 to 10; whence, in Mr. *Caſſini's* excellent 36 Foot Glaſs, thoſe Stars ought to be about four Seconds in Diameter; and they would undoubtedly appear ſo, if view'd after the ſame manner; whereas we are *aliunde* certain, that they are leſs than one ſingle Second in Diameter. The great ſtrength of their native Light, forming the reſemblance of a Body, when it is nothing elſe but the ſpiſſitude of their Rays.

As to the other part of the Argument, that the alteration of the declination of *Sirius*, on the ſcore of the acceſs of the Earth in *December*, and its receſs in *June*, amounts to 6 Seconds; I can only remark, that, beſides that a *Radius* of 3 Feet, as it ſeems that made uſe of was no more, is ſomewhat too ſmall for ſo extremely nice an Obſervation, 6″ being ſubtended by the $\frac{1}{1000}$ part of an Inch, ſome of the Obſervations before recited do plainly ſhew, that the *Refraction* of the *Medium* did intermix with thoſe Differences that might be occaſioned by the *Parallax*.

But the principal Objection againſt the Concluſion of this Argument, ſeems to be, that the Meridian altitude of *Sirius* at *Paris* being under 25 Degrees, the ordinary Refraction of the Star is 1′ 55″ or 115 Seconds; and the Barometer riſing and falling above two Inches in Thirty, ſhews that the *denſity* of the *Air*, on that ſcore, may be a 15th part more at one time than another. Whence the Refractions being always proportional to the *denſity* of the *Medium*, as we have all ſeen it often demonſtrated by Mr. *Hawksbee*, both in *Vacuo*, and in a *doubly* and *trebly condenſed Air*, it is plain that in that Altitude the Refraction of a Star may differ about 7 or 8 Seconds, or the 15th part of 115″, which is more than the whole *Parallax* ſuppoſed to have been obſerved.

It were to be wiſh'd that Mr. *Caſſini* would pleaſe to try this Matter by the *Lucida Lyra*, inſtead of *Sirius*, which, tho' ſomewhat leſs than him, is as near to the *Solſtitial Colure*, and has much greater Latitude, being but 28 *grad.* from the *Pole* of the *Ecliptick*, whence its Parallax would be ſo much greater; and being at *Paris* within 10 *grad.* of the *Zenith*, the grand Objection of the difference of Refraction, would be almoſt wholly removed.

II. *An Account of the external Maxillar, and other Salivary Glands: Alſo of the Inſertions of all the Lymphaticks (as well above as below the Subclavians) into the Veins; which Glands and Inſertions have not hitherto been mention'd, or not truly deſcribed by any Authors.*

By Richard Hale, M. D. Reg. Soc. & Coll. Med. Lond. Soc.

THE external Maxillar Glands in Brutes are of the Conglomerate kind. They lie externally, laterally (lengthways) on the lower Jaw, partly under the *Depreſſor Labiorum*, and partly under the *Buccinator*. A ſtrong Membrane intervenes between theſe Glands and the Jaw on one ſide, and between them and the Buccal Glands on the other ſide. They are more or leſs red (like the *Pancreas*) according to the quantity of Blood that remains in them, otherwiſe their Subſtance is white.

Theſe Glands receive Arteries from the external Carotids, Veins from the external Jugulars, and Nerves from the third Branch of the *Par Quintum*.

The Number of excretory Ducts from theſe Glands, is not always the ſame, in the ſame ſpecies of Animals. In Cows generally fourteen are diſcovered by the Probe. Their Orifices are valvular, about four times leſs than their Ducts. Every Duct is about half an Inch from the next. Thoſe in the middle of the Glands are largeſt, becauſe the Glands are there broadeſt and thickeſt. The Ducts don't communicate with one another, nor with the Buccal. Every Duct is made of leſſer Ducts united, which riſe from the Lobules (thro' the whole ſubſtance of the

Glands)

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Glands) which conſtitute each diſtinct Lobe, and has the ſame ſtructure as the Pancreatick Duct. Each Lobe is depreſſed on its ſides, where it is joined to other Lobes; and between the Lobes many Buccal Glands are interſperſed.

In Calves ſeldom more than ſix or ſeven Ducts admit any Probe; when the Animal grows older, the Ducts appear more plain and open.

In Sheep ſix excretory Ducts are always found in each external Maxillar Gland.

In Dogs and Cats, *&c.* theſe Ducts are fewer, in proportion to the ſmallneſs of the Glands. 'Tis obſervable that theſe Ducts in Dogs open obliquely towards the Mouth, whereby the *Saliva* may be better mixt with the Food in Maſtication; which might be ſwallowed unmixt from another ſtructure of Ducts, in theſe Animals that ſwallow greedily.

Dr. *Wharton* * firſt mentions the external Maxillar Glands. What he ſays of them, is applicable only to their appearance in Men, in which Subjects they are of the Conglobate kind, and very ſmall, unleſs in Scrophulous and Venereal Caſes. 'Tis plain that he had not ſeen them in Brutes; for in his Figures (which were drawn from Brutes) no notice is taken of theſe Glands. He deſcribes them as very ſmall, and calls them Emunctories of the Nerves, which was the Notion (in his time) concerning the uſe of the Conglobate Glands; and the Saliva was ſaid † *è Nervoſo Genere profundi.*

⁎ *Steno* juſtly blames *Blaſius* for aſcribing to the external Maxillars an Excretory Duct opening into the Mouth, like the common one from the Parotid Gland. Yet *Steno* (otherwiſe very accurate) does not truly deſcribe theſe Glands, nor diſtinguiſh them from the Buccal, tho' they are as diſtinct from the Buccal, as the Sublinguals are from the internal Maxillars. *Steno* divides his Buccal into 3 Parts. The large Ducts in a Line riſe from the external Maxillars; and how diſtinct theſe Glands are from the Buccal appears plainly in Fig. 17th, &c. *Steno*'s 2d part of the Buccal. * *intra quæ, & in mediâ parte*, are mark'd *e e*, in Fig. 14. *quæ alias*, &c. higher are the ſame *e e*, among the *Papillæ*. The third Part *quæ à ſuperiore deſcendunt*, are *a b c d*.

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* *Cap.* 21. † *Cap.* 21. *pag.* 134. ⁎ *Obſ. Anat. p.* 14.

The external Maxillars differ from the Buccal in bigneſs, figure, ſtructure, particular number of Ducts, colour, &c. The Buccal, Labial, internal Maxillar, and ſublingual Glands, are of a yellow Colour; beſides the Buccal are ſeparated from the external Maxillars by a ſtrong Membrane. Indeed many of the Excretory Ducts of the Buccal Glands open near the Ducts of the Maxillars (from whence *Steno* confounded theſe Glands) but they do ſo likewiſe round his own Ducts from the Parotids; and ſome Ducts from like Glands open near the Sublinguals, as alſo about *Nuck*'s Ducts, in which places the Buccal Ducts are moſt numerous.

In ſhort, there is a very great Number of Excretory Ducts diſperſed all over the Membrane, that inveſts the Mouth, *Fauces*, &c. which riſe from Glands that lie under this internal Membrane. Theſe Glands are more numerous in ſome Parts than others, and receive different Names, according to the Part they belong to; as Labial, Buccal, Palatine, &c. But theſe are ſmall Glandules with one Excretory Duct, and tho' they ſeparate Saliva like the large Conglomerate Glands, Parotids, Maxillars, &c. yet they differ from theſe in Conſtructure, one common Excretory Duct, &c. Whereas the external Maxillars differ from all the other Glands of the Mouth, *viz.*

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* *Pag.* 18.

by many ways from the Buccal, beſides their Colour; in which particular, they are alſo diſtinguiſhed from the internal Maxillar and Sublingual Glands; they differ alſo from theſe as well as from the Parotids, in having a great number of common Excretory Ducts. This number of Excretory Ducts was not obſerved by *Steno*, nor did he know that theſe Ducts in the ſame Line, were the Excretory Ducts of large Conglomerate Glands (like the Parotids) diſtinct from the Buccal.

Bartholine * mentions the external Maxillar Glands, but does not deſcribe them. *Nuck* † only gives them a Place in his Catalogue of Glands, but takes no farther Notice of them, tho' he writes a * Book chiefly about a new Salival Duct riſing from a Gland, that is found in no Animal beſides a Dog.

Mr. *Cowper* had never ſeen theſe external Maxillar Glands, as appears by a Letter of his (now by me) written above Twenty Years ago, in anſwer to one I ſent him upon the firſt diſcovery of theſe Glands. The external Maxillars in Men (of the Conglobate kind) are marked *g*, in the firſt Figure of his *Myotomia Reformata*.

The Ducts of the external Maxillar Glands are oppoſite to the Orifices of *Steno*'s Ducts; from which Glands and Ducts, as alſo from the Buccal, Labial, and Gingival Glands, the Saliva flows from all parts of the Mouth without the Teeth. From *Wharton*'s and the Sublingual Ducts, from the Tonſils, *Fauces*, *Fretum Stenonis*, Gingival, Lingual and Palatine Glands, the Saliva is derived, from the upper and lower, former and hinder parts of the Mouth within the Teeth.

What has been ſaid of theſe Salivary Glands, *&c.* will be beſt underſtood by the following Figures, which were drawn for me in *October*, 1697. at *Trin. Coll. Oxon.* by Mr. *Burghers*, and have been lately compared with the Parts themſelves in Cows, Calves, *&c.* Theſe Figures are part of many more taken from Preparations at the ſame time, which were figured in the ſame Order as drawn. This is the Reaſon that the Cuts are marked in this manner, and it can't be of uſe to alter theſe Marks and Numbers.

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* *Pag.* 542. † Adenol. *p.* 5. *n.* 11.

* Sialog. *p.* 15. 158.

The Inſertions of all the Lymphatick Veſſels into the Veins can be diſcovered but in few Subjects, and no Figure has yet been given of them.

Theſe Figures ſhew the Courſe of the *Lympha* both below and above the Subclavians in Men, and Axillars in Dogs. The *Lympha* below the *Receptaculum Chyli* is conveyed from all the inferiour Parts by a great number of ſmall Lymphatick Veſſels, which uniting with others obliquely above the Valves, become bigger in proportion, till at length they conſtitute two large Trunks near the Emulgents, which are the *Pedunculi* or Beginnings of the *Receptaculum Chyli*. The *Lympha* from the Parts above the Subclavians, is derived in like manner from leſſer Lymphaticks, to the common Ducts that are here delineated.

I know *Pecquet* has given a Cut of the Thoracick Duct in a Dog; which Duct is double from the Receptacle, and is inſerted by four Branches into each Axillar. I believe with * *Bartholine*, (who has borrowed this Figure from *Pecquet*) that ſuch an Inſertion is a *Luſus Naturæ*. For tho' the Thoracick Duct may be double, and is ſometimes divided into two Parts near the Subclavians, yet generally it is ſingle, the *Lympha* from all parts on both ſides the Body being carried by proper Lymphæducts into one common Thoracick Duct, that conveys this Liquor, together with the Chyle from the Lacteals, into the left

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* Barth. p. 616, 620.

Subclavian Vein, by one, three, or more Branches. For there is as great a variety in the number of theſe Branches, as in the places of their Inſertion.

Mr. *Cowper* injected the Thoracick Duct in a Humane Subject, and has given a Figure of that Preparation in his Book of Anatomy. But this Figure is imperfect, and the Inſertion of the Thoracick Duct ſo ill drawn, that little can be learnt by it. However, no Anatomiſt has given any Cut, that demonſtrates the Inſertions of the Lymphaticks from both Arms and both ſides of the Head, *&c* above the Subclavian Veins, which appear ſo plain in theſe Figures, that no Deſcription can make them plainer.

Explanation of the Figures.

Fig. 12. Demonſtrates the Paſſages, or Veſſels, by which the Chyle and *Lympha* paſs into the Veins of a Dog.

12, 12 Thoſe Lymphaticks that bring *Lympha* from the Thighs and lower Parts.

13, 13 Are lateral Lymphaticks ariſing from the Groin, Teſticles, and neighbouring Parts.

14 The Receptacle of the Chyle.

15 An Indenture in the Receptacle, thro' which paſſes one Tendon of the Diaphragm.

16 Lymphaticks from a neighbouring Gland.

17 Some Lymphaticks from the Diaphragm.

18 An Artery that ſerves the Loins, and runs through a Diviſion of the Receptacle.

19 The *Pancreas Aſellii.*

20 The *Vaſa Lactea 2di Generis.*

21 The beginning of the *Ductus Thoracicus.*

22 Some Divarications of the *Ductus.*

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* Barth. *p.* 620.

23 The Continuation of the *Ductus*, and its progreſs.

24 The *Aorta Deſcendens*. *N B.* 18, 24. by their Pulſation (and the Tendon at 15) much promote the Aſcent of the Chyle and *Lymphæ*.

25 A common Divarication of the Duct.

26 A Lymphatick from ſome neighbouring Gland.

27 A double Lymphatick from the ſecondary Gland 42, in *Fig.* 13.

28 That part of the *Ductus Thoracicus* where both its Branches, and the Lymphaticks from the left ſide of the Head and left Fore-Leg meet.

29 The Lymphaticks from the left ſide of the Head and left Fore-Leg united; they lie on the inſide of the Vein.

30 A Lymphatick with a Pin in it from a neighbouring Gland, perhaps the *Thymus*.

31 A Lymphatick from the Neck, *&c.* It is divided and enters the Jugular by two diſtinct Branches under the *Sacculus* 43.

32 The Lymphatick from the right ſide of the Head.

33 The Lymphatick from the right Fore-Leg.

34 The large *Sacculus*, or Receptacle of the *Lympha*, on the right ſide, that receives all the *Lympha* on that ſide, and conveys it into the Jugular.

35 The *Cava Deſcendens*.

36 The *Vena Mammaria*, which is ſometimes ſingle.

37 The *Vena Subclaviæ*.

38 The *Vena Vertebralis*.

39 The Axillars.

40 The Jugulars.

41 The right internal Jugular not injected.

42 A ſmall ſecondary Lymphatick Gland on the back part of the top of the *Thorax*.

43 The *Sacculus*, that receives all the Chyle and *Lympha* from the whole Body (except 30, 31, 32, 33, 34.)

and diſcharges it into the Vein: at leaſt we know of no other Lymphaticks that any where elſe enter the Veins.

44 A Lymphatick, (or Membrane, for 'twas not injected) that joins 29 to the largeſt Branch of the *Ductus Thoracicus.*

Fig. 13. Is the upper part of *Fig.* 12. revers'd, the Duct, *&c.* being turned up, that the Inſertion, both *Sacculi, &c.* may be better diſcovered.

This is to be explained by the preceding, and has only from 42 to 44 more Figures than the upper part of *Fig.* 12. has; all which are already taken notice of.

N. B. In this Subject the Chyle and *Lympha* are emptied into the Jugulars, and not into the Axillars; they are ſometimes emptied partly into the Jugular, and partly into the Axillar, or Subclavian. In Men generally into the Subclavian.

Fig. 14. Repreſents part of the Left Cheek of an Ox, ſeparated from the lower Jaw-bone, with the external Maxillar Glands, its Ducts, *&c.*

1, 2, 3, *&c.* to 14. Briſtles inſerted into the Ducts of the external Maxillary Gland *l l l.* Theſe Ducts open ſloping into the Mouth, for the better mixture of the *Saliva* with the Food.

15 The Duct 3 injected with Wax, to diſcover its diviſion and bigneſs, in reſpect of the Orifice.

16 A Lobulus of the Maxillar Gland. Its excretory Duct is filled with Wax, and ends at 15.

17 The Duct 1 laid bare and open'd, to ſhew its large Cavity, *&c.*

A A, Part of the Muſcles and Fat, *&c.* belonging to the lower Jaw.

B B, Part of the internal Membrane that inveſts the Mouth.

a b c d, Briſtles in thoſe Ducts of the Buccal Glands, *n n*, that I could paſs any into.

e e e, Thoſe

e e e, Thoſe Orifices of the Buccal Glandules, that were too little to admit Briſtles.

k k k, The *Papillæ* on the inſide of the Mouth.

l l l, The Lobes that conſtitute the external Maxillar Gland.

m m m, The Orifices of the Labial Glandules *p p*, that were too ſmall for paſſing Briſtles.

n n n, Buccal Glandules interſperſed between the Lobules of the Maxillar Gland.

n n n near *r r r*, Part of the Buccal Glandules, where they appear thickeſt, and are rais'd to diſcover the Ducts *r r r*, running under them.

p p p, The Labial Glandules like the Buccal. Mr. *Cowper* in *Fig.* 4. letters them H, H.

r r r, The Ducts mark'd 6, to 14. as they appear under the Glandules *n n*.

N. B. The ſame Numbers and Letters expreſs the ſame Things in the following Figures.

Fig. 15. Exhibits part of the left Jaw-bone and Cheek of a Sheep, where the Briſtles 1, 2, 3, *&c.* ſhew the conſtant number of excretory Ducts from the external Maxillary Gland in theſe Animals.

Fig. 16. Shews part of the right Cheek of a large Dog, taken from the lower Jaw-bone.

f, The Orifice of *Steno*'s Salival Duct.

g, The Orifice of *Nuck*'s Duct, which riſes as a *Papilla* on the Membrane B B.

h, *Nuck*'s new Duct, not found in Men, Oxen, or Sheep, but in Dogs, their Orbit not being entirely bony.

i, *Nuck*'s Gland.

o o o, The Orifices of ſome excretory Ducts, belonging to the external Maxillar Gland, that were too ſtrait for the admiſſion of Briſtles.

q q, The Teeth. In this Subject they are the Teeth of the upper Jaw; near the ſecond of which, the Orifice of *Nuck*'s Duct appears.

Fig. 17. Demonſtrates the back part (next the Cutis) of the external Maxillar Gland of the ſame Dog, as 'tis beſet with the Buccal Glandules.

Fig. 18. Explains the external Maxillar Gland in the right Cheek of a Calf. In this Subject I could only probe two Ducts, ʒ, *&c.* would not admit Briſtles.

III. *De Peſte* Conſtantinopoli *graſſante. Auctore nupero* V. Cl. Emanuele Timone, M. D. *Hoc ſcriptum ab Auctore Clariſſimo, qui* Conſtantinopoli *per multos annos Medicinam fecerat, Excellentiſſimo Ablegato* Britannico, Roberto Sutton, *Eq. Aurato, traditum, ejuſdem Equitis permiſſu, cum Societate Regia communicavit* R: Hale, M. D.

PEſtem *Conſtantinopoli* ex *Ægypto* communicatam tum hiſtoriis, tum quotidianâ obſervatione conſtat. Nidum tamen in hac urbe ſibi fecit, &, quamvis nunquam fere veteris Peſtis deſint ſeminia, novus etiam ſubinde fomes advehitur. Ab hyemali intenſo frigore ſatis bene ſopitur; emicant tamen hinc inde ſcintillæ aliquot hyeme & vere: æſtate incrementum ſumit; autumnali tempore ſummo flagrat incendio. Venti Aquilonares ſtatis temporibus æſtate flantes (Eteſiæ ſcil. hujus loci) quamvis frigidiuſculi ſint, veneni tamen peſtilentialis dilatationi haud obſiſtunt. Auſtrales ſatis calidi, ſi conſtanter flent, æſtivo tempore Peſtem ſupprimunt. Quoad ſymptomata Peſtis *Conſtantinopolitana* adamuſſim reſpondet peſti

Noviomagenſi annorum 1636 & 1637, ab *Isbr.* de *Diemerbroeck* deſcriptæ, Humanum genus invadit *cap.* 4. *Diemerbr.*Compertum tamen eſt unum vel alterum equum, canem, ſelemve peſte bubonaria correptum, & mortuum. Vulgus (præcipue, cui auguſta domi res eſt) tum Turcarum, tum etiam Chriſtianorum, Hebræorumq; facile ſibi perſuaſum habet divinitûs miſſam peſtem; hinc non cavet à contagio: Politiores, & præcipuè Chriſtiani ſibi cavent.

Symptomata à * dicto auctore enumerantur iſta, Febres, bubones, carbunculi, exanthemata, capitis dolor phrenitis, ſopor, vigiliæ, anxietas, debilitas, ſeu magna virium proſtratio, viſus turbulentus, cordis palpitatio, linguæ ſiccitas, vomitus, ſingultus, lumbrici, fluxus alvi, hæmorrhagia narium, menſtruorum profluvium, ſanguinis mictus, ſputum cruentum, dolores lateris, hepatis, renum, aliarumve partium.

His adjicio ego laſſitudinem artuum ulceroſam, horripilationem, ſubſequente aliquando calore, ſæpius non ſubſequente, nauſeam ſine vomitu, vertiginem, & motus vertiginoſos, tremorem manuum ſtatim à morbi initio. Nullum ex omnibus ſymptomatibus eſt inſeparabile, imo nec tria illa peſti ſpecialia, bubones dico, carbunculos, & exanthemata; febris ſæpiſſime abeſt; ita ut regula iſta generalis ſtabiliri poſſit: *Quando adſunt manifeſta peſtis ſigna, certo ſtatuere poſſumus morbum eſſe peſtem: ſed quando abſunt ſigna iſta, non poſſumus certò ſtatuere contrarium.* Multis enim peſte correptis, leni præcedente rigore, quo nec catarrho ſe laborare quis ſuſpicaretur, per plures dies ex iſtis ſignis non manifeſtatur nec minimum, poſtea autem catervatim erumpunt. Nonnulli poſt ſuſceptum contagium leviſſimo tantum languore attacti, mox erectè obambulant, & conſueta munia ſine incommodo exercent; tertiâ autem, vel quartâ die ſtatim corruunt, animamque exhalant in viâ: & ne quis ſuſpicetur apoplexiam vel quid aliud, poſt mortem demum nigræ maculæ deprehenduntur: fidem etiam facit tum præcedens cauſa contagii maniſeſtiſſima, tum propagatum poſteà contagium. Multi etiam ſine ullâ manifeſtâ læſione erectè obambulantes ſanantur, erumpentibus etiam bubonibus & carbunculis. Plerique tamen febricitant, & graviter ægrotant. Vomitus & alvi fluxus cum ſubitâ virium concidentiâ, & cum febre peſtem indicant, & ſine febre adhuc magis. Minime ex ſignis ſuſpectis ſi dolor in emunctoriis communibus jungatur, pronunciandum eſt eſſe peſtem.

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* *Cap.* 7. Diemerb.

Puſtula rubicunda ad ſemi-ciceris magnitudinem in extremo purulenta maligna eſt: in lividum enim carbunculum brevi excreſcit. Carbunculi in omnibus corporis partibus indifferenter erumpunt, non exceptis labiis, linguâ, bulbis oculorum, glande penis, &c. Bubones tantum in emunctoriis. Glandulæ parvæ duræ circa collum malignæ. Exanthemata ſemper ſunt lethalia. Bubones ſi cito ad ſuppurationem tendant, ſpem promittunt. Non malum eſt, ſi etiam non bene maturi ferro aperiantur. Multi per bubonis reſolutionem curati, annis ſequentibus, ſi peſte infecta loca petant, eo in loco dolorem obſcurum ſentiunt, in quo bubonem habuerunt. Peſtis in aliquibus individuis per plures dies latitare poteſt, & poſtea in actum erumpere, A peſte convaleſcere incipiens, ſi ante quadrageſimum gravem aliquem in diæta errorem fecerit, novo erumpente bubone moritur. Pro monſtro habetur, ſi quis à peſte perfecte ſanatus eodem anno peſte corripiatur. Si diſpoſitionem corporum ad octo gradus placeat revocare; intacta peſte corpora diſpoſitionem habent ut ſeptem, & rudem merita ut unum. Infectas ædes per menſes aliquot impune incolens tandem peſte correptus fuit. * Senes difficillimè peſte corripiuntur, facillimè

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* *Cap.* 4. Diemerb.

pueri. Exteri multo magis periclitantur, quam Cives. Armenii omnium nationum minimè ad peſtem ſunt diſpoſiti. Obſervo illos pauciſſimis uti carnibus; cœpis, porris, alliis, vinoque maximè utuntur. Caro porcina in peſte præſentaneum venenum eſt. Nil æque ad peſtem diſponit ac animi pathemata, ac præcipue intenſus mœror, & terror inopinatus. Lue venereâ infecti indifferenter ſe habent ad contagium: bubones tamen illis ſuppurantes in fiſtulas ut plurimum degenerant. Nitidæ ædes haud æque facile inficiuntur ac ſordidæ. Cachectici, ictero laborantes, & variis aliis chronicis morbis conflictati homines dantur, & tamen peſtis ſpicula vel eludunt, vel ſuperant; roſeâ è contra facie præditi, & robuſtiſſimi moriuntur multi.

Suffumigia ex baccis Juniperi, pice, ſulphure utilia ſunt, ſi diu noctuque continuentur, ita ut ab inferiore parte totam penetrent domum. Odoramenta ex aceto haud inutilia cenſentur. Oleum ſuccini naribus illitum optimum eſt. Vinum moderate ſumptum, hilaritas animi, & bona victûs ratio haud parvi momenti ſunt in præſervatione. Phlebotomia damnatur, & præcipue ſera; ſanguis è venâ ſectâ fluens, nullo modo multoties ſiſti poteſt; ex ſcarificationibus etiam pro cucurbitularum applicatione factis ad plures horas, hoc eſt ad mortem uſque, ſanguinem vi magnâ effluxiſſe obſervatum eſt. Vomitoria levia non proſunt, fortia valde nocent: purgantia ſtatim enecant. Sola ſpes in ſudore, & alexipharmacis. Communiſſimè utuntur in hac urbe Theriaca Veneta. Lapis Bezoar, quamvis genuinus, nullius fere eſt momenti. Judæi acidis utuntur. Armenii & Græci vino, & ſpiritu vini. A carnibus, & carnium juſculis relligiosè abſtinent Chriſtiani ad multos dies. Turcæ etiam hanc diætam non ſpernunt. Multi, queis parvus tuberculus nullo livore inſignis erupit, à carnibus abſtinentes, per hebdomadam ſani obambularunt, poſtea carnibus uſi breviſſimè mortui ſunt. Opium crudum, & opiata omnia bona ſunt pro ſcopo præſervativo, ſæpe etiam pro curativo. Summum remedium Turcis magnatibus habetur oleum napthæ, ſeu Petroleum albicans, ad duas uſque drachmas epotum in vehiculo ad libitum; convenit hoc cum Camphorâ à noſtris tantopere celebratâ. Quidam multos ſanâſſe dicitur phlebotomiâ (primâ tamen morbi die) ad lipothymiam uſq; celebratâ, propinato poſtea hauſtu aceti acerrimi, in quo ſanguinis draconis & boli armenæ ā drachmam unam diſſolvebat. Phlebotomiæ negotium in medium examinandum relinquo; aſt certe præſtantiſſimum remedium eſt. Frictio rubificans tum in pectore, tum in dorſo, in locis utrinque cordi reſpondentibus, cum allio contuſo fortiſſimè per aliquot horas celebrata, proficua eſt. Radix hellebori, ſectione factâ, carni ad emunctoria tranſverſim infixa utilis eſt. Pulli gallinacei, vel columbini, ſeu vivi, ſeu per medium ſecti, bubonibus & carbunculis, & etiam regioni cordis appliciti, & ſæpiſſime iterati (ſed ultra ſemihoram non retinendi) utiliſſimi comperti ſunt. Oleum Succini, & extractum Juniperi in praxi fæliciter ſucceſſerunt. Methodus prædicti *Diemerbroeck* per omnia utilis eſt huic peſti; certè bene de Peſte ſcripſit auctor hic, excepto quod peſtis cauſam nimis alte deducat. *Barbette* etiam haud eſt contemnendus.

Anno 1712. Peſtis *Byzantii* in fine *Maii* Menſis incrementum ſumpſit. In fine *Julii* erat in ſummo furore. In determinato loco obſervatorem habui, qui mortuorum cadavera ad nonaginta & ſupra uno die numeravit. Eteſiæ flabant vehementiſſime; ceſſantibns Eteſiis, Auſter flavit ſatis intenſe. Primâ poſt Auſtri flatum hebdomadâ, cadavera non numeravit in die niſi circiter quadraginta; ſecundâ hebdomadâ triginta circumcirca;

cumcirca; tertiâ minus viginti: qui numerus in bona etiam urbis ſalubritate tali anni tempore ſingulis diebus non deeſt. Sic autumnali tempore peſtis ſopita eſt anno illo: ſed ut plurimum tali tempore ſævire ſolet, & mediâ æſtate, vel in fine æſtatis incrementum ſumere. Peſtem vere incipientem, autumno ſopitam alias etiam obſervatum eſt. Notandum eſt poſt ſolſtitium æſtivum peſtem in Ægypto, quamvis ferociſſimam, infallibiliter deſinere: cauſa altioris indaginis eſt. Climatis Ægyptiaci characteriſmus in hoc peculiari peſtis phænomeno aliquatenus ad Smyrnam uſq; inſulamque Chiam, ipſaſq; Helleſponti anguſtias ſeſe extendit. Obſervandum ulterius Eteſias, ventos ſcil. Aquilonares, curſum ſolis ſequentes, aerem vehere, non excrementitiâ ſolum humiditate, ſed nitroſis particulis valde ſaturatum & imbutum. Anſa mihi fuit hujus rei cognoſcendæ veſtis quædam, quâ nos utimur ex pilis caprarum textâ. Veſtis hæc Hygroſcopii quoddam genus videtur; nam quâ parte ſudore manuum, vel colli, fuerit antea madefacta, quamvis ſereſcit, & poſt multos etiam menſes in uſum denuo accipiatur, horâ tamen dimidiâ, vel unâ poſt ortum Solis, quando nimirum Eteſiæ ſpirare incipiunt, talem madorem denuo contrahit, ut maculâ nigrâ intenſiſſimâ inficiatur. Veſtis poſt duas, vel tres horas denuo exſiccatur, & macula evaneſcit. Hic duo ſunt notanda: primum eſt; ſi ſimplici aquâ veſtis illa fuerit ſæpius imbuta, & poſtea ſereſcat, haud ſuccedit hoc phænomenon madoris & maculæ, prout quando ſudore imbuta eſt: ſecundum conſideratione dignum eſt hoc: ſi forte fortunâ tempus ſit actualiter pluvium, haud æque facile madere inficiq; maculâ veſtem illam, dummodo actualiter pluviæ non exponatur; ex quibus colligo, præter aqueas particulas, nitroſis ſalibus imbutum aërem ex Ponto Euxino huc advehi primo Eteſiarum flatu. Particulæ iſtæ nitroſæ

a congeneribus

à congeneribus ſalinis particulis à ſudore veſti illi communicatis fixantur & retinentur, & combinatione factâ maculam efficiunt. Salibus igitur iſtis imbutus aer fermenti peſtilentialis (quod indolis ſalino-corroſivæ eſſe probabile eſt) dilatationi fomitem præbere poteſt. *Ægyptium* autem aerem, mediante vento auſtrino huc pervenientem, ſumme rarefactum & talium expertem ſalium, Peſtis dilatationi obſiſtere poſſe veriſimile eſt: Addè, quod calidus Auſter corpora ad tranſpirationem magis diſponat.

Si quis quærat interim, utrum Peſtis ſanguinis coagulationem vel nimiam diſſolutionem inducat? dico neutrum primario, & immediate facere, ſed utrumq; poſſe accidere tum proceſſu temporis, tum ratione maſſæ ſanguineæ, in variis individuis, quoad principia activa, variantis. Primario autem & immediate Peſtis ſpiritus adoritur: liquidis ſubſequenter, & ſolidis varia inducit mala, ſecundum varium tum liquidorum, tum ſolidorum ſchematiſmum. * Hinc varia ſymptomata in peſte, & ſibi è diametro oppoſita, quæ unico remedio vinci non poſſunt, ſed prudentis Medici requirunt Judicium. ut, quod opportunum eſt, in actum deducatur. Generaliter tamen obſervandum, ut citiſſime generoſiſſima remedia, & in magna doſi propinentur: ut in principio oleoſo-balſamica, & ſpiritibus amica volatilia uſurpentur; ut omnes evacuationes præter ſudorem vitentur; artificiales igitur non tentandæ, naturales ſiſtendæ ſuis quæque appropriatis; (ſic alvi fluxus ſalutariter ſedatus fuit clyſtere aſtringente, cui Theriaca erat immixta,) ſi ſpiritus ſint effrænes & peccent in motu aucto, quod à Vigiliis potiſſimum cognoſcitur, ut ſiſtantur per Opiata. Si contra necroſi, ſeu languore afficiantur, quod ab effectibus potiſſimum ſoporis cognoſcitur, ut ſuſcitentur per volatilia, & Cam-

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* Diemerb. *cap.* 7.

phorata: ut ſanguinis conſiſtentia integra ſervetur in proceſſu, mali, quod fit acidis & terreis adſtringentibus, ut Venenum ad peripheriam alliciatur, &c.

Anno 1714. **Emanuel Timonius,**
Conſtantinopolitanus.

IV. *An Account of a Luminous Appearance in the Air at* Dublin, *on* January *the 12th,* 17$\frac{19}{20}$. *By* Philip Percival, *Eſq; Brother to the Right Honourable the Lord* Percival, *and communicated by his Lordſhip to the Royal Society.*

I Here ſend you a Sketch of an odd appearance in the Sky; it began about 10 a Clock on *Tueſday* laſt, but nothing very remarkable till about half an Hour after Eleven, when I was call'd out to ſee it, by the Servants, who had been looking at it about half a quarter of an Hour, and told me it looked juſt like Fire. But it appeared firſt to me in long ſtreams of light, of a round Body, as at A, and very bright, tho' ſome were coloured, as at A *a*. They came before the Wind, which was then Weſt, as near as I could gueſs, there not being a Cloud in the Sky, and the brighteſt Moon I have known. We had Rain about Five, but at 6 a Clock the Night was clear. The Streams of Light A A, moved very ſlow, (there being but little Wind) but as they moved they joined, and, ſwelling out in the middle, formed themſelves into the Figure *b b* B, continuing to advance ſlowly in that ſhape for about a Minute, when the two Ends *b b*, approaching near each other, as deſcribed by the prick'd Lines, the advanced part B, ſuddenly, and with great ſwiftneſs, ran back, and joining it ſelf with the Ends *b b*, formed it ſelf into the Figure C, quivering in the upper part, and darting down perpendicularly in ſharp Points, as at D D D; and its Colour from a bright Light changed into the colours of a Rain-bow, but much fainter. It continued this way about a Minute, and then the ſharp points D D D, gathering themſelves up into C, it changed again into a ſquare Sheet of Light, as at E, and ſwell'd out at F, as before at B; and advancing leiſurely, repeated the ſame Scene as before, till it ſeem'd at a great diſtance to diſperſe it ſelf into ſmall thin light Clouds; tho' 'tis probable that to thoſe who ſaw it in a like Situation, as it travell'd, it might make the ſame appearance as it did to me. I was very particular in obſerving it, and the next Morning drew it, and I think very exactly. I ſhould have continued longer to look at it, (which I did for above a quarter of an Hour) but that it was exceſſive cold; the beginning of it was very like the *Aurora Borealis*, which has been very frequent this Winter here.

V. *Of the Infinity of the Sphere of Fix'd Stars.*

By Edmund Halley, L. L. D. R. S. S.

THE Syſtem of the World, as it is now underſtood, is taken to occupy the whole *Abyſs* of *Space*, and to be as ſuch actually infinite; and the appearance of the Sphere of Fixt Stars, ſtill diſcovering ſmaller and ſmaller ones, as you apply better Teleſcopes, ſeems to confirm this Doctrine. And indeed, were the whole Syſtem

ſtem finite; it, though never ſo extended, would ſtill occupy no part of the *infinitum* of Space, which neceſſarily and evidently exiſts; whence the whole would be ſurrounded on all ſides with an infinite *inane*, and the ſuperficial Stars would gravitate towards thoſe near the center, and with an accelerated motion run into them, and in proceſs of time coaleſce and unite with them into one. And, ſuppoſing Time enough, this would be a neceſſary conſequence. But if the whole be Infinite, all the parts of it would be nearly *in æquilibrio*, and conſequently each fixt Star, being drawn by contrary Powers, would keep its place; or move, till ſuch time, as, from ſuch an *æquilibrium*, it found its reſting place; on which account, ſome, perhaps, may think the Infinity of the Sphere of Fixt Stars no very precarious Poſtulate.

But to this I find two Objections, which are rather of a Metaphyſical than Phyſical Nature; and firſt, this ſuppoſes, as its conſequent, that the number of Fixt Stars is not only indefinite, but actually more than any finite Number; which ſeems abſurd *in terminis*, all Number being compoſed of Units, and no two Points or Centers being at a diſtance more than finite. But to this it may be anſwer'd, that by the ſame Argument we may conclude againſt the poſſibility of eternal Duration, becauſe no number of Days, or Years, or Ages, can compleat it. Another Argument I have heard urged, that if the number of Fixt Stars were more than finite, the whole ſuperficies of their apparent Sphere would be luminous, for that thoſe ſhining Bodies would be more in number than there are Seconds of a Degree in the *area* of the whole Spherical Surface, which I think cannot be denied. But if we ſuppoſe all the Fixt Stars to be as far from one another, as the neareſt of them is from the Sun; that is, if we may ſuppoſe the Sun to be one of them, at a greater diſtance their Disks and Light will be diminiſh'd in the proportion of Squares, and the Space to contain them will be increaſed in the ſame proportion; ſo that in each Spherical Surface the number of Stars it might contain, will be as the Biquadrate of their diſtances. Put then the diſtances immenſely great, as we are well aſſured they cannot but be, and from thence by an obvious *calculus*, it will be found, that as the Light of the Fix'd Stars diminiſhes, the intervals between them decreaſe in a leſs proportion, the one being as the Diſtances, and the other as the Squares thereof, reciprocally. Add to this, that the more remote Stars, and thoſe far ſhort of the remoteſt, vaniſh even in the niceſt Teleſcopes, by reaſon of their extream minuteneſs; ſo that, tho' it were true, that ſome ſuch Stars are in ſuch a place, yet their Beams, aided by any help yet known, are not ſufficient to move our Senſe; after the ſame manner as a ſmall Teleſcopical fixt Star is by no means perceivable to the naked Eye.

VI. *Of the Number, Order, and Light of the Fix'd Stars. By the ſame.*

AT the laſt meeting of the Society, I adventured to propoſe ſome Arguments, that ſeemed to me to evince the Infinity of the Sphere of Fixt Stars, as occupying the whole Abyſs of Space, or the τὸ πᾶν, which at preſent is generally underſtood to be neceſſarily Infinite; and thence I laid before you what may ſeem a very *Metaphyſical Paradox*, *viz.* That the number of Fixt Stars muſt then be more than any finite Number, and ſome of them more than at a finite diſtance from others. This ſeems to involve a Contradiction, but it is not the only one that occurs to thoſe who have undertaken freely to conſider the nature of Infinite, which perhaps the very narrow limits of humane Capacity cannot attain to.

Since then, I have attentively examined what might be the conſequence of an Hypotheſis, that the Sun being one of the Fixt Stars, all the reſt were as far diſtant from one another, as they are from us; and by a due calculation I find, that there cannot, upon that Suppoſition, be more than 13 Points in the Surface of a Sphere, as far diſtant from the Center of it, as they are from one another: and I believe it would be hard to find how to place thirteen Globes of equal magnitude, ſo as to touch one in the Center: for the twelve Angles of the *Icoſaedron* are from one another very little more diſtant than from its center; that is, the ſide of the Triangular Baſe of that Solid, is very little more than the Semidiameter of the circumſcribed Sphere, it being to it nearly as 21 to 20; ſo that it is plain that ſomewhat more than twelve equal Spheres may be poſited about a middle one; but the Spherical Angles or Inclinations of the planes of theſe Figures being incommenſurable with the 360 degrees of the Circle, there will be ſeveral interſtices left, between ſome of the Twelve, but not ſuch as to receive in any part the thirteenth Sphere.

Hence it is no very improbable Conjecture, that the number of the Fixt Stars of the firſt magnitude is ſo ſmall, becauſe this ſuperior appearance of Light ariſes from their nearneſs; thoſe that are leſs ſhewing themſelves ſo ſmall by reaſon of their greater diſtance. Now there are in all but ſixteen Fixt Stars, in the whole number of them, that can indiſputably be accounted of the firſt magnitude; whereof four are *extra Zodiacum*; viz. *Capella*, *Arcturus*, *Lucida Lyra*, and *Lucida Aquilæ*, to the *North*; four in the way of the *Moon* and *Planets*, to wit, *Palilicium*, *Cor Leonis*, *Spica*, and *Cor Scorpii*; and five to the *Southward*, that are ſeen in *England*, viz. The

Foot and Right *Shoulder* of *Orion*, *Sirius*, *Procyon*, and *Fomalhaut*; and there are three more that never riſe in our Horizon, *viz.* *Canopus*, *Acharnâr*, and the *Foot* of the *Centaur*. But that they exceed the number Thirteen, may eaſily be accounted for from the different magnitudes that may be in the Stars themſelves; and perhaps ſome of them may be much nearer to one another, than they are to us; this exceſs of Number being found ſingly in the Signs of *Gemini* and *Cancer*. And indeed within 45 degrees of Longitude, or one 8th of the whole, there are no leſs than *five* of theſe *ſixteen* to be ſeen. If therefore the Number of them be ſuppoſed *Thirteen*, omitting Niceties in a Matter of ſuch Irregularity, at twice the diſtance from the Sun there may be placed four times as many, or 52; which, with the ſame allowance, would nearly repreſent the number of the Stars we find to be of the 2d magnitude: ſo 9 × 13, or 117, for thoſe at three times the diſtance: and at ten times the diſtance 100 × 13 or 1300 Stars; which diſtance may perhaps diminiſh the light of any of the Stars of the firſt magnitude to that of the ſixth, it being but the hundredth part of what, at their preſent diſtance, they appear with. But if, ſince we have room enough for it, we ſhould ſuppoſe the Sphere continued to 10 times the laſt, or 100 times the firſt diſtance, the number of Stars would be 130 000, and they would appear but with the 10 000th part of the Light of a firſt magnitude Star, as we now ſee it. This is ſo ſmall a pulſe of Light, that it may well be queſtioned, whether the Eye, aſſiſted with any artificial help, can be made ſenſible thereof. But 100 times the diſtance of a Star we ſee, is ſtill Finite: from whence I leave thoſe that pleaſe to conſider it attentively, to draw the Concluſion.

VII. *An*

VII. *An Account of the Method of making Sugar from the Juice of the* Maple Tree *in* New England. *By* Paul Dudley, *Eſq*; F. R. S. *Communicated by* John Chamberlayne, *Eſq.*

MAple Sugar is made of the Juice of Upland Maple, or Maple Trees that grow upon the Highlands. You box the Tree, as we call it, *i. e.* make a hole with an Axe, or Chizzel, into the Side of the Tree, within a Foot of the Ground; the Box you make may hold about a Pint, and therefore it muſt ſhelve inwards, or towards the bottom of the Tree; you muſt alſo bark the Tree above the Box, to ſteer or direct the Juice to the Box.

You muſt alſo Tap the Tree with a ſmall Gimblet below your Box, ſo as to draw the Liquor off. When you have pierced or tapp'd your Tree, or Box, you put in a Reed, or Pipe, or a bit of Cedar ſcored with a Channel, and put a Bowl, Tray, or ſmall Cask at the Foot of the Tree, to receive your Liquor, and ſo tend the Veſſels as they are full.

After you have got your Liquor, you boil it in a Pot, Kettle, or Copper. Ten Gallons will make ſomewhat better than a pound of Sugar.

It becomes Sugar by the thin part evaporating in the boiling, for you muſt boil it till it is as thick as Treacle. Ten Gallons muſt boil till it comes to a pint and half.

A Kettle of twenty Gallons will be near 16 Hours in boiling, before you can reduce it to three Pints; a good Fire may do it ſooner.

When you take it off, you muſt keep almoſt continually ſtirring it, in order to make it Sugar: otherwiſe it will candy as hard as a Rock.

Some put in a little Beef Sewet, as big as a Walnut, when they take it off the Fire to make it turn the better to Sugar, and to prevent its candying, but it will do without. A good large Tree will yield twenty Gallons. The Seaſon of the Year is from the beginning of *February* to the beginning of *April.*

Mr. Dudley *in a following Letter adds this Note.*

I have nothing to add to my Chapter of Maple Sugar, but that our Phyſicians look upon it not only to be as good for common uſe as the *Weſt India* Sugar, but to exceed all other for its Medicinal Virtue.

VIII. *Copy of an Affidavit made in* Scotland, *concerning a Boy's living a conſiderable time without Food. Communicated by* Patrick Blair, M. D. F. R. S.

Court of the Barony of Erroll, *holden at* Erroll, *upon the 26th day of* December, 1719 *Year. By Mr.* Charles Brown, *Bailly to the Right Honourable* David *Earle of* Northesk, *Lord* Roeſhill, *&c.* Gilbert Anthone, *Clerk*, Charles Gill, *Officer.*

Court lawfully fenced and affirmed.

THE whilk Day Compeared before the ſaid Bailly ſitting in Judgment, *James Jackſon* ſewar in *Carſe Grange*, and *Eliz. Bell* his Spouſe, who being purged of partial Council, ſolemnly Sworne and Interrogat, Depons, That *Gilbert Jackson*, their ſixth Son, being about Fifteen Years of Age, fell ſick, and complained of pains over all his Body, upon the 3d day of *February*, 1716. when King *George* his Army was marching by their Dwelling Houſe from *Perth* to *Dundee*; and toward the end of the ſaid Month, was ſeized with a violent Fever, in which he continued for three Weeks, and then recover'd.

2. That he fell in a Fever again the beginning of *April* thereafter, wherein he alſo continued for the ſpace of three Weeks; and during that Fever, he had a ſhaking in his Body, as if he had been paralytick.

3. Upon the 10th day of *June* following, he fell in a great Fever again, when he became Dumb, loſt his Stomach intirely, and the uſe of his Limbs, and continued without eating or drinking any kind of thing, tho' all means were uſed to make him do both; but recovered of his Fever upon the 17th day of *May*, 1717. but continued ſtill Dumb, without eating or drinking, or having the uſe of his Limbs, till the 10th day of *June* the ſaid Year, when he was again ſeized with an extraordinary Fever, and the next day recovered his Speech; but continued in the Fever, without eating or drinking any thing at all, or having the uſe of any of his Limbs, till the 11th day of *November* following; when he recovered his Health pretty well, and the ſtrength of one of his Legs. And thus he continued without eating or drinking, only waſhed his Mouth ſometimes with Water; and always, when he ſaw the reſt of the Family going to take any kind of Food, the ſight of it being altogether uneaſie to him, he retired.

Upon the 10th day of *June*, 1718 year, he fell in a Fever again, which continued till the beginning of *September* thereafter, when he recovered of the ſaid Fever,

tho'

tho' he never could be induced to take any kind of Meat or Drink; and thus he continued in pretty good Health, and freſh coloured, till the 9th day of *June*, 1719. when he was ſeized again with a ſevere Fever; and upon the 10th at Night, his Father preſſed him extreamly to take a little Milk boiled with Oat-meal, which at length he agreed to; and he took a Spoonful of it, which ſtuck ſo long in his Throat, that his Parents thought he had been choaked; and ever ſince he has taken a little Food, but ſo very little, that a Halfpenny Loaf ſerves him eight Days. That all the time he faſted, he never had any Evacuation either by Stool or Urine; and it was fourteen or fifteen days after he began to eat, that he got any benefit that way; and that he is now in pretty good Health, but ſtill wants the uſe of one of his Limbs. And this is the Truth, as we ſhall anſwer to God.

James Jackson,
Elizabeth Bell,
Charles Browne, Baily.
Gilbert Anthone, Clerk.

IX. *A Diſcourſe concerning a Method of diſcovering the Virtues of Plants by their external Structure. By the ſame.*

HAving hitherto delivered my Thoughts concerning the ſtructure of the Flowers, Fructification, various Methods, Generation and Nouriſhment of Plants, I now come to add ſomewhat concerning their Virtues. I cannot enough admire the Judiciouſneſs and Sagacity of the Ancients, who, without any of thoſe means made uſe of by the Moderns, have handed down to us ſuch an account of the Virtues of thoſe Plants, which by the unanimous Conſent of all Phyſicians and Pharmacians, are more particularly dedicated for uſe in Phyſick, that all the laborious Endeavours of their inquiſitive Succeſſors, have never been able to outdo them. It muſt have been a long Tract of Experience, which enabled *Dioſcorides* and *Theophraſtus* to collect and receive from their wiſe Anceſtors, ſuch a laſting Catalogue of the Virtues of Plants, as ſcarce any thing has been added to even to this day. The Royal Academy at *Paris*, has been at great pains to find out the Virtues of Plants by the Chymical Analyſis, and ſeveral other Experiments, of which we have the Abſtracts in *Tournefort*'s *Hiſtoire des Plantes aux environs de Paris*, and *Tauvry* his *Traité des Medicaments*: But theſe laborious Endeavours only ſerve to confirm what the Ancients advanced, without any new Diſcovery. For *Tournefort*, after having made the Experiments with the *Tourneſol* and blue Paper, and given an exact Account of the ſeveral active Chymical Principles, which are obſerv'd in ſuch and ſuch a Plant, uſually concludes, *ainſi il n'eſt pas ſurpenant s'il a de telles vertues. Therefore*, ſays he, *'tis not ſurpriſing if it is endow'd with ſuch Virtues*; which is nothing but giving a Reaſon why the Ancients believed they were good for ſuch a Diſtemper.

The Means uſed by our Foreſathers to diſcover the Virtues of Plants, and their Uſe in the ſeveral Diſeaſes, as they were the moſt ſimple, ſo they are moſt aſſiſting at this very time. It ſeems they have narrowly conſidered their *Facies externa*, and thus concluded; if ſuch a Plant partake of ſuch Virtues, ſuch another ſo very like to it, muſt be endow'd with the ſame, *v. g.*

Apium and *Fœniculum* have the ſame manner of flowering; both produce their Seed after the ſame manner; their Roots are both alike, being long, white, ſtreight, carnous, *&c.* Therefore ſince a long Tract of Experience, handed down by Tradition, ſhews that ſuch a Plant has ſuch Virtues, ſuch another like to it muſt have the ſame. Thus we find *Apium, Fœniculum, Petroſelinum,* all join'd together, and preſcrib'd as the opening Roots in the Diſpenſatory.

This induc'd that expert Botaniſt, and diligent Enquirer into the Knowledge of the *Materia Medica*, the Celebrated Dr. *Herman*, to lay down theſe general Maxims, *Quæcunque flore & ſemine conveniunt eaſdem poſſident virtutes*: And *Omnia ſemina ſtriata ſunt carminativa*. The late ingenious and accurate Natural Hiſtorian, ſometime a noted Member of this Society, Mr. *James Petiver*, a few Years ago obliged us with a Diſcourſe upon this Subject, printed in the Philoſophical Tranſactions, in which he obſerves, That the *Plantæ Umbelliferæ, Galeatæ, Verticillatæ, Tetrapetalæ, Siliquoſæ* and *Siliculoſæ*, for the generality, have a tendency to the ſame Virtue and Uſe. This was the occaſion of ſome Intercourſe betwixt him and me, in two or three Letters, printed in my Miſcellaneous Obſervations, in which he obſerves, that the *Plantæ Flore ſtamineo*, which he calls Blink Flowers; ſuch as Hops, Nettles, Docks, Sorrels, Betes, Blites, Spinage, *Oraches, Bonus Henricus*, or *Engliſh Mercury*, and *Kali minus album*, are all good Sallads, raw, or boil'd. Alſo the *Leguminoſæ*, or Pea kind; ſuch as Peaſe, Beans, *Phaſeoli*, are good nutritive Food for Men; and the Tares, Trefoils, *Medicæ, Loti*, and Saintfoins, are good *Pabulum* or Fodder for Beaſts. To theſe he adds the *Frumentaceæ* or *Cereales*; as the Wheat, Rye, and Oats, in *Europe*, and the Maiz, Millet, Panick, and *Sorgum*, in the *Indies*, make good Bread; and that from Barly and Rice we have good fermented and ſpirituous Liquors. To theſe he adds, that the *Iris*, or Flag-kind, in Foreign Parts, afford us prevalent Drugs, of no mean Virtue and Uſe; ſuch as Ginger, Galingal, *Turmerick*, *Zedoary*, *Caſumuniar*, and *Cardamoms*. The *Laurus*, or Bay-kind, has ſome noble Attendants of the ſame Tribe with it ſelf; ſuch as *Cinnamon*, *Caſſia Lignea*; *Malabathrum*, *Folium Indicum*, and the *Camphire* Tree.

In anſwer to his, I added, that all the *Pappeſcentes* & *Lacteſcentes*, ſuch as the *Sonchus*, *Dens Leónis*, *Hieracium*, *Lactuca*, *Cichoreum*, *Endivia*, *Tragopogon*, and *Scorzonera*, have the ſame Virtues, and ſerve for the ſame Uſes both in the Kitchin and Shops. All the *Aſperifoliæ*, ſuch as *Borago*, and *Bugloſſum*, are thoſe which are called Coolers in a more or leſs intenſe degree; for ſome are Aſtringent, as *Conſolida*, others Narcotick, as *Cynogloſſum*. All the *Galeatæ* and *Labiatæ*, for the moſt part conſiſt of ſubtile Particles, and are therefore Cephalicks; as *Lavendula*, *Roſmarinus*, *Majorana*, &c. *Mentha*, *Pulegium*, *Meliſſa*, Hyſtericks. Attenuaters and Inciders, as *Salvia*, *Horminum*, &c. A fourth Sort ſomewhat Aſtringent, as *Bugula*, *Lamium*, &c. So that by having an *Idea* of the Virtues of a *Majorana*, *Mentha*, *Salvia*, *Lamium*, we come to know the Virtues of all their Congeners. All the *Papavers* are Narcotick. The *Eſulæ* and *Tithymali* are Cathartick; tho' both theſe are Lacteſcent, yet they differ from thoſe which are Pappeſcent alſo. All the *Malva*'s are chiefly Emollient; the Pentaphyllous kind Aſtringent; as are alſo the Plantains. The Corymbiferous kind, are either Stomachicks, Hyſtericks, or Vermifuges. The Gentian Bitters, Stomachicks, Hyſtericks, and Febrifuges. The *Pomiferæ Scandentes*, as Cucumbers, Melons, &c. are Coolers; but ſome are Cathartick, as *Cucumis ſylveſtris*, and *Colocynthis*. The *Convolvuli*, as *Mechoacanna*, &c. are Purgative; to which *Jalappa*, by

Flower and Fruit, is near of kin. *Digitalis* and *Gratiola*, are Emetick and Purgative. The ſquamous and bulbous Roots are Emollient, and more or leſs Acrid. Thus *Allium*, *Cepa*, *Porrum*, unboil'd, are Hot, Diuretick and Lithontriptick. All the *Seda* are coolers.

Thus at the firſt view, without knowing the Chariſtericks ſo nicely as Botaniſts do, but only exactly obſerving the *Facies externa* of the Plant, when the Virtue of one Species is known, the Virtues of all the Congeners may be gueſs'd at, if not fully determin'd.

The next ſimple Method of the Ancients, to diſcover the Virtues of Plants, ſeems to have been the Taſte and Smell. Thus *Apium* and *Petroſelinum* have a Taſte reſembling to each other, therefore they are to be preſcribed together. The Seeds of *Fæniculum* and *Aniſum* have much of the ſame taſte and ſmell, and therefore both of them muſt be Carminative, or Expellers of Wind, *&c.* They had likewiſe recourſe to the Temperament and Qualities, ſuch as Hot and Dry, Cold and Moiſt, in the 1ſt, 2d, 3d, and 4th Degrees. But ſince the Taſte is not always the ſame in one Perſon, and that different Perſons have different Senſations; that, as being too much ſubjected to the different Tempers and Imaginations of People, is deſervedly exploded.

I have lately compos'd a Compendious Scheme of all the Plants us'd in Phyſick; in which, that I might render it leſs liable to Objection, and not ſeem to introduce any innovation in the diſtribution, I have not ſo ſtrictly obſerv'd the making their Characteriſtick Notes and Virtues agree, as the diſtributing them according to their Operations.

The firſt Diſtribution, is, by joining together all thoſe which are preſcribed under one Title in the Shops; ſuch as the Opening Roots, Emollient and Capillary Herbs, Cordial Flowers, hot and cold, greater and leſſer

ſer Seeds. In this I have not kept to the Diſpenſatory Catalogue, but have added ſeveral Congeners, that I might give a Specimen of what is propoſed concerning the Virtues and Characters. Thus I have added *Cuminum* and *Meum* to *Fœniculum*; *Laurus Alexandrina*, and *Hippogloſſum* to *Ruſcus*; *Alcea* to *Malva* and *Althea*; *Bonus Henricus*, *Atriplex*, *&c.* to *Beta*, under the Title of *Oleraceous Emollients*; *Lingua Cervina*, *Polypodium*, *&c.* to the Capillary Herbs; and ſo on in the Cordial Flowers, and hot and cold Seeds.

I have, 2dly, diſtributed the Plants into ſuch as are Altering and Evacuating. The Altering are divided into thoſe that conſiſt of Groſs, and ſuch as are ſaid to conſiſt of Tenuious and Subtile Particles. Thoſe conſiſting of Groſs Particles, are Aſtringent. Such as prevent Abortion and Ruptures, Stoppers of the *Fluxus menſtruus immodicus*, *Fluor Albus*, *Diarrhæa*, *Dyſentery*; good in Burnings, Bruiſes, Cancers, ſpitting of Blood. Groſs Medicines are Narcoticks, Vulnerary, good for Scrophulous Tumors, Squinancy, Refrigerators.

Plants conſiſting of ſubtile Particles, are Aperient; ſuch are all Opthalmicks, Arthriticks, Nephriticks, Lithontripticks, Diureticks, Hydropicks. They are alſo Pectoral, Anti-Apoplectick, Paralytick, Hyſterick, Hypochondriack, Provokers of Birth, Febrifuges, Scorbuticks, Stomachicks, Vermifuges.

The Evacuating Medicines are Emetick, or ſuch as work upward; or Laxative and Purgative, ſuch as work downwards. The Nutritive Medicines are the *Plantæ Cereales* and *Leguminoſæ*.

It is here to be noted, that I have not inſerted any Plant in this Table, but ſuch as are indigenals in *Britain*, or ſuch as are Cultivated in *Britiſh* Gardens; and to render it ſtill the more uſeful, I have added ſuch particular Parts as are uſed in the Shops; *viz.* the

Root, Herbs, Leaves, Tops, Flowers, Fruit, Nuts, Bark and Wood.

Having thus reduc'd within a ſmall compaſs the moſt conſiderable Virtues of Plants, both General and Specifick, and ſhewn the moſt eaſy, ſimple, and natural Method of diſcovering them, I would not be ſo far miſunderſtood, as if I were averſe from uſing other Experiments in finding them out. On the contrary, I could heartily recommend another Method, hitherto much neglected, and which I am convinc'd would be of great Uſe, if accurately gone about; and that is, their Infuſion in different Liquors, in order to find out the proper *Menſtruum* for extracting their more uſeful Parts.

Every Phyſician is ſenſible that there are ſeveral Simples, and theſe ſpecifick too, which adhibited in Subſtance, are of great efficacy; whereas, if their Contexture is diſſolved, their Parts can never be ſo reunited as to produce the ſame effect. Thus *Cortex Peruvianus* is never ſo effectual, as when given in Powder. That there are others which will communicate their uſeful Particles when infus'd, to one Liquor and not to another; and that the ſame Subſtance will impregnate two Liquors diverſely, according to the different *Menſtruums*. That expert Chymiſt, Mr. *Lemery*, adviſes to infuſe *Opium* in Water and Spirit of Wine, ſeparately; and after to mix both Infuſions together, in order to make the *Laudanum* or Extract; wiſely conſidering, that the Water will be impregnated by the more ſoluble ſaline Particles, whereas the Spirit will only imbibe the more reſinous; for Water is the proper *Menſtruum* for a ſaline Subſtance, which will not diſſolve in Spirit of Wine; this rather hardening and preſerving it from being diſſolv'd, either by Air or Water. Thus the moſt convenient way to preſerve the volatile Salt of

Animals, is to keep it among Brandy; and every one knows, that Water immediately diſſolves Sugar, which Brandy will not do. Therefore *Senna* will impart its purgative Quality to Water or Ale, having its ſaline Particles more diſengag'd; but the purgative Virtue of *Jallap* conſiſting in its reſine, requires Wine or Brandy for the *Menſtruum* or Diſſolvent.

Therefore, in my humble Opinion, a moſt proper Means to find out the Virtues of Plants, is to have recourſe to the proper *Menſtruums*. A Simple may be infus'd in Rain Water, Snow Water, or pure Fountain Water; if its Texture is looſe, and it abound with ſaline Particles, thoſe pure Elements will be impregnated by it; but if the Texture be more compact, firm and ſolid, if its Particles are more fix'd, Mineral Waters; or by the addition of a proportional quantity of the fix'd Salt of a Plant, a proper *Menſtruum* may be prepared. And next to the adhibiting of the Bitters in ſubſtance, ſuch as Wormwood, Gentian, and Camomile Flowers, this is the moſt convenient way of adminiſtring them; not but their Tincture extracted by Brandy or Wine may do very well; But ſince they abound very much with a fix'd Salt, a great deal of their Virtue may be communicated to a leſs ſpirituous Liquor, when a more ſpirituous will not extract it. The proper means to know which *Menſtruum* will beſt extract the more uſeful parts of any Simple, or rather ſuſpend its more ſolid Particles, is to uſe the Hydroſtatical Ballance; when having weighed the *Menſtruum* before infuſion, and after the *Materies* has been infus'd for ſome time, it will ſoon be obſerv'd by the Augmentation of the Weight, how far the *Menſtruum* is impregnated, and which is the moſt proper Diſſolvent. The propereſt Method of adhibiting the fix'd Simples, if not in ſubſtance, is by Decoction, Infuſion, or Tincture. (*N. B.*

It is call'd Infuſion, when the *Menſtruum* is either Water, Ale, or Wine; but a Tincture, when Brandy is employ'd;) and the beſt way to obtain the uſeful Particles of volatile, tenuious, or ſubtile Subſtances, is by Diſtillation. Theſe may indeed be proper Ingredients for an Infuſion or Tincture. But there are a great many fix'd Subſtances as improper for Diſtillation as the Volatile are improper for Extracts. Thus I have thought fit to ſhew the means of finding out the Virtues of Plants without diſſolving their Texture: But if any has a mind rather to do it by the Chymical Analyſis, this is not to diſſuade them.

X. *An Account of a Book, Entituled,* Geometria Organica, ſive Deſcriptio Linearum Curvarum Univerſalis. *Auctore* Colino Mac Laurin, *Matheſeos in Collegio Novo* Abredonenſi *Profeſſore, &* R. S. S.

THE Deſign of this Treatiſe, is to examine the various Methods propoſed by Mathematicians, for deſcribing Geometric Curves; and at the ſame time to demonſtrate a new one, infinitely more General than any hitherto publiſhed; built on thoſe Theorems propoſed by our Illuſtrious Preſident, at the end of his Enumeration of the Lines of the Third Order.

The great Improvements that have been made by moſt of the other Modern Geometricians, have related chiefly to the Lines of the Infinite Order; they have been ſo fond of applying their new Methods to Mechanic and Exponential Curves, (which undoubtedly ought to give place to thoſe that are more ſtrictly Geometrical) that they have neglected to cultivate Geometry after the moſt regular manner. The Writers on theſe Subjects commonly riſe at once, from conſidering the Lines of the Second Order, or Conic Sections, to thoſe of the Infinite Order, overlooking all the intermediate Ranks. And hence it was, that all the Orders of Geometric Curves lay unregarded, without the known Limits of Geometry, beſides the firſt two, and a few of the ſuperior Curves that had been conſidered with ſome particular Views, till that great Author, by enumerating the Lines of the Third Order, enlarged the Bounds of Geometry, and enriched it with almoſt Seventy new Curves. Their Properties which he has given, and the manner of deſcribing thoſe of them that have a *Punctum Duplex*, have almoſt brought them on a Level with the Lines of the Second Order; which alone had long uſurped the Place in Geometry.

After this great Example, 'tis attempted in this Treatiſe, to give an univerſal Deſcription of all Geometric Lines of the Third, or any Order whatſoever. But becauſe the higher Kinds cannot be deſcribed but by means of the inferior Sorts, ſome of theſe muſt be poſtulated to deſcribe thoſe: And becauſe ſtreight Lines are the ſimpleſt and moſt eaſily deſcribed, and are always the ſame, that is, of one Sort, therefore it was thought proper to inveſtigate of what uſe they alone might be, for deſcribing Lines of all the higher Orders, in the Firſt Part of this Treatiſe; an Abſtract of which has been publiſhed in the Tranſactions for *January* and *February* laſt. I ſhall only add, that beſides the Method of deſcribing the Curves, the Manner of determining their Aſymptotes and Species is alſo demonſtrated; and the more ſimple Curves of every Order, are particularly conſidered as Examples of the Method. In the Firſt Section the Lines of the Second Order are conſidered; in the Second, thoſe of the Third Order, that have a *Punctum duplex*; in the 3d Section, the Lines of the Fourth Order, and thoſe of the Third Order that have no *Punctum duplex.* In the laſt Section there are many various Methods of deſcribing the Lines of any Order.

In the Second Part, the Curves of the inferior Orders are made uſe of for deſcribing thoſe of the higher kinds. In the Firſt Section, the Theorems publiſhed by Sir *Iſaac Newton* at the end of the Enumeration of the Lines of the Third Order are demonſtrated. In the Second Section, Curves are ſubſtituted in the room of ſtreight Lines, in all the Propoſitions of the Firſt Part. From one of theſe Propoſitions, Lines of the 1024th Order may be deſcribed by making Angles move on ſeven Conick Sections; and by three Conick Sections more, Lines may be deſcribed above the 11,000th Order. Laſtly, theſe Theorems are applied to ſhew how the more Complex of the Infinite Order, may be deſcribed from the more Simple.

In the Third Section, ſome other Methods of deſcribing Curves are conſidered, that are not ſo general as the preceding, but give ſometimes more ſimple Methods of deſcribing ſome few Lines of the Superior Orders. Particularly the *Epicycloids* deſcribed by the Motion of any Curve, whether Geometric or not, upon another equal to it are eaſily conſtructed, and ſeveral Infinite Series of them rectified or meaſured by Arches of more ſimple Curves. In this Section, ſeveral other Deſcriptions of Curves are treated of, that have been propoſed by others. In the laſt Section, to ſhew the uſe of Curves in Natural Philoſophy, two of the moſt eminent Problems in Mathematical Philoſophy are ſolved. In the firſt, the Centripetal Force, by which a Body deſcribes any Curve, is inveſtigated after an eaſy manner; and a ſimple Conſtruction of all thoſe Curves that a body would deſcribe, if projected with the velocity it might acquire by falling from an infinite Height, in any Hypotheſis of Gravity, is demonſtrated. In the Second, 'tis found, that if any body deſcribe a Curve in a reſiſting Medium, the Reſiſtance is always as the Moment or Fluxion of a Quantity, that expreſſes the *ratio* of the Centripetal Force, to that Force by which it would deſcribe the Curve *in Vacuo*, multiplied by the Fluxion of the Curve. 'Tis alſo demonſtrated, that if a body deſcribe any Curve in a reſiſting *Medium*, which *in Vacuo* could have been deſcribed by a Centripetal Force, proportional to any power of the Diſtance, the Denſity of that *Medium* will be reciprocally as the Part of the Tangent intercepted between the Point of Contact, and a Line perpendicular to the *Radius* at the Center of the Forces. This Theorem is applied to ſeveral Curves; and then the 10th *Prop.* of the Second Book of the Principles, and all its Examples, are demonſtrated from it. Theſe Propoſitions are treated of here, not only becauſe they ſhew the uſe of Curves in Philoſophy, but becauſe more ſimple *Ideas* of the Deſcriptions of ſome Curves may be drawn from them, than from any other Method; and becauſe this is the Method, by which Nature herſelf deſcribes Curve Lines.

The whole is concluded by an attempt to draw a Line of any given Order, through any given Number of Points, that is ſufficient to determine the Curve. Thus if a Curve of the Order 2 *m* is to be deſcribed through as many Points, as determine a Line of the Order *m*, and three more Points, each of which are Nodes, formed by the concourſe of as many Arches of the Curve, as there are Unites in *m*, then the

Curve is determined, and a Method how to deſcribe it is demonſtrated. This, and ſome other Theorems relating to the Number of Points that determine Curves, and the manner of deſcribing them thro' theſe Points, conclude this Part.

This Book is Printed for William *and* John Innys,
at the Weſt End of St. Paul'*s Church Yard.*

ERRATUM.

P. 14. *l.* 5. For *Calf* read *Cat.*

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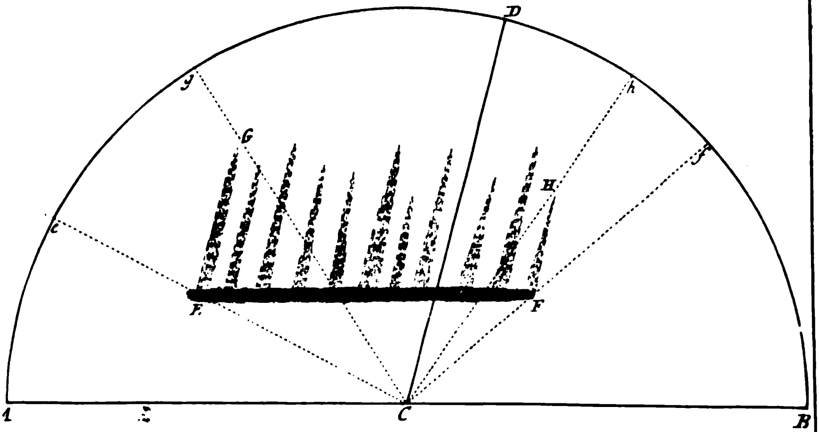

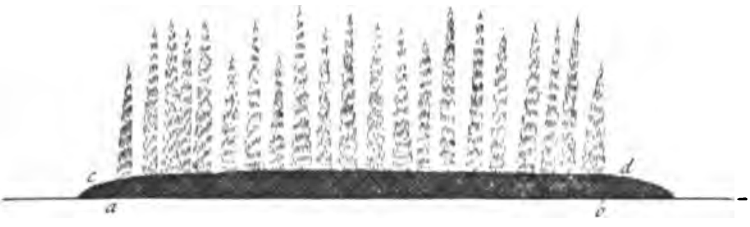

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PHILOSOPHICAL TRANSACTIONS.

For the Months of *May*, *June*, *July* and *Auguſt*, 1720.

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I. *Part of a Letter from Mr.* Deverel *Surgeon at* Briſtol; *concerning a Caſe in Chirurgery, which is commonly miſtaken for a Fracture of the Patella.*

RICHARD BURT, a Gardiner, living with Mrs. *Lloyd* of *Stoke* near *Briſtol*, was thrown from his Horſe, and in the fall receiv'd ſuch a hurt in one of his Knees as made him incapable of remounting: He felt ſomewhat crack in that Knee (as he expreſs'd it) before it touch'd the ground. Upon examining the Part I found (as I then thought) the ends of the broken Bone drawn above four fingers diſtance from each other: But upon a ſtricter examination of the parts, I found the *Patella* (which was drawn upwards by the extenſors of the Leg) retain'd its natural Figure, and that the hardneſs which was felt below was the end of the torn Ligament that ties it to the *Tibia.* The ends of the Ligament were brought as near as poſſible, and kept ſo about three Weeks without any very remarkable accident intervening. He then began to walk, which was a little too ſoon, cauſing thereby ſome pains, and looſening the Cicatrix, which made it the longer before it was perfectly firm; however he walks without any perceptible lameneſs. I have met with two others in the ſame caſe, the one a Perſon of diſtinction, who has not met with the good Fortune of walking ſo well as ſhe uſed, tho' not for want of all the care and circumſpection imaginable; for it is hardly to be expected that one in ten, to whom this Accident happens, ſhould ever go right, it being next to an impoſſibility that the ends of the torn Ligament ſhould be ſo exactly plac'd, and retain'd as not to lie one over the other.

Pareè in the 22d Chapter of his 15th Book affirms, that he never ſaw one of thoſe who have had this Bone broken, but that halted during the reſt of their lives: I am very apt to believe what this excellent Author affirms about the halting, and am ſorry I muſt diſſent from his notions of the caſe, and the cauſe of the Lameneſs. *Hildanus* in his *Obſervationes Chirurgicæ. Cent.* 5th. *Obſerv.* 88. P. m. 485, has given us a Hiſtory of a tranſverſe Fracture of this Bone, which, after all the Symptoms were remov'd, was cur'd. *Sed claudicatio ac ſumma imbecillitas totius cruris ſecuta eſt, ita ut non niſi maxima cum difficultate ambulare poſſit.* He afterwards mentioning the cauſe of the Lameneſs makes a Quæry, if what *Pareè* (to whom this Obſervation is dedicated) ſays in his *Exc. lib. de oſſibus, lib.* 4. *cap.* 2 can be the cauſe of the Lameneſs; his words are, *Etſi vero Opinionem hancce rationi conſentaneam eſſè in dubium revocare minimè conabor, unus tamen reſtat ſcrupulus, videlicet, an, fractâ patellâ, extuberantia calli talis eſſe poſſit, ut cavitatem hancce (quæ inter femur & os tibiæ magna eſt) adeo adimpleat, ut motum actionemque genu impedire poſſit? Videmus enim ut plurimum in reliquis oſſium fracturis, (niſi contritio oſſis & perioſtei fuerit maxima) naturam tam decenter & eleganter connectere oſſa, ut rarò relinquatur veſtigium fracturæ. Perioſtium enim, quam diu integrum eſt, materiam calli retinet, quo minus in extuberantiam excreſcere poſſit, niſi, &c. Deinde in hoc noſtro Ægro callum non fuiſſe cauſam claudicationis ex ſequentibus apparebit: quapropter diſtinctione hic opus eſt.* After this he goes on to ſhow us how many ways this Bone may be fractur'd, but doth not obſerve that he ever ſaw it any other way broken, (unleſs in a Gun-ſhot-wound) than tranſverſly; if he had, I am perſuaded, that this curious Obſervator would never have omitted it in his Obſervations.

Ruyſch in his *Centuria obſervationum Anatomico-Chirurgicarum 40. obſervatio* 3, writes as follows. *Patellam propter duritiem ſuam haud frangi poſſe nonnulli perverſe aſſerunt. Nos vero eandem non ſolum à caſu graviori in genu frangi experti ſumus, verum etiam ſine caſu; cujus rei Hiſtoriam recenſere animus eſt. Ante tres præterpropter annos, viſitavi cum Magiſtro Petro Adriani filio, virum ſatis robuſtum, qui a ponte deſcendens in terram ferè ceciderat; uno pede lubrico, reſiſtens tamen quantum potuit, in Terram non fuerat prolapſus; ſed ab illa reſiſtentia vehementiſſima tranſverſim fracta ejus patella, & adeo quidem evidenter, ut inter utrumque locari potuerit manus, una enim ſupra altera infra genu ſentiebatur.*

This Obſervation is ſo like in all its circumſtances to that which I related above, eſpecially in that the Fracture was made without a fall, that I am very apt to believe it is the ſame; and do imagine he might be deceived by miſtaking the hard end of the Ligament for the broken Bone.

And when theſe ſort of Accidents, which are now called Fractures of the *Patella*, come to be nicely examined; they will perhaps be found to be generally of the ſame Nature.

II. *A Letter concerning the Antiquity of the Venereal Diſeaſe. By Mr.* William Becket, *Surgeon*, F. R. S. *to* William Wagſtaffe, M. D. S. R. & Coll. Med. Lond. Soc. *and by him communicated to the Royal Society.*

S I R,

BEFORE I engage in the principal Deſign of this Letter; which will be to prove, that the Venereal Diſeaſe, when it came to be confirmed, was frequently known among us ſome hundreds of Years before the Siege of *Naples*: I ſhall endeavour to refute the Opinion of thoſe Perſons, who believe it to have had its riſe there, if any ſuch ſhall remain, who hav eread over my former Letter *. True indeed it is, that there have not been wanting ſeveral modern Authors, who have aſſerted it; but I determine to make it appear to be an Error as inconſiderately, and haſtily received, as ſtarted by ſome Chimerical Author; who, becauſe ſeveral Writers about that time, obſerving the Diſeaſe to begin in the *Pudenda*, ſeparated it from another, with which it was before confounded, muſt likewiſe take upon him to aſſert its being a new Diſtemper, and to aſſign a certain Time and Place for its Riſe. Now one might with all the Reaſon in the World expect, that, if the Diſeaſe had its Original there, it muſt have been ſo certainly and infallibly known, that there could have been no doubtful or uncertain Opinions about it, but

H that

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* *Philoſophical Tranſactions*, Num. 357.

that the Phyſicians, who reſided in or near the Place, and thoſe more eſpecially, who intereſted themſelves ſo far as to write of it, muſt have all of them, to a Man, agreed upon the Certainty of a thing, the Knowledge of the Truth of which was ſo eaſily attainable. But on the contrary, *Nicholas Leonicenus*, who was the firſt *Italian* Phyſician, that wrote of this Diſeaſe, and who lived at the very time, when *Naples* was beſieged, is ſo far from acknowledging it to have had its riſe there, from the *French* Soldiers Converſation with the *Italian* Women, and ſo little did he know of its true Cauſe, that he does not allow it to be the Conſequent of impure Embraces. About this time it was likewiſe, that Pope *Alexander* the VIth engaged *Gaſpar Torella* to write of this Diſtemper. This Pope was in League with *Alphonſus* King of *Naples*, againſt *Charles* VIII. King of *France*, to prevent his paſſing thro' *Italy* when he went to beſiege *Naples*; yet this Author is ſo far from allowing it to have had its Original there, that he tells us, the Aſtrologers were of opinion, that it proceeded from I know not what particular Conſtellations. Nor does *Sebaſtianus Aquilanus*, who lived at that time, allow it to be any other than an ancient Diſeaſe; or *Antonius Scanarolius*, who wrote in 1498, which was but four or five Years after the beforemention'd Siege. Nor do ſeveral other Authors, then living, ſay one Word about this *Neapolitan* Story. But it ſeems *Ulricus de Hutten*, a *German* Kt. who was no Phyſician, poſitively affirms this Diſeaſe to have had its riſe there; but how he ſhould come to know this, who lived at ſuch a diſtance from the Place, and they, who were Phyſicians reſiding as it were upon the ſpot, be ignorant of it, will be as much credited, as his following inconſiſtent Relation, which will ſufficiently prove, how little care he took to beappriſed of the Truth of what he wrote. This very Author tells us this Diſeaſe was unknown till the Year 1493, or thereabouts; that he himſelf had it, when he was a Child, and ſo conſequently that it was hereditary, or from the Nurſe. He wrote his Book of this Diſtemper at *Mentz*, where it was printed by *John Schœffer* in *4to*, in the Year 1519. Now if we allow him to be but 27 Years of Age, when he wrote, (for he cannot be ſuppos'd to be leſs, who before this took upon him to cure his Father of the Venereal Diſeaſe, without the Aſſiſtance of any Phyſician or Surgeon,) he muſt have had the Diſtemper upon him, according to his own Account, before ever it was in being. Thus we may ſee, how Perſons may be impos'd upon by a haſty and inconſiſtent Writer, no way qualified for ſuch an Undertaking, and greedily receive in Falſhoods inſtead of Truths, if they will not be at the Pains of conſulting the Original Writings of our Predeceſſors, the only ſure Method of overthrowing ſuch Chimerical and imaginary Notions.

But to come to what is principally deſigned in this Letter; If I have, *Sir*, in my former ſufficiently proved, that the firſt Degree of the Venereal Diſeaſe was very common among us ſome hundreds of Years, before it is commonly ſaid to have been known in *Europe*; there will be no Reaſon for any body to conceive we were at that time in any meaſure Strangers to it, when it came to be confirmed; more eſpecially, when we conſider the Methods of Treatment in thoſe Times, which conſiſting principally in topical Applications, many of their Patients could not poſſibly eſcape having it confirmed on them. Now when it was in this confirmed State, the Writers of thoſe early times looked upon it as an entirely new Diſeaſe, and not a

Conſequent of any Evil before contracted, becauſe they were not appriſed, that the firſt Symptoms being removed, and the Diſeaſe to Appearance cured, it ſhould afterwards diſcover it ſelf in ſuch a manner, as ſhould not ſeem to have the leaſt Analogy with the Symptoms, that firſt attack'd a part, which had been for a conſiderable time free from any Misfortune. But becauſe the Symptoms are the only true Characteriſticks, whereby we are infallibly able to know one Diſeaſe from another, it may be expected, that I produce ſufficient Authorities, to demonſtrate they were all of them known and deſcribed by ancient Phyſical and Chirurgical Writers, juſt as they appear to be in the Venereal Diſeaſe at this Day, if I would prove that Diſeaſe to be of a much more ancient Date, than is generally thought; and if I do this, I cannot but think it will be ſatisfactory, ſince we can have no other way of coming to a Knowledge of any one Diſtemper, than by its Symptoms. The Method of laying down the exact Succeſſion of them, will be impoſſible to be reduced to any certain and infallible Rule, there being ſo great a Variety of Cauſes, that obſtruct ſuch a Regularity; for which Reaſon, I ſhall take notice of them in ſuch Order as they moſt generally appear, which was upon no account to be expected from our ancient Writers, for as much as they mention every particular Symptom by it ſelf, not knowing but that they were independent of each other, and that each of them was a diſtinct Diſeaſe. However, the proving theſe Symptoms were in being in theſe early times, will be as ſtrong an Argument to prove the Antiquity of this Diſtemper, as if they had been regiſter'd in the moſt exact Order of Succeſſion, becauſe we ſhall, upon the ſtricteſt Examination,

tion, find they are peculiar to the Venereal Malady only. I have, I hope, ſufficiently made it appear in my former Letter, that the firſt degree of this Diſeaſe was anciently known among us by the name of the Brenning, or Burning; and that it was the ſame Thing with what we now call a Clap. The Symptoms, which are uſually its concomitants, are the Phymoſis, and Paraphymoſis, both which are accurately deſcribed, and proper Remedies for them ſet down by the before mentioned *John Arden* Eſq; in another Manuſcript of his, curiouſly written upon Vellum, and beautifully illuminated. The imprudent Method of Cure of this firſt degree of the Venereal Malady, is ſometimes attended with a Caruncle in the Urethra, which was a Diſeaſe very common among us anciently: For not to mention other early writers, our before mentioned Author gives us the Caſe of a certain Rector, that had ſuch a Subſtance, like a Wart, growing in the Penis, which in another Place he ſays frequently happens and of another, which had ſuch an Excreſcence as big as a ſmall Strawberry, which (ſays he) proceeded from the corrupted Matter, which remained in the Urethra.

And indeed there is not any Symptom of the Venereal Diſeaſe, that I find ſo often mentioned as this of the Caruncle, inſomuch that it ſeems to have been more common in thoſe early times, than at this Day. But this muſt be certainly owing to the ſmooth and oily Remedies they were continually injecting, which, by their relaxing and ſoftning the Fibres of the part, muſt neceſſarily diſpoſe the contexture of ſmall Blood Veſſels, lodged at the bottom of the little Ulcerations, to fill with nutritious juices, and to extend themſelves ſo, as to form ſuch fungous Excreſcences; and ſo ſolicitous were they for removing theſe inconveniencies, that they made uſe of ſeveral ways by Corroſives and other Methods, to accompliſh this end; and a very early Writer among us, has given us a very methodical and curious Tract on this Subject, wherein he recommends the removing them by the medicated Candle, which we uſe at this Day, and lays down divers other Inſtructions, in Relation to it, which makes it probably the beſt Diſcourſe on this Subject, that was ever yet written The ſame Author takes notice of thoſe contumacious Ulcers, which happen upon the *Glans* and the neighbouring parts, which we now call Shankers; and the great Trouble, our ancient Authors found in attempting their Cure ſufficiently diſcover them to have had their Original from a Venereal Infection. Theſe ſeveral Symptoms of the Venereal Malady our early Writers are very full in their Accounts of, and others, when the Diſeaſe was in a more confirmed State, to which they appropriated particular Names, perhaps more ſignificant and expreſſive than thoſe impoſed by modern Authors. Thus for inſtance, the Bubo's in the Groins they called Dorſers, which I have given a Reaſon for before; and the Venereal Nodes on the Shin Bones they termed the Boonhaw, which gives us a perfect Idea, not only of the part affected but after what manner it was Diſeaſed; for the old Engliſh Word, *Hawe*, ſignified a ſwelling of any Part. Thus for inſtance, a little Swelling upon the *Cornea*, was anciently called the Hawe in the Eye, and the Swelling that frequently happens on the Finger, on one ſide the Nail, was called the white Hawe, and afterwards Whitflaw. The proceſs our laſt mentioned Author recommends, for the Cure of the Boon or Bone Hawe, is by making uſe of a Plaiſter, which had a Hole cut in the midſt, to circumſcribe it; and applying a Cauſtick of unſlacked Lime, and black Soap incorporated together, which with Plaiſter and Bandage, was to be ſecured on the part four Hours, and longer, if that was not found ſufficient; after this he proceeds to the ſeparating the Slough, *&c.* This Practice of his ſeems to have been found out by accident. For he tells us, when he was a young Practitioner, he having applyed both the Natural and Artificial Arſenick to the Leg of a Man, that was his Patient, it ſo mortified the Fleſh, as ſurprized him; but by proper digeſtives, the Eſchar coming off, and leaving the Bone bare, he ſcraped it with an Inſtrument for ſeveral Days, and dreſt it with Incarnatives, deſigning to have ingendred Fleſh on it; but this proving unſucceſsful, he continued to ſcrape it, till he obſerved it move under the Inſtrument, after which having ſeparated it, he found the Sore covered with new Fleſh, and that the Bone was four Inches in length, two in breadth, and very thick, upon the removal of which the Patient was ſoon cured. Thus its probable this Obſervation of this great Man led our Predeceſſors to practice the very ſame Method; and we do at this Day in our Hoſpitals treat the Venereal Nodes on the Shins exactly as is here deſcribed, where we obſerve the ſame appearances, he ſo long before took notice of; and 'tis not in the leaſt to be doubted, but the Boon Haw and our Venereal Nodes are the ſame Diſeaſe. By the appearance of ſome of the laſt of the above mentioned Symptoms, we infallibly judge the Patient has had the Infection upon him a conſiderable time, and that the Diſeaſe is making its gradual advances, to the corrupting and deſtroying the whole frame of the Body. That this was the Concluſion of the miſeries of thoſe Perſons, that gave themſelves up to the deceitful delights and entertainments of lewd Women, in thoſe early times as well as now, I cannot better prove than by thoſe remarkable inſtances you quoted from a MS. in *Lincoln* Colledge, in *Oxon*, which you kindly communicated to me, after you had mentioned them in one of your Learned Lectures in our Theatre. They are as follow, *Novi enim ego Magiſter* Thomas Gaſcoigne, *licet indignus, ſacræ Theologiæ Doctor, qui hæc ſcripſi & collegi, diverſos viros, qui mortui fuerunt ex putrefactione membrorum ſuorum genitalium & corporis ſui; quæ corruptio & putrefactio, ut ipſi dixerunt, cauſata fuit per exercitium copulæ carnalis cum mulieribus. Magnus enim dux in Angli, ſcil.* J. de Gaunt, *mortuus eſt ex tali putrefacâtione membrorum genitalium, & corporis ſui, cauſatâ per frequentationem mulierum. Magnus enim fornicator fuit, ut in toto Regno Angliæ divulgabatur, & ante mortem ſuam jacens ſic infirmus in lecto, eandem putrefactionem Regi Angliæ Ricardo ſecundo oſtendit, cum idem Rex eundem Ducem in ſuâ infirmitate viſitavit; & dixit mihi qui iſta novit unus fidelis ſacræ Theologiæ Baccalaureus. Willus etiam longe vir maturæ ætatis & de civitat. Londonii, mortuus eſt ex tali putrefactione membrorum ſuorum genitalium & corporis ſui, cauſatâ per copulam carnalem cum Mulieribus, ut ipſemet pluries confeſſus eſt ante mortem ſuam, quum manu ſua propria eleemoſynas diſtribuit ut ego novi anno Dni.* 1430. Now what thoſe inſtances mentioned from *Arden*, or theſe from *Gaſcoigne*, who was then Chancellor of *Oxford*, cou'd poſſibly be but Venereal Caſes, I would be obliged to any body to inform me. Certain it is, no Diſeaſe was ever known to be gotten by the carnal Converſation of Women, which firſt attacked the *Genitals*, cauſing a Corruption and Putrefaction of them, and afterward of the whole Frame of the Body, but that which is Venereal. For nothing is more commonly known at this Day, than that after the venereal ingagement with an impure Woman, the Penis is the part where the Scene is firſt laid for the ſucceeding Tragical appearances; and there, and in the Neighbouring Parts, do the Symptoms of the Diſeaſe as its retainers, always firſt aſſemble, till the malignant Poyſon taints the Blood and other Juices; which being convey'd over the whole Frame of the human Fabrick, if not check'd, ſoon brings about its total Corruption.

What I have further to add in relation to this, is, becauſe we do not find the Diſeaſe mentioned by *Gaſcoigne*, was diſtinguiſh'd by any particular Name, and that great Numbers muſt unavoidably die of the Venereal Malady at that time, from the imperfect Knowledge of thoſe who had the treatment of the firſt Degrees of it, it muſt neceſſarily follow, that when the whole Frame of the Body had received a taint from the Venereal Poyſon, ſo as to occaſion its breaking out in Scabs and Ulcers, almoſt all over its Surface it muſt generally be called by the Name of ſome particular Diſeaſe, whoſe appearances had ſomewhat of an Affinity to it. Now if we examine the Nature of all the Diſeaſes that attack the Human Body, we ſhall not find the Venereal Malady, when it arrives at this State, to bear a greater Similitude to any than the Leproſy, as it is deſcribed by the Ancients: Nay ſo great was the Analogy betwixt theſe Diſeaſes ſuppoſed to be, that *Sebaſtianus Aquilanus* has endeavoured to prove from *Galen*, *Avicen*, *Pliny*, &c. That the *Pox* is only one Species of the Leproſy; and *Jacobus Cataneus*, a Writer almoſt as early as the riſe of the Name of the *Pox*, tells us 'tis not only poſſible there may be a tranſition, from one of theſe Diſeaſes into the other; but that he ſaw two Perſons in whom the *Pox* was changed into the Leproſy: That is, from having great Pockes or Puſtules on the Surface of their Bodies, from whence the *Pox* is denominated, to have become Ulcerous or Scabby. This particular State of the Diſeaſe anciently put the Surgeons to a great deal of trouble: For they finding that theſe Ulcers were of a very contumacious and rebellious Nature, they were obliged to make uſe of great numbers of remedies, in order to conquer the evil Diſpoſition of them. But they obſerved that all of them were uſeleſs, unleſs Mercury was joined with them. Now the dreſſing each particular Ulcer being ſo very tedious, they ordered the Patients to daub the Ointments over the Parts which were ulcerated; which done they were wrapt in Linnen Cloaths till the next dreſſing: But after a few Days they were extreamly ſurprized, to find their Mouths began to be ſore, and that they ſpit very profuſely; but they tell us to their aſtoniſhment, that in a little time the Sores became healed, and the Patients cured. And by this accident it was the Method of Salivating by Unction was firſt diſcover'd, which is in ſo much uſe among us at this Day. From theſe and ſome other inſtances I have given of the Induſtry and Application of our Predeceſſors, and with what Sagacity they applyed every accidental hint, to the relieving their diſtreſſed fellow Creatures from the Misfortunes they laboured under; we ought to be led to the higheſt Eſteem and Veneration of them, (and ſo much the more moſt certainly) foraſmuch as they were principally our own Country Men, who I can prove not only from ſeveral Perſons, coming from Foreign Parts to be cured of their Diſeaſes here in *England*, but for other Reaſons that they excelled moſt of their Contemporaries in the Divine Art of Healing. Now although

though thoſe Foreign Authorities, I before mentioned, might be looked upon as ſufficient to convince any one, how our Anceſtors blended theſe two Diſeaſes together: Yet ſhall I purſue my deſigned Method, and prove from our own Writers, long before thoſe, that altho' the *Pox* was not only among us, but in diſtant Nations anciently confounded with the Leproſy; yet ſo exact were our Writers in their Obſervations of the Infectious Nature of one Species of that Diſeaſe, and deſcribing the Symptoms, as was ſufficient to lead any Perſon to the diſtinguiſhing between them, ſo as to ſeparate one Diſeaſe from the other. I ſhall therefore firſt enquire into the manner how the Leproſy was ſometimes ſaid to be gotten in thoſe early times, and then examine the Symptoms of the Diſeaſe, that attacked the Patient. *John Gadiſden* a very learned and famous *Engliſh* Phyſician, who flouriſhed about the Year 1340, in an excellent Work of his, he entitles *Roſa Anglica*, ſpeaking *de infectione ex coitu Leproſi, vel Leproſæ*, ſays as follows, *Primo notandum quod ille qui timet de excoriatione & arſura Virgæ poſt coitum ſtatim lavet Virgam cum aqua mixta aceto, vel cum urina propria & nihil mali habebit*; and in another place ſpeaking, *de Ulcere Virgæ*, he ſays, *ſed ſi quis vult membrum ab omni corruptione ſervare, cum a Muliere recedit, quam forte habet ſuſpectam de immunditie, lavet illud cum aqua frigida mixta cum aceto, vel urina propria intra vel extra præputium*, He likewiſe ſpeaking ſtill of the Leproſy recommends a Decoction of Plantain and Roſes in Wine, to be made uſe of by the Woman, immediately after the Venereal Encounter; upon which he tells us ſhe will be ſecure. From hence it is evident ſome of their Leprous Women (as they call'd them) were capable of communicating an infectious Malady to thoſe that had carnal converſation with them which proves, the Pudenda of the Women muſt be diſeaſed, for as much as we are abſolutely aſſured Infections of that Nature only happen when a ſound Part comes to an immediate Contact with a diſeaſed one; for the Symptoms always firſt diſplay themſelves in thoſe Parts through which the Virulency is firſt conveyed. Now in a true Leproſy we never meet with the mention of any diſorder in thoſe Parts, which, if there be not, muſt abſolutely ſecure the Perſon from having that Diſeaſe communicated to him by coition with Leprous Women; but it proves there was a Diſeaſe among them, which was not the Leproſy, although it went by that Name; and that this could be no other than Venereal becauſe it was infectious: for there is no other Diſeaſe that is capable of being communicated this way but the Venereal Diſeaſe, ſeeing the Pudenda are only in that Diſtemper ſo diſeaſed as to become capable of communicating their Contagion. I find the learned *Gilbertus Anglicus* who flouriſhed about the Year 1360, reaſoning concerning the manner how it is poſſible a Man ſhou'd be infected by a Leprous Woman; where if we allow him to call the Malignant Matter, which, is lodged in the Vagina, [*the Womans ſeed*] we ſhall find he accurately deſcribes the very firſt Venereal Infection, by part of the virulent Matters being received into the Urethra; from whence by the Communication of the Veins and Arteries, it is conveyed into the whole Body, after which (ſays he) enſues its total Corruption. Let us now examine the Symptoms of one ſort of their Leproſy, for it muſt be neceſſarily divided into different Species, when another Diſtemper was blended with it, in which we obſerve ſuch a diverſity of appearances; and this I ſhall the rather do in this Place, becauſe it will furniſh us with the next Succeſſion of Symptoms after thoſe already mentioned, as the Venereal Ozænas, the Ulcers of the Throat, the Hoarſneſs, the proof of its being communicable from the Nurſe to the Child, by Hereditary ſucceſſion, *&c.* All which we find to be true in the Venereal Diſeaſe at this Day. Our Country-Man *Bartholomew Glanvile*, who flouriſhed about the Year 1360, in his Book *de Proprietatibus Rerum*, tranſlated by *John Treviſa* Vicar of *Barkley* in 1398 tells us *ſome Leprous Perſons have redde Pymples and Whelkes in the Face, out of whome oftenne runne Blood and Matter: In ſuch the Noſes ſwellen and ben grete, the vertue of ſmellynge faylyth, and the Brethe ſtynkyth ryght fowle.* In another place the ſame Author ſpeaks of *unclene ſpotyd glemy and quyttery, the Noſethrilles ben ſtopyl, the waſen of the Voys is rough, and the voyce is horſe and the Heere falls.* Among the Cauſes of this ſort of Leproſy, he reckons lying in the Sheets after them, eaſing Nature after them; and others which the firſt Writers on the *Pox* looked upon to be capable of communicating that Contagion: alſo, ſays he, *it comyth of fleſhly lyking by a Woman, after that a Leprous Man hathe laye by her; alſo it comyth of Fader and Moder; ann ſo thys Contagyon paſſyth into the Chylde, as it ware by Lawe of Herytage. And alſo when a Chylde is fedde wyth corrupte Mylke of a Leprouse Nouryce.* He adds *by what ever Cauſe it comes, you are not to hope for Cure if it be confyrmyd; but it may be ſomewhat bidde and lett that it diſtroye ſo ſoone.* Thus we ſee how our Author under the Name of one Species of the Leproſy, gives a Summary of the Symptoms of the *Pox*, and the ſeveral ways whereby it is at this time communicated. Now when theſe two Diſeaſes were anciently blended together and paſſed under the Name of the Leproſy only, it muſt be the real Cauſe why that Diſeaſe ſeemed to be ſo rife formerly; for two Diſtempers paſſing under one Name muſt neceſſarily make it more taken notice of and much more frequent; not but that much the greater Number of thoſe who were formerly ſaid to be Leprous were really Venereal, ſeems to be very evident; for ſince that Diſeaſe has been ſeparated from the Leproſy, it has drawn off ſuch vaſt Numbers, that the Leproſy is become as it were a perfect Stranger to us. Thoſe that are acquainted with our Engliſh Hiſtory well know the great Proviſion that was anciently made throughout all *England* for Leprous Perſons, inſomuch that there was ſcarce a conſiderable Town among us but had a Lazar-Houſe for ſuch diſeaſed. In a Regiſter which belonged to one of theſe Houſes, I find there were in *Hen.* the VIIIth's time ſix of them near *London*, (*viz.*) at *Knight*'s *Bridge*, *Hammerſmith*, *Highgate*, *Kingſland*, the *Lock*, and at *Mile-end*, but about 40 Years before I find but four mentioned; and in 1452 in the Will of *Ralph Holland*, Merchant Taylor, regiſtred in the Prerogative Office, mention is made but of three, which, with his Legacies to them, are as follow. *Item lego Leproſis de Lokes, extra Barram Sti Georgii* 20*s*. *Item Lego Leproſis de Hackenay* (which is that at *Kingſland*) 20*s*. *Item lego Leproſis Sti Egidii extra Barram de* Holborn 40*s*. from which its worth while to note, that the *Lock* beyond St. *Georges* Church, and that at *Kingſland*, are at this time applyed to no other uſe than for the entertainment and Cure of ſuch as have the Venereal Malady. Some of our learned Antiquaries have been much concerned to know the Cauſe why the Leproſy ſhou'd be ſo common in thoſe early times, and ſo little known among us now: But I believe the Reaſon will be impoſſible

poſſible to be aſſigned, unleſs we allow according to the Proofs which I have already brought, that the Venereal Diſeaſe was ſo blended with it, as to make up the Number of the diſeaſed. It ſeems to have been the ſame thing with them in *France* as with us: For the Author of the Hiſtory of that *Kingdom*, which was lately publiſhed here in two Volumes in Octavo tells us that the Houſe of the Fathers of the Miſſion of St. *Lazarus*, was formerly an *Hoſpital* for Leprous People, but that Diſeaſe being ceaſed in this laſt Age (ſince the *Pox* has been ſeparated from it) theſe Lazar Houſes have been converted to other Uſes; and it may not be perhaps foreign to my purpoſe to take notice that the Writ *de Leproſo amovendo* contained in the Regiſter of Writs was, (according to *Coke* upon *Littleton*,) to prevent Leprous Perſons aſſociating themſelves with their Neighbours, who appear to be ſo by their Voice and their Sores; and the Putrefaction of their Fleſh; and by the Smell of them. Well then, let us examine what Method was to be taken to prevent this noyſom and filthy Diſtemper, the Leproſy; why truly that which would infallibly prevent their getting the *Pox* after the uſual Method, and that was Caſtration. It is certain that *Eunuchs* are rarely or never troubled with the Leproſy, according to *Monſieur le Preſtre*, a Councellor in the Parliament of *Paris*, who has theſe Words (*a*) *Antipathia vero Elephantiaſis veneno reſiſtit: Hinc Eunuchi & quicunque ſunt mollis, frigidæ & effeminatæ naturæ nunquam aut raro Lepra corripiuntur, & quidem quibus imminet Lepræ periculum de conſilio medicorum ſibi virilia amputare permittitur.* And *Mezeray* ſays, he has read in the Life of *Philip* the *Auguſt*, that ſome Men had ſuch Apprehenſions

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(*a*) Centur. 1 *Cap.* 6. de ſeparatione ex cauſa Luis Venereæ.

prehenſions of the Leproſy, (that ſhameful and naſty Diſtemper) that to preſerve themſelves from it, they made themſelves *Eunuchs*. Now it is highly probable that thoſe Perſons that ſubmitted to ſuch a painful Operation, having before obſerved, that thoſe that gave themſelves up to a free and unreſtrained uſe of Women, fell at length under ſuch unhappy circumſtances; and ſo found the only meaſures to preſerve themſelves from it was to be diſabled for ſuch engagements, which ſufficiently proves this Species of the Leproſy was Infectious; and for the reaſons before aſſigned could be no other than Venereal; for how the true Leproſy ſhould be prevented by ſuch means will be, I believe, impoſſible for any Perſon to determine. There yet remains one very conſiderable Symptom of the Venereal Malady for me to take notice of, becauſe it is looked upon to be the moſt remarkable in that Diſeaſe, which, is the falling of the Noſe; but ſince it has been already proved, that this Diſeaſe when it had arrived to ſuch a pitch as to diſcover it ſelf by thoſe direful Symptoms as are the immediate forerunners of this, was by the Ancients confounded with the Leproſy, and called by that Name, it muſt be among the Symptoms of that Diſeaſe we are the moſt likely to meet with it, if any ſuch thing as the falling of the Noſe was known among them. Now the moſt likely Method of coming to a certain Knowledge of the Infallible Symptoms of the Leproſy of the Ancients in its more confirmed State, is to conſult the Examinations thoſe unhappy Perſons were obliged to undergo, before they were debarred the Converſation of Human Society, and committed to cloſe confinement: But this being a thing ſome Ages ſince laid aſide, no Author that I know of having the particular Hiſtory of it, and ſomewhat of

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being abſolutely neceſſary in this deſign, I ſhall do it as briefly as I can from what Remains I have met with in Records, and other ſcattered Papers. Firſt then, after the Perſons appointed to examine the diſeaſed, had comforted them, by telling them this Diſtemper might prove a Spiritual Advantage; and if they were found to be Leprous, it was to be looked upon as their Purgatory in this World; and although they were denied the World, they were choſen of God: the Perſon was then to ſwear to Anſwer truly to all ſuch Queſtions as they ſhould be asked; but the Examiners were very cautious in their enquiries, leaſt a Perſon that was not really Leprous ſhould be committed, which they looked upon to be an almoſt unpardonable Crime: They conſidered the Signs as Univocal, which properly belonged to that Diſeaſe, or Equivocal, which might belong to another, and did not upon the appearance of one or two Signs, determine the Perſon to be a *Lazar*; and this I find to be the Caſe of the Wife of *John Nightingale* Eſq; of *Burntwood* in *Eſſex*, who in the Reign of *Edw.* the IVth *An.* 1468 being reported to be a *Lazare*, and that ſhe did converſe and communicate with Perſons in publick and private Places, and not (according to cuſtom) retire her ſelf, but refuſed ſo to do, was accordingly examined by *William Hattecliff*, *Roger Marcall*, and *Dominicus de Serego* the Kings Phyſicians; but they upon ſtrict enquiry adjudged her not to be Leprous, by reaſon the Appearances of the Diſeaſe were not ſufficient: Some of the Queſtions put to the Leprous Perſons (as they called them) which will more fully confirm what I have before advanced, I ſhall now give as I tranſcribed them from an Ancient Book of Surgery *yf there were any of his lygnage that he knew to be* Lazares *and eſpecially their Faders*

and

and Moders; for by any other of their Kynred they ought not to be Lazares, *then ought ye to enquire yf he hath had the Company of any lepreſs Woman, and yf any* Lazare *had medled with her afore him; and lately becauſe of the infect matter and contagyous filth, that ſhe had received of hym. Alſo his noſtrills be wyde outward, narrow within and gnawn. Alſo yf his lips and Gummes are foul ſtynking and coroded, Alſo yf his voice be horse, and as he ſpeaketh in the noſe.* Now the Signs which are here mentioned, were looked upon to be Univocal: And theſe were they that made the Examiners principally determine the Perſons to be Leprous; but what Determinations any one would immediately give from ſuch Symptoms now, no Perſon is ſurely ignorant of. But even theſe certain appearances would not always ſatisfy ſome Perſons, if we may believe *Fælix Platenus* in his Medicinal and Chirurgical Obſervations, *Lib.* 3. who tells us ſome did not look upon them to be ſo, till they had an horrible aſpect, were hoarſe, and Noſes fell. Likewiſe in the *Examen Leproſorum* printed in the *De Chirurgia Scriptores Optimi*, the Author ſpeaking of the Signs of the Leproſy relating to the Noſe begins thus, *Si nares exterius ſecundum exteriorem partem ingroſſentur, & interius conſtringantur, & coarctentur. Secundo ſi appareat cartilaginis in medio corroſio, et caſus ejus ſignificat Lepram incurabilem.* And the before mentioned *John Gadiſden* in his Chapter *de Lepra* ſays as follows, *Signa confirmationis etiam incurabiliter ſunt corroſio cartilaginis quæ eſt inter foramina & caſus ejuſdem.* Thus Sir have I proved we had a Diſtemper among us ſome hundreds of Years before the Venereal Diſeaſe is ſaid to have been known in *Europe*, which was called the Burning; that this Burning was Infectious, and that it was the firſt Degree of the Venereal Diſeaſe; that this being common at that time, from their Method of Treatment; the *Pox* muſt be unavoidable: That it had exactly the ſame appearanees it has now, although they were generally called by different Names, that the Ancients confounded it with the Leproſy; that the vaſt Numbers of Leprous Perſons among us, before the Venereal Diſeaſe was ſeparated from it; and the ſmall Number we obſerve at this Time, is a flagrant Proof of the former; that in deſcribing the Symptoms of the Leproſy, they give us thoſe of the Venereal Malady; and, by mentioning how it is communicated, they deſcribe the ways by which the *Pox* is gotten at this Day; that ſuch Remedies were by them recommended to prevent the firſt attack of the Leproſy, as are at this Time in Uſe to prevent the firſt Symptoms of the *Pox*; and that the falling of the Noſe, which has been look'd upon to be the moſt remarkable Symptom of the Venereal Diſeaſe, was commonly obſerved in what they called the Leproſy in former Ages.

I am, Sir,

Yours &c.

William Beckett.

III. *Part*

III. *A Deſcription of the great* Meteor *which was on the 6th of* March, 171$\frac{5}{6}$. *ſent in a Letter from the late Reverend Mr.* Roger Cotes, *Plumian Profeſſor at* Cambridge, *to* Robert Dannye, D. D. *Rector of* Spofferth *in* Yorkſhire.

THE appearance of the Meteor was very nearly the ſame with us here at *Cambridge* as with you, excepting that the triangular Streams of Light were not ſo permanent as you ſeem to deſcribe them, and the Point to which they all converg'd was diſtant from the Zenith about 20 Degrees, its Azimuth lying between the South and the Eaſt at about 10 Degrees from the South, towards which Point of the Compaſs the Wind tended. The poſition of this point of Convergence may be more accurately determin'd, if there be occaſion: For at a quarter after Seven, when the appearance to us was in its greateſt perfection, it lay nearly in the middle between the two bright Stars, in the heads of *Caſtor* and *Pollux*. I am told that ſome Streams were ſeen to ſhoot forth immediately after Sun ſet, and that they did not perfectly ceaſe till about 3 or 4 in the morning.

It was after Seven before I had notice of this uncommon ſight. At firſt I ſaw only two or three of the triangular Streams towards the North and North-Weſt: Theſe were not of long Duration, but were ſucceeded by others which appear'd and vaniſhed again by turns, ariſing from, and aſcending up to Places in the Heavens, of very different Altitudes above the *Horizon*. From the time I began to view them, they continued to aſcend more and more copiouſly, being propagated ſtill further and further from the North towards the Weſt and Eaſt, and directed always to the Heads of *Gemini*, till at length when they ſeem'd almoſt to meet at the Point of Convergence, they began to aſcend up towards it from the Southern Parts alſo and all around it; inſomuch that at a Quarter after Seven, we had a perfect Canopy of Rays over us: The bottom of this Canopy did no where reach down to the *Horizon*; for near the North, where it deſcended the moſt, its Altitude was about 10 or 15 Degrees; and near the South where it deſcended the leaſt, its altitude was about 40 Degrees. It remain'd in this State about 2 Minutes, during which time, we ſaw ſeveral Colours, ſome fainter and more permanent, others brighter, but quickly vaniſhing. Thus in the Weſt I obſerv'd the Rays to be ting'd for ſome conſiderable time with an obſcure and heavy Red; and in one of the brighteſt Streams at another time, there ſuddenly broke out a very vivid Red which was inſtantly and gradually ſucceeded by the other Priſmatick Colours, all vaniſhing in about a Second of Time. Theſe Colours affected the Senſe ſo ſtrongly, that I thought them to be more intenſe than thoſe of the brighteſt Rainbow I had ever ſeen. A ſmall time before the Appearance loſt it's perfection, we were ſurpriz'd to obſerve a Shaking and Trembling of the Streams chiefly in their upper Parts, during which their Convergence was confounded, and the whole Heaven ſeem'd to be in a Convulſion. At the ſame time I cou'd perceive Waves of light towards the North, which moved upwards, and in their motion croſs'd the ſtreams, lying parallel to the Horizon. Theſe Waves were different from thoſe broad ones, which you mention, and which I alſo took notice of: Their breadth ſeem'd to be about a Degree, their length about 90 Degrees; and I can compare them to nothing better than to thoſe ſlender Waves upon the ſurface of ſtagnant Water, which are made by caſting in a ſmall Stone.

About ſeven or eight Years ago, I happen'd to ſee a Meteor which it will be of uſe to deſcribe to you. Along the Horizon in the North, there lay a white and luminous, and ſeemingly denſe matter in the form of a Cloud repreſented by a b c d; the length of it, a b, was about 10 or 15 Degrees. From this there aroſe, directly upwards, pointed ſtreams of the like luminous and white matter, which yet did not appear in any part of it to be ſo denſe as the former; and grew gradually more and more rare in its upper Parts ſo as to vaniſh almoſt inſenſibly at the Points. There was ſome little difference in the height of theſe ſtreams, but they generally aſcended up to about 4 degrees above the Horizon. They were very numerous and contiguous to each other, and ſeem'd to be compos'd of very ſlender parallel filaments or Rays. This was the common appearance, and the only remarkable Thing which I farther obſerved was, that ſometimes a Fire or Flame would break out in the Cloud, a b c d, and move along it in a direction parallel to the Horizon: And during this motion, a pointed Stream directly over the Fire ſeem'd to run along with it, and to paſs by the other more fix'd Streams to which it always kept it ſelf parallel.

I am perſuaded that the late Appearance was of the ſame Kind with this, which, I have now been deſcribing. For let A B, repreſent the plane of the Horizon, C the place of the Spectator. E F, a fund of Vapours or Exhalations at a conſiderable height above us, diffus'd every way into a large and ſpacious Plane, parallel to the Horizon. This Fund of mixt matter by Fermentation will emit Streams from it ſelf, ſuch as EG, F H, *&c.* which, if the Wind be perfectly ſtill, will aſcend perpendicularly upwards; if it be boiſterous and irregular, they will be blended and confounded together; but if it be very gentle and uniform, as it was at the time of our Appearance, they will be inclined towards the point of the Horizon, which is oppoſite to that from which the Wind blows. Now if A D B repreſent the concave of the Heavens and a Line, C D, be drawn parallel to the Columns E G, F G, *&c.* 'tis certain by the Rules of Perſpective, that theſe Columns will appear upon that concave to converge all around towards the Point D: Thus the Column, E G, will ſeem to ariſe from the Point e, to aſcend up to g, and to take up the Space e g; and in like manner the Arch f h will be the Projection of the Column F H. From hence it is evident that the reaſon why the triangular Streams aſcended at firſt only from the Northern Parts of the Heavens was this: The Fund of Matter, E F, was not yet arriv'd by its motion to the Line C D. After it had paſs'd that Line, it is plain they muſt appear to aſcend from all Quarters. A great number of Columns being therefore diſpos'd to emit Light, at the ſame time, caus'd that perfect Canopy, which, I deſcribed above. The reaſon why that Canopy deſcended lower in the North, than in the South, was this: The ſhining Columns which had not yet paſs'd the Line C D, were more numerous and more remote from it than thoſe which had paſs'd it; for if the Point E, be farther diſtant from C D than the Point F, the Arch A e, muſt needs be leſs than the Arch B f. An irregular guſt of Wind blowing upon and ſhaking the Columns, was (I ſuppoſe) the Cauſe of that trembling, which appear'd in the triangular Streams, and the Cauſe alſo which deſtroy'd that fine appearance of the Canopy. The ſlender circular waves ſeen at the ſame time might alſo be explain'd from the ſame Cauſe. I need not detain you any longer by endeavouring to make out ſome other particulars of this unuſual Appearance: I fear I have been already too tedious. However I will not omit to mention a very eaſy Contrivance by which the Thing may be tolerably well repreſented to view. Take a Hoop and round about it faſten ſeveral ſtreight Sticks parallel to each other, but all inclin'd to the plane of the Hoop, hold this plane parallel to the Horizon, and in that poſture move it with Sticks over a Candle, the ſhadows of the Sticks upon the Ceiling of your Room, will converge to a point not directly over the Candle, (as they would have done, had the Sticks been perpendicular to the plane of the Hoop) but to the Point in which a Line drawn from the Candle parallel to the Sticks, ſhall interſect the plane of the Ceiling.

IV. *A Letter of Dr.* John Quincy, *to the late learned Mr.* Sam. Moreland, F.R.S. *concerning the Operation of Medicines.*

WHAT I had an opportunity ſome time ſince of ſaying in Converſation concerning the Operation of Medicines, and particularly of Purges, I here beg leave to ſend you for a Re-Examination, becauſe I ſhall be glad to have my thoughts confirm'd by your Approbation, or be advertis'd of any miſtakes which ſelf Partiality may lead me into, before they are too far drawn into Conſequences.

With regard to this affair I ſuppoſe,

1. That all thoſe parts of an animal Body, which are vaſcular, or through which any Fluid paſſeth, from the Inteſtines to the minuteſt Fibre, are the ſeat of Medicine's Operation.

2. That this whole courſe of Circulation, or Animal Motion is naturally diſtinguiſhed into three different Stages, by the different capacities of the Veſſels and motions of their Contents, each having its proper Out-let, and that theſe are the ſeat of the three Concoctions, ſo often mentioned by Phyſical Writers; the firſt in the Stomach and Bowels, having the Anus for its Emunctory; the ſecond, all within the Blood's motion, ſo far as it retains its Colour, having the Kidneys; and the third, all beyond that Circuit, having the Skin for an excretory Organ.

3. That every Medicine which cauſeth Evacuation is a Purge.

4. That every Purge operates as a Diſſolvent, by fuſing the Juices, and increaſing the quantity fit for Ex-

pulſion; or as a Stimulus, by accelerating their Motions, ſo as to bring the matter fit for Expulſion, oftener to the ſecretory Out let; or both.

Theſe Aſſumptions, which, I expect, will not be diſputed, are premiſed only for the better proof of this grand Propoſition.

That a change in the Bulks, Figures, and Motions of the component particles of a purging Medicine, will change the ſeat of its Operation, and fit it for exertion in larger or leſſer Veſſels as thoſe mechanical Affections are intended, or remitted.

For Illuſtration hereof, it may be convenient to attend the management of common Practice, in making a Purge operate more, or leſs, than it otherwiſe would do.

Subſtances which are groſs and heavy, as thoſe conſiſting chiefly of Saline and earthly Particles, ſuch as Tartar, Manna, and the like, if they are reduced ſmaller by Triture, or repeated Solutions, operate more gently, but if acuated by Acids, or any management which expoſeth their Angles more ſenſibly to the Membranes, they are rougher, and take Place ſooner.

Reſinous Medicines, as Scammony, Gamboge, Jallap, and moſt of vegetable Production are more violent, and operate ſooner, when they are more tenacious, and adheſive, as in their Extracts, but gentler, when divided by hard brittle Subſtances, ſuch as ſalt of Tartar, Sugar, *&c.*

Medicines which have in their Compoſition, Sulphur and Salt, are more or leſs rough, and ſpeedy in their Operation, in proportion to their greater or leſſer participation of the Saline Ingredient, and the aſperity of its Angles; of this kind are moſt Minerals, and their Preparations, and it may be ſufficient to inſtance in the management of Antimony, and

Mercury; The firſt of theſe is by chymical Analyſis known to be a compoſition of a ſubtil Sulphur and Salt, and the more the Saline Part is let looſe by Preparation, and opening the Sulphur, as it is commonly term'd, with the more vehemence will this operate, and the ſooner, whereas in its leſſer Preparations when the Salts are cloſely wrap'd up in their native Sulphur they will hardly work at all till they are got into the fartheſt ſtages of Circulation. Mercury *per ſe* is little known as a Medicine, and its firſt Preparation which makes it into Sublimate, ſo loads it with Saline Spicula, that it amounts even to a Poyſon; but the more thoſe Spicula are broke by Triture, Sublimation, *&c.* the milder doth it operate, and if to the comminution of its Points be added a Sulphur ſubtil enough to join it, it may be reduced to ſo mild a Medicine, as not to be felt, but in the laſt ſtage of Operation.

This ſhort View may be ſufficient to ſhew, that it is Exceſs of Aſperity, and Motion in a Medicine, that will not ſuffer it to paſs the Stomach, without irritating it into ſuch Convulſions, as will throw it up again by Vomit; that a farther Comminution, and ſmoothing its Figure, will admit it into the Bowels, and make it operate by Stool; that a yet farther remiſſion of its Properties will carry it into the Blood, and allow it there to promote evacuation by Urine; and that a yet farther Comminution will paſs it into the minuteſt Canals, where by the ſame Properties, only in a lower Degree, it will increaſe Perſpiration, or cauſe Sweat: So that the ſubtiler Medicines operate in the Capillaries, and ſmalleſt Fibres by the ſame Mechaniſm, that more groſs do in the common ſtream of the Blood, when they go off by Urine, or the groſſeſt of all do in the greater Paſſages, when they promote Stool.

Hence the Skill of Preparing, and Adminiſtring of Medicine conſiſts in proportioning its manifeſt, and known Properties to the Capacity, and Circumſtances of the Part it is to operate in, and to intend, or remit its mechanical Affections, as it is ſooner, or later to take place in the greater or leſſer Veſſels.

Of the firſt Claſs there are few to be reduced ſmall enough to go beyond the greater Paſſages, and none of them are worth the Pains they require, to fit them farther than for Diureticks: beſides, their natural fitneſs to attract, and join with the ſerous Part of the Blood, whenever they get into that Stage of Motion, runs them off by the Kidneys, before they can undergo Comminution enough to get farther; but if by frequent Repetitions of ſuch Medicines, and uncommon Laxity of the Paſſages, any thing is paſſed into the Habit, their groſſneſs fouls the delicate ſtrainers, which are left for their Expulſion; and they lodge upon the Glands, and Capillaries in ſuch manner, as induceth Intermittents, which is obſervable in many Perſons, after the uſe of Cream of Tartar, the common Cathartick Salts, and the purging Waters, eſpecially at the latter end of the Summer, when the heat of the preceding Seaſon hath debilitated the Solids, and left them under too great Relaxation.

Among the reſinous Purges, there are many very powerful ones, but where their Operation is deſired in the Viſcera, Blood and remoter Parts, they muſt be extreamly divided, and this we find Spirituous Menſtruums will do, by taking up the moſt ſubtil Parts only, and carrying them into very ſmall Paſſages, where their Operation is chiefly by Fuſion, becauſe the ſoftneſs of ſuch Subſtances cannot enable them in hardly any Degree to act as Stimuli, farther at leaſt than ordinary Detergents. And thus we find Aloes, the chief of this Tribe, to go farther into the Habit, and be longer e'er it operates, when managed in a ſpirituous Menſtruum, as in the *Tinctura Sacra*, the *Rad. Turpethi*; And *Colocynth* likewiſe, with all of the vegetable Kind, that will yield to a ſpirituous Liquor, are to be carried by that means into the fartheſt ſcenes of Animal Action, and there prove efficacious Medicines in Caſes, that with other management, they would never be able to reach: And on this Foot it undoubtedly muſt have been, that we frequently meet with, in practical Writers, many of this ſort mention'd, as Alterants; the Colocynth particularly by *Helmont*, for all Medicines which operated in the fartheſt Paſſages they commonly included under that general Appellation.

But the moſt efficacious Purges, and thoſe which require the moſt Skill are from the mineral Kingdom; theſe abound in ſolidity beyond any other Materials, and therefore whereſoever they are brought into Action, excel in quantity of Impulſe: Many of theſe therefore want not only the utmoſt Comminution to carry them into the farther Scenes of Operation, but alſo ſome reſtraint to their Aſperities, and Motions, to fit them for many Intentions. Thus Sublimate is not only to be much ſweeten'd, that is, ſmooth'd in its Points, to make it a ſafe Purge in the larger Veſſels; but if it is intended to go farther than the Blood, and thoſe Glands, which in that Circuit, they are moſt apt to be lodged upon, when it Salivates, it muſt be rendred not only very fine, but covered with ſuch Subſtances, as weaken its Points, and make it ſtrain into the laſt Subdiviſions of the Conſtitution. To this purpoſe, the common

Practice wiſely contrives in Diſtempers which lie fartheſt off, according to the Courſe of Circulation, to wrap up the Baſis of this Medicine, in Sulphurs, and ſuch like Subſtances, as follow it into its laſt Diviſion, without giving it any Aſperities to make it act as a Stimulus. Thus for all cutaneous Foulneſſes, and habitual Taints, the Cinnabar, the Æthiops, and all of that Sortment are in readineſs; and that ordinary Sulphurs will cover and deaden the efficacies of Mercurial Preparations, ſo that they ſhall not operate, but in ſuch Parts only and in certain Circumſtances, is demonſtrable in ordinary Salivations, which are to be lower'd at pleaſure by Sulphureous Medicines.

Medicines from ſuch Minerals where a Salt, and Sulphur are united by Nature, as they are in ſome Mercurials by Art, as in Antimony, the native Cinnabar, Steel, *&c.* are manageable only upon the ſame Principles, and the more they are deſigned to be carried into the Habit, the more are they to be reſtrained by their natural, or adventitious Sulphurs: Steel when opened by, and joined with, the points of acid Liquors, operates ſooner, and will ſometimes prove even Emetick, but when it is covered with an additional Sulphur, it will go farther, and anſwer intentions much more remote, as is manifeſt in the common Preparations of Steel, with Tartar, or Vinegar, and with Sulphur.

This way of thinking on theſe Occaſions, ſeems to me alſo the more juſt, from conſidering the Texture of thoſe Subſtances, which by a natural Preparation are fitted for Operation in the minuteſt part of an Animal Body, ſuch as thoſe of the Aromatick Kind, all which more or leſs, according to their greater, or leſſer Degree of Subtilty, and Smoothneſs promote a Diaphoreſis: They conſiſt of exquiſitely fine Salts, covered with a moſt Subtil Sulphur, as is demonſtrable by Chymical Analyſis; and the common Sal Volatile Oleoſum is an admirable Contrivance upon the ſame Foundation, where a very Volatile Animal Salt is covered with a moſt exalted vegetable Oil, whereby it is ſuited to paſs into the minuteſt Fibres, and make as it were, a Part of the Animal Spirits themſelves.

And here it may not be amiſs to obſerve, that all Animal Salts are very Volatile or eaſily render'd ſo, but when bare, and naked, juſt as the Fire draws them out, with a mixture alſo of its own Particles in their Compoſition, they are too pungent to be felt without painful Senſations, but when ſoften'd with a fine Portion of an oppoſite Texture, which is ſmooth and yielding, they become moſt efficacious, and ſafe Sudorificks.

On theſe Conſiderations it likewiſe ceaſeth to be a wonder why the ſubtil Salts of Cantharides are more ſenſibly injurious to the Bladder, than any other Parts, and why Camphire prevents thoſe injuries; for the exquiſite ſmallneſs of thoſe Spicula makes them imperceptible, but in the moſt minute Canals, into which the Fibres compoſing the Membranes of the Bladder are known to be divided; and Camphire blunts their irritations, becauſe its exquiſite Subtilty enables it to follow them into thoſe Meanders, and ſheath their Aſperities.

To this purpoſe it is very remarkable, what many (as I have been informed) now commonly practice in guarding even Mercurials againſt their ſtimulating Properties, and ſending them into the fineſt Paſſages to operate by Fuſion, and the bare Force of Impulſe: for not only Calomel and the Mercurius Dulcis may be reſtrained from manifeſt Operation in the wider

Paſſages, and the Glands about the Mouth, but even the mineral Turbith, which of it ſelf in a ſmall Doſe, will operate powerfully by Vomit and Stool, when mixed with Camphire, will not be ſo much felt in thoſe reſpects, but go into the fartheſt Circuit of Motion, and promote the cutaneous Diſcharge in a more efficacious manner, than any Medicine of leſs ſpecifick Gravity. But in this management the Camphire is to be mixed but very little before taking, otherwiſe it hath not this effect, which appears to be from its great Volatility, which makes it in a great meaſure exhale, while it ſtands mixed in a Medicine.

I am ſenſible of many good purpoſes in Practice, that this Theory is applicable to, but becauſe the Limits I have ſet my ſelf herein will not ſuffer me to inlarge, I ſhall content my ſelf with this only Inſtance of Camphire, which is enough to ſuggeſt to thoſe who turn their thoughts this way, in what Caſes, that, and ſuch Subſtances of like Subtilty, and Texture may be uſed with ſucceſs: For the Seat and Cauſes of many Chronick Diſtempers lie moſt remote in the Courſe of Circulation, and the reaſon why they elude the ordinary means of Cure, it is to be feared, is owing to the want of ſufficient Attention to that particular management of efficacious Remedies, which is neceſſary to carry their Operations ſo far; An ordinary Judgement knows how to intend, or remit the Efficacies of Medicines by acuating their Points, and Quantity of Impulſe; or ſoftening, and weakening them with Broths, and the groſs-expreſs'd Oils of Almonds, or Linſeeds in the firſt and larger Paſſages; but an active Medicine, or a diſtemper'd irritating Salt in the minuteſt Capillaries and Fibres is not to be managed by ſuch coarſe Inſtruments.

If, upon the beſt examination, theſe thoughts be found to hold good, which I am much perſuaded they in great meaſure will; I promiſe my ſelf an opportunity of drawing ſome farther practical Remarks, and ſhall venture to communicate them to you, with the ſame freedom, I now do.

V. *An Account of two extraordinary caſes in Surgery: Communicated by Dr.* Steigerthall, F. R. S.

Ebſtorff, *May the 15th*, 1720.

Preſent Dr. Nottellman, *Mr.* Henzel *an Officer of the Cuſtom, and Mr.* Niemeyer, *Auditor.*

JOhn *Henry Oizmann*, aged 31 Years and born at *Barum*, was fifteen Years of Age, when the following Misfortune befel him.

He felt a Spaſmus or Cramp in his left Hip, and the inferiour Part of his Leg; as this pain ſeized him pretty often, he conſulted Mr. *Rack* a Surgeon at *Ulzen*, who applied ſeveral Plaiſters to the place where the pain was, but without any relief to the Patient. After all thoſe fruitleſs efforts, the Surgeon, to ſee whether *Oizmann* had ſtill a feeling in his Leg (which to outward appearance was become very brown) made about 37 Inciſions over the whole Leg of which the Patient was not at all ſenſible, except at ſuch times when the Inſtrument happened to grate upon the Bone, the Perioſteum being as yet ſound and not infected by the Diſeaſe of the Fleſh. The Leg however did daily grow blacker, and the pain continued both in the Perioſteum and in all the Bodies of the ſuperior and inferior Part of the Leg. At laſt a black Circle was ſeen round about the Muſcles of the Hip as an indication of an approaching Putrefaction. This Circle appeared ſo viſibly, as if it had been cut off with a Knife from the other Part. It has ever ſince ſpread it ſelf, and come to ſuch a head, that without any other help and cure the Fleſh began gradually to rot away from the Bones; and at laſt quite fall away from the ſuperior Part of the Leg which has preſerved it's ſoundneſs. After this nothing was ſeen but the bare Tendons or Sinews hanging down like ſo many Strings or Cords. There remained alſo one piece of the inferior Muſcles of the Hip faſtned to the ſuperior Part. At laſt the Tendons being grown dry, conſumed away, and after all, the Leg it ſelf, I mean the *Os Femoris*, did wholly drop off in ſuch a manner, that there remained about four Inches betwixt the Bones and the Fleſh looſely hanging down from them. The Fleſh is at laſt grown up to the Bone, and without the help of any man has faſtned it ſelf to them. And in this ſound Part the Patient feels a great Pain, when ever the Weather proves tempeſtuous It is remarkable that at the ſame time he perceives alſo a ſwelling in *Tarſa pedis Dextri*, the Matter whereof diſcharged it ſelf through the Toes, and is of ſo corroſive a Nature, that it had conſumed all the Toes but the little one. The Surgeon has at laſt healed up this Wound; but after all there is as yet but little feeling or warmth in the Foot.

This Man after this great accident is married now about ſeven Years to a Woman, whoſe bodily Conſtitution is almoſt as remarkable. She is now in the 41ſt Year of her Age. In her younger Years ſhe had the misfortune to be goared by a wild Boar under

der the ſhort Ribs of the left ſide. Of this Wound ſhe then received, ſhe has ſtill a Fiſtula to this day, what Food ſhe eats diſchargeth it ſelf half concocted through this opening, and ſhe is obliged to clean this Wound often for that purpoſe, however ſhe has notwithſtanding this, her daily Evacuation *per Anum*: It is remarkable, that the Victuals thus diſcharged, by the Wound aforeſaid, are ſtill diſtinctly known what kind they have been of.

All this is atteſted by *Olzmann* mentioned above. *In fidem Subſcripſit*,

J. Niemeyer.

VI. *An Account of an Experiment made on* Thurſday *the laſt Day of* June, 1720. *before the* R. Society, *to ſhew by a new proof, that Bodies of the ſame Bulk do not contain equal quantities of Matter, and therefore that there is an interſpers'd Vacuum. By* J. T. Deſaguliers.

I Took 3 Pound of *Mercury*, which by meaſure fill'd three times a ſmall glaſs Jar exactly full, and pour'd it into a thin Florence Flask: then having pour'd the ſame quantity of Water (that is, three of the ſame Jars full) into another ſuch Flask, I ſet both the Flasks in a Pail, and pour'd boiling Water about them, keeping the Flask that had the Water down by force, that it might be as low in the hot Water as the *Mercury*. After the Fluids in the Flasks had receiv'd a ſufficient degree of heat from the Water, which was round the Flasks, for the ſpace of five Minutes, I took the Flasks out of the hot Water, and putting that which held the Water into a Cylindrick Veſſel that had three Pints of cold Water in it, I did at the ſame time plunge the Flask with *Mercury* into another Cylindrick Veſſel containing alſo three Pints of cold Water, and obſerv'd which of the cold Waters was moſt heated in the following manner.

A little Thermometer being held in the firſt Veſſel of cold Water ſo as to have its Ball cover'd with the Water, upon the putting in the Flask of warm Water, the Spirit roſe 2 degrees; then putting the Thermometer into the Water where the Flask that had the *Mercury* was, the Spirit roſe three degrees higher. The Thermometer being again put into the firſt Veſſel fell 4 degrees, and afterwards again into the laſt it roſe almoſt 3 degrees.

This ſhews that more heat is communicated by warm *Mercury*, than by an equal Bulk of Water equally warm'd; and therefore that there is more matter in the *Mercury*; but how much more matter there is in the *Mercury* is not determin'd by this Experiment alone.

N. B. The warm *Mercury* and the warm Water were not pour'd into the cold; but only communicated their heat thro' the Flasks.

An Account of a Book, intituled, Conghietture del Dottor Pietre Anton. Michelotti, Filoſofo, e Medico d' Arco, ſopra la Natura, Cagione e Rimedi dell' infermità regnanti ne' Animali Bovini di molte Città, *&c.* Nell' Autunno del' Anno cadente, 1711. *In* Venezia, 1712.

THE Learned Author of this Diſcourſe happening to ſpend ſome time in the Country about the Month of *October*, in the Year 1711, took that opportunity of making a particular Enquiry into the circumſtances of the Mortality, that then reign'd among the Black Cattle in the *Venetian* Territories. The reſult of which he gives in this relation, having been an Eye-Witneſs to the greateſt part of the Facts herein contain'd, and having receiv'd the reſt upon the place from Perſons of Integrity and Credit.

Almoſt all the ſick Cattle refuſed every ſort of Food and Drink, they hung their Heads, had ſhiverings in their Skin, and in their Limbs, they breath'd with difficulty, and their Expiration in particular was attended with a ſort of rattling noiſe, they were ſo feeble, that they could ſcarcely go or ſtand upon their Legs. Some few of them eat a little, and drank very much; others had Fluxes of Excrements variouſly colour'd of a very offenſive ſmell, and frequently tinged with Blood: Many of them had their Heads, and their Bellies ſwell'd in ſuch a manner, that, upon clapping them with the Hand on their Paunches

ches, or along the Vertebræ of the Loins, they ſounded like a dry Bladder when full blown. In ſome the Urine was very turbid, in others of a bright flame Colour. In comparing the Pulſes of the ſound Cattle with thoſe of the diſeas'd, he found the latter to be quicker and weaker. There was but little heat perceivable by the touch in any of them, their Tongues were ſoft and moiſt, but their Breath was exceedingly offenſive. Beſides theſe particulars he was inform'd by thoſe, who attended the ſick Cattle, and by other Perſons worthy of Credit, that in ſome of theſe Beaſts they had obſerved crude Tumors in ſeveral parts of the Body, as likewiſe watery Puſtules and diſorderly motions of the Head, with dry, black, and fiſſur'd Tongues; that in others of them they met with Tumours, that came to Maturation, putrid Matter iſſuing from the Mouth and Noſtrils, Worms in the Fæces, and in the Eyes, bloody ſweats, and the falling off of the Hair.

In comparing the Fleſh of the Cattle dead of this Diſtemper, with that of others kill'd for the Market, he found the Muſcles in the former lying immediately under the Skin to be ſomething livid. Having opened the three Cavities of the Body, he applied himſelf with the utmoſt diligence to examine the Brain with it's Membranes; the Trachæa, Oeſophagus, Lungs, Heart with its Auricles, the Vena Cava, Aorta, and Diaphragm; the Liver, Spleen and other parts of the lower Venter. In all which there was no diſcernable difference, either as to figure, ſize, contents, ſituation, or connexion, with the neighbouring Parts, from what was obſerv'd in ſound Cattle kill'd by the Butcher, except the particulars hereafter mention'd. The Blood found in the Ventricles of the Heart, in the Pulmonary Veſſels, in the Aorta and Cava,

Cava; though ſtill warm, was conſiderably blackiſh, and near a Coagulation. In opening the upper and middle Cavity, the ſcent was offenſive, but tolerable enough, whereas the Stink, that proceeded from the lower Belly, was not to be endur'd without prejudice. In ſome few Carcaſſes the Viſcera differ'd from their natural State, with regard to their ſize, their conſiſtence, their contents, colour and ſmell. In many of them the Paunch was found very much contracted and dry'd, with a hard Subſtance contain'd in it. In others the Lungs were ſwell'd and livid, the Liver tumified, and the Brain watery and putrid.

Upon obſerving the aboveſaid ſtate of the Blood in the Cattle Dead of this Diſtemper, he was deſirous to ſee what condition it was in, while the ſick Beaſts were yet living With which deſign having order'd ſeveral of them to be blooded, he found the Blood not to iſſue out of the Veſſels in a continued Stream, as uſual, but with a broken and interrupted Flux, one Part of the Blood not immediately ſucceeding another. Having caus'd the Blood to be receiv'd in proper Veſſels, and ſuffer'd it to ſtand for ſome time, he found it intirely coagulated, without any Separation of the Serum, and attached to the ſides of the Veſſels, with a reticular Pellicle upon the Surface expoſed to the Air. All the Cattle which were blooded, being Eighteen in Number, died in a few days after the bleeding, one only excepted, in which the Vein was open'd upon its being firſt taken ill.

Having enumerated all the Symptoms of the Diſtemper, the Author concludes from the whole, that the Sickneſs among the Cattle was a Malignant Peſtilential Fever, killing almoſt all thoſe that were infected with it.

The immediate cauſe of this he takes to be a præternatural thickneſs of the Blood occaſion'd by a beginning Coagulation of thoſe parts of it, which conſtitute the Craſſamentum, whereby the Globules of the Blood, and the particles of the Serum were impriſon'd in à ſort of Reticulum form'd by the Union of the Fibres of the Blood.

The occaſional Cauſe of this Sickneſs he deduces from the cold and wetneſs of the Seaſon, which reign'd all the preceeding Year from *October* 1710, to *November* 1711. Which Obſervation is worthy of remark, ſince the Seaſon preceeding the Mortality among the Cattle here in *England* was remarkably dry, and yet the Symptoms of the Diſtemper agreed with thoſe obſerv'd in *Italy*, as may appear from the Account given by the Learned Mr. *Bates*, Surgeon to his Majeſty's Houſhold in *Philoſophical Tranſactions*, *No.* 358

For the particular manner in which this learned Gentleman endeavours to account Mechanically for the thickneſs of the Blood in theſe Animals, from the condition of the Seaſon, and from that thickneſs of the Blood to deduce all the particular Symptoms of the Diſtemper, as likewiſe for his Conjectures concerning thoſe Medicines, which might have been ſerviceable to the ſick Cattle; he not having made tryal of any, we muſt refer the Curious to the Treatiſe it ſelf.

FINIS.

ERRATA.

No. 363. *Fig.* 8. A B, which by miſtake of the Graver, is ſhaded and made to repreſent a Hollow Cone, ought to be only two lines meeting in an Angle, to repreſent a perpendicular Section thro' two inclined Planes. *No.* 364. *p.* 24. *l.* 4. for *Biquadrate* read *ſquare*.

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Numb. 366.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *September*, *October*, *November* and *December*, 1720.

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I. *Hiſtoria portionis Inteſtini Coli propendentis ex Vulnere per* 14 *annos. Communicante Abrahamo Vater, Phil. & Med. Doct. Prof. Public. Ord. Anat. & Botan. Subſtituto Wittembergæ, Natur. Curioſ. Coll.*

VIR quidam *Georgius Deppe* Halberſtadienſis annorum xxxiv. caſtra ſecutus, anno MDCCVI. in prælio prope *Ramellies* in Belgio commiſſo, ab inflicto in ſiniſtro hypochondrio vulnere, per hos xiv annos magnam portionem inteſtini coli, ictu hoc ſecti atque inverſi, ſpithamæ magnitudine e corpore prominens gerit.

Inteſtinum hoc, per vulnus in illud ſimul penetrans, egreſſum, interiorem ſuperficiem ad exteriora vertit, & ita in medio cohærens duas portiones efformat, quarum altera verſus ſuperiora extenſa orificium monſtrat, quod ad inteſtina tenuia ſpectat, & fæces alvinas educit; altera vero portio ad inferiora propendens orificium ad rectum apertum habet, ut Clyſterem injectum per anum reddat. In interiori ſuperficie, nunc ad extra vergente, glandulæ apparent copioſiſſimæ, albi ac cinerei coloris, verrucarum inſtar eminentes, & jucundiſſimum ſpectaculum exhibentes, quæ inclementius tactæ ſanguinem fundunt. Inteſtinum ita prominens nunquam totum regreditur intra abdomen, ventriculo tamen vacuo quodammodo regreditur, ſed ventriculo pleno cibo aſſumpto longius protruditur, imprimis ſpiritu retento. Utitur vir ille aqua gelidiſſima, imo glacie ac nive hyeme permiſta, ad abſtergendas ſordes, ſine ullo incommodi ſenſu; fert etiam inteſtinum aerem frigidiſſimum; ab eodem tamen intra ſe retrahitur & indureſcit, ac quodammodo palleſcit. Cibos omnis generis perferre poteſt vir iſte; fructus tamen recentes & olera cibis reliquis non miſcentur, ſed indigeſta exeunt, quemadmodum & juſcula ſine ſolidis cibis aſſumpta. Hæc ſunt quæ ex relatione ipſius conſignavi.

Wittembergæ, *Abr. Vater.*
*Oct.*1. 1720.

Explicatio Figuræ.

A. B. C. D. Inteſtinum colon inverſum, ex vulnere prominens, & verſus ſuperiora & inferiora porrectum.

a. Orificium ſuperius ad inteſtina tenuia patens, per quod exeunt fæces alvinæ.

b. Orificium inferius, quod ducit ad rectum, & Clyſterem per idem infuſum per anum reddidit.

c. d. Veſtigia vulneris à Chirurgo ampliati; per cujus medium adhuc apertum inteſtinum prominet.

e. Umbilicus.

f. f. f. Glandulæ copioſiſſimæ.

g. Regio Inguinalis.

h. h. Dorſum.

i. Coxendix.

K. Regio Lumbaris.

II. *Obſervations upon the Bones and the* Perioſteum, *in a Letter to the* Royal Society, *from Mr.* Leeuwenhoek, *F. R. S.*

Delft, Nov. 20. 1720.

SOME Years ago, I communicated to the *Royal Society*, my Obſervations upon the Bones; what thoſe Obſervations were, I do not now remember, nor have I endeavour'd to recollect them, leſt I might be influenced by them in my later Enquiries, which I here preſent you with.

In my laſt Letter I acquainted you, that I had ſeveral times endeavour'd to diſcover, after what manner the *Perioſteum* was united to thoſe Veſſels, that compoſe the Bone, but that I had not been able to obſerve it to my ſatisfaction. Since which time I have frequently employ'd myſelf in making Obſervations upon the Bones, and have found that the ſuperficial part of them conſiſted of an inconceivable number of ſmall Veſſels, and ſome few of a larger ſize, which laſt, when they came to the ſurface of the Bone, appear'd to me to be cloathed with either a Membrane, or a bony Subſtance, that was perfectly tranſparent.

I once happen'd to diſcover in a ſmall portion of a Shinbone, four or five Veſſels of ſuch a ſize, that a ſingle filament of Silk might have been drawn thro' their Aperture. One of theſe appear'd to me to conſiſt of two openings, each of which ſeem'd to be pro-

vided

vided with a Valve, which was diſpoſed in ſuch a manner, as to let out what was contain'd in the Veſſel, but to ſuffer nothing to go in. In all my Obſervations upon the ſurface of the Bones, I never, but this one time, diſcover'd ſo many of theſe Veſſels in ſo ſmall a compaſs.

As for that Matter, which iſſues out from the Bone, and is carried into the *Perioſteum*, I have diſcover'd the ſource of it to be the ſpongy or cellular Subſtance on the inſide of the Bone, which is the repoſitory for the Marrow.

This ſpongy Subſtance conſiſts of long Particles cloſely united and link'd together, which Particles are compos'd of an infinite number of ſmall Veſſels, ſome running lengthwiſe, and others taking their courſe towards the ſides of the bony Particles.

Theſe bony Particles, notwithſtanding their great number of Apertures, are yet exceeding hard, and lie ſome of them parallel, aud others perpendicular to the length of the Bone.

Thoſe Particles, that lie perpendicular to the length of the Bone, have Veſſels proceeding from their ends; and from their ſides, where they do not lie cloſe together, proceed other Veſſels, that compoſe the *Cortex*, or ſuperficial part of the Bone. And thoſe bony Particles that lie parallel to the length of the Bone, ſend out Veſſels from their ſides, that iſſue out thro' the ſide of the Bone: It is impoſſible for thoſe, who have not ſeen this with their own Eyes, to conceive the prodigious number of ſmall Veſſels, of which the cortical part of the Bone conſiſts; which in ſome places lies no thicker upon the ſpongy part of the Bone, than a thick Hair of a Man's Head, tho' in other places it has three or four times that thickneſs.

To the *Cortex* of the Bone, the *Perioſteum* is united, not only on the outſide, but even by entring in many places into the very ſubſtance of the Bone, and is join'd to it by the Veſſels, which iſſue out from the Bone, in ſuch a manner, that ſometimes one cannot determine which is the Bone, and which belongs to the Membrane inveſting it, they both appearing in the Microſcope to conſiſt alike of exceeding ſmall Veſſels.

To make this be the better underſtood, I have given in Fig. 1. a repreſentation of a ſmall part of the Bone, with the *Perioſteum* adhering to it, in which *ABCDEF* repreſents the bony part, whether taken from an Ox, or Sheep, I do not now remember. The *Perioſteum* is mark'd with the Letters *BGHIE*, the thickneſs of which is deſign'd by *BG*, or *IE*, tho' in other places of the Bone, and even at no greater diſtance than two or three Hairs breadth, it is twice or thrice as thick. We ſee here, that all the ſmall Veſſels in the *Perioſteum* are repreſented by ſo many Dots or Points; but in other places, where I had ſeveral times ſeen the Membrane of twice this thickneſs, the upper half of it has appear'd to be of a different make from the under part, for as much as in the upper part I could diſcover not only thoſe Veſſels, that had been cut tranſverſly, and which conſequently were repreſented by ſo many Points, but likewiſe a great number of other Veſſels running lengthwiſe along the Membrane, as is repreſented in Fig. 2. by *LOPQNM*.

I am fully perſuaded, that the part repreſented by *BGHIE*, Fig. 1. is not entirely membranous, but that ſome part of it is really bony. If we cut thro' the *Perioſteum* ſo deep as to divide the part of the Bone mark'd with the Letters *ABCDEF*, in the ſame Figure, we find the ſame appearance of Pores in the bony Subſtance, which are no other than the tranſverſe Sections of ſmall Veſſels; and beſides theſe, there are other Veſſels running longways in the Bone. And we find juſt the ſame in thoſe tranſparent parts, that lie between the bony Particles, which are repreſented thicker between *B C D E*, than they appear'd to me.

It is my Opinion, that the uſe of theſe bony Particles is, to convey an Oleaginous Liquor into the *Perioſteum*, and that from the *Perioſteum* it is carried by the intervention of the other Membranes into all parts of the Body, when in a healthful Condition.

In another place, I ſaw a great number of Veſſels ariſing from a greater depth within the Bone, which drew cloſer together, ſo as to compoſe ſmall *Faſciculi*, before they entred the *Perioſteum*, in which they ſeparated one from another, and diſpers'd themſelves again. It is difficult to determine, whether theſe Veſſels bring any Liquor into the Bone, or carry it out; but I rather think they ſerve to carry it out of the Bone.

Having placed another piece of Bone before the Microſcope, with the *Perioſteum* adhering to it, I could diſcover a great number of Veſſels, that I had cut thro' lengthways, as they ran along the *Perioſteum*, and others that were cut thro' tranſverſly, and appear'd as ſo many Points, as is repreſented in Fig. 2. by *K L O P Q N A*, where the bony part is mark'd by the Letters *K L M N A*, in which, tho' no Pores, or Veſſels, are here repreſented, yet is it full of openings. That part, which is deſign'd by *L O P Q N M*, we muſt not take to be entirely membranous, for I am of opinion, that that part of it, which lies next the Bone, and which is repreſented by *L M N*, is of a bony Subſtance.

I had another ſmall piece of Bone lying before a Microſcope, of which I caus'd a part to be repreſented by *R S W X T V*, Fig. 3. in which *R S T V* is the Bone, and *S W X T* the *Perioſteum*, which in this place was no thicker than a thick Hair of a Man's Beard, but in another part of the ſame Bone at a ſmall diſtance, it was full four times that thickneſs.

I placed another piece of Bone before a Microſcope in ſuch a manner, as that the Bone did not appear, but only the *Perioſteum* and the Muſcular Fibres, which were cut thro' tranſverſly, and appear'd to be ſurrounded by the *Fibrillæ* of the *Perioſteum*, as is repreſented by *T Z C D A B*, Fig. 4. where *T Z A B* is the *Perioſteum*, and *Z C D A* are the fleſhy Fibres cut thro' tranſverſly. This piece of Bone was taken from one of the Ribs of a fat Ox, and I was ſurpris'd to find, that in this place, as I cut longways thro' the Rib, I could not diſcover any Particles of the Marrow, whereas in other parts the Rib abounded with them.

Notwithſtanding the great number of Obſervations that I have made upon the Bones, and the Membrane that ſurrounds them, which is commonly call'd the *Perioſteum*, I have never been able to ſatisfy myſelf entirely about them. I ſtill imagin'd, that the part of the *Perioſteum*, which immediately covers the Bone, and is ſtrictly united to it, muſt have a degree of hardneſs approaching to that of the Bone, and that at a ſmall diſtance from the Bone, the *Perioſteum* muſt have a ſoftneſs and flexibility like that of the carneous and adipoſe Membranes.

I had lying by me four pieces of Ribs of a fat Ox, which I had kept by me full two Months, and which were now grown very dry. From one of theſe I tore off the *Perioſteum*, which I found ſtuck much harder to the Bone than I could have imagin'd, and I obſerv'd, that a great many Particles of this Membrane were left on cleaving to the Bone. This I did with deſign to make ſome Obſervations on the ſuperficial part of the Bone, which is not near ſo hard as thoſe bony Particles, that lie a little deeper. From this Bone I cut off ſome very thin ſlices, both along the Bone, and likewiſe tranſverſly, one of which I placed before a Microſcope, and gave it to the Painter, that for many Years has drawn all my Obſervations.

This Piece is repreſented by *A B K C*, Fig. 5. having been cut off tranſverſly, and as thin as poſſible, from the Rib, with part of the *Perioſteum*, as from *K* to *C*, ſtill adhering to the Bone, and another part of it torn off from the Bone, as deſign'd by *B K D*, except that in ſome places the Bone and the Membrane are ſtill united by Veſſels torn out of their places, that run from one to the other. In this Figure *D E F C* repreſents the *Perioſteum*, and the part deſign'd by *E G H I F*, is ſomething lying upon it, which I could not tell what to make of, tho' it appear'd to me to be Membranous.

I had likewiſe ſome very thin ſlices ſhaven off from the Rib both of an Ox, and of a Calf, from which I tore off the *Perioſteum* entirely, or at leaſt as much of it, as poſſibly I could; after which I cauſed the edge of the Bone it had ſtuck to, to be repreſented by the crooked Line, *L M N*, Fig. 6.

In Fig. 7. *O P Q* repreſents the edge of another ſmall ſlice of Bone, from which the *Perioſteum* has been torn off, by which appearance it ſhould ſeem, that the Union of the *Perioſteum* with the Bone is ſo firm and ſtrong, that, in ſeparating it, ſome of the ſuperficial Particles of the Bone are torn off with it.

I have likewiſe diſcover'd ſome Veſſels running along within the Marrow-bone of the Shank of an Ox, that ſeem'd to be Blood-veſſels.

Since now it appears from our Obſervations, which have been made with great Diligence and Care upon Bones of all kinds, that the Bones do for the moſt part conſiſt of exceeding ſmall Veſſels, which Veſſels ariſe from the inner, hollow, or ſpongy part of the Bone, and paſſing thro' the ſuperficial or cortical Subſtance, enter the *Perioſteum*, and are from thence continued farther into the Body, yea even into the utmoſt parts of the Body; we may from hence reaſonably conclude, that, in a healthful Body, as there is a conſtant ſupply of an Oily Subſtance carried into the Bones, ſo this is again conſtantly carried out from the Bones by means of theſe Veſſels, into all parts of the Body, even to the extremities of the Fingers. To give an evident proof of this, let any Man lay the ends of his Fingers upon a clean and bright Pewter Plate, and he ſhall find the Pewter appear ſoil'd in the place where he has touch'd it; for in reality this Soil is nothing elſe, but ſome Oleaginous Particles diſcharg'd from the ends of his Fingers. There is indeed ſomething of a watery Subſtance mix'd with the Oily Particles, but this evaporates in a little time, and leaves the Oily Particles lying upon the Plate.

III. *An Account of a præternatural Tumour on the Loins of an Infant, attended with a* Cloven Spine. *By Dr.* Rutty, *Fellow of the College of Phyſicians, and of the* Royal Society.

THE curious Mr. *Ruyſch*, in an Obſervation on the *Spina Bifida*, takes notice that other Writers have deſcrib'd it to be cloven into two equal parts longitudinally, as Butchers ſplit the Back-bone of an Ox or Sheep; whereas out of ten Subjects, which he had an opportunity to examine, not one prov'd to be in that manner, the Body of the *Vertebræ* being entire, and the *Acute Proceſſes* only divided. Pretty near this Deſcription, is the *Spine* to be treated of, and I the rather trouble this Society with it, becauſe it don't appear by the Tranſactions, that it has as yet come before us.

The Back-bone is of a female Infant ſix Days old; whoſe Mother, when ſeven Weeks gone with Child, upon a fright occaſion'd by her Husband's falling from a Horſe and very much bruiſing his Loins, gave the *Embryo* this injury; but notwithſtanding ſhe went out her Time, and the Child was full grown.

There appeared upon the region of the Loins, in the ſame place where the Father receiv'd his hurt, a Tumour about the bigneſs of a ſmall Turnep, with a broad *Baſis*, around which the Skin was diſcolour'd, as by an *Ecchymoſis*, but it grew immediately from thence pellucid, like a Veſicle rais'd by *Cantharides*, and continued ſo throughout, except juſt at its *Apex*, where was a Subſtance like a *Fungus*. This Bladder was fill'd with a Liquor, which in Scent and Colour reſembled the true Urine: inſomuch, that upon ſtrictly examining the Linnen ſtain'd by what iſſu'd from hence, with that from the *Pudenda*, we cou'd perceive no ſenſible difference, and concluded there was a Communication between the left Kidney and the Orifice, into which the Surgeon's Probe paſs'd obliquely upwards, about an Inch. Of the ſame opinion was Dr. *Pellet*, then preſent. Whether the ſame Similitude may hold, betwixt the Urine and this Liquor, in all Caſes of the like Nature, I have not had opportunites to obſerve: but as Mr. *Ruyſch* has taken no notice of it, in ſuch a Number of Subjects, that have been before him, I am apt to think, that it does not; being eaſily perſuaded, that if it did, it would not ſo often have eſcap'd his inquiſitive Reſearches. I cou'd have wiſh'd however, that this Fluid had been ſav'd; to have try'd by Experiments, in what other Qualities, beſides the Scent and Colour, it might correſpond with the Urine; which wou'd have given us a farther Inſight into the Cauſe of this Affinity between them, in the particular inſtance now before us.

Upon opening the Body, where were preſent Dr. *Pellet*, and Mr. *Stephens* an ingenious Surgeon, the Kidneys, contrary to Expectation, were *perfect*, and did not any way communicate with the outward Orifice.

But upon clearing away the Fungous Subſtance, which took up all the *Sulcus* or Hollow of the *Spine*, we found where the Perforation tended; a long Probe eaſily paſſing up the Channel, which contains the *Medulla Spinalis*. Throughout this *Fungus* were diſpers'd a great many endings of ſmall Nerves, from whence diſtill'd this, as it were, Urinous Liquor, which occaſion'd the Tumour: the reſt of the *Medulla* was more compact, and fill'd the Cavity of the *Spine*; tho' in ſome Subjects it has been waſted to ſuch a Degree, that by blowing

into

into the Orifice you may inflate the *Dura Mater*; as was done by Mr. *Dobyns* a Surgeon, in a Caſe of this Nature. The Coat of the Tumour conſiſted at its *Baſis* of the *Cuticula*, *Cutis*, and *Membrana Adipoſa*: the two firſt of which forthwith terminating, the *Cuticula* only was continued; immediately under which appear'd the *Muſcles* and *Fungus* above mention'd. In this it differs from thoſe of Mr. *Ruyſch*, which received their Coats from the *Membranes* that cover the *Medulla*: as likewiſe from thoſe taken notice of by *Tulpius*, which borrow'd theirs from the *Peritonæum*, as he ſays; tho' by his own Deſcription, it is more likely they proceeded from the ſame Membranes.

Theſe Tumours conſtantly attend the *Spina Bifida*: inſomuch, that when any of them preſent themſelves on the Loins or Back of a new-born Inſant, we have good grounds to pronounce *This* to be the Cauſe; but we may be poſitive in it, if the Child can't move its lower Limbs; the want of which motion is an infallible Indication, that the *Medulla Spinalis* reaches no farther than the Swelling, whereby the Nerves are not diſtributed to thoſe Parts. They appear differently in different Subjects. In ſome the whole Tumour is *Opake*; which proceeds from ſmall Filaments of Nerves propagated in great numbers throughout its Coat, and not from the thickneſs of the Skin, as *Ruyſch* will have it. In others it is *pellucid*; and then the *Medulla* terminates at once at the Aperture of the *Spine*, and does not ſhoot out into any Ramifications. This before us is a Compoſite of both Species, the greater part of which was *pellucid*; the leſs, *viz.* the *Apex*, *opake*.

The Back-bone itſelf, you may obſerve, is not cleft, but has its *Vertebræ*, with their *other Proceſſes* entire, and is only defective in its *Spines* or *Acute ones*. But that Portion of the *Vertebræ*, which ſhould make an acute Angle, from whence their *Spinal Proceſſes* naturally ariſe, in order to form the *Specus* or Paſſage for the Marrow, inſtead of that, gapes and lies almoſt in a ſtrait Line on each ſide; whereby the *Medulla* is defrauded of its uſual Guard from external Injuries. This Defect begins at the third *Vertebra* of the Loins, and is continued to the end of the *Os Sacrum*: which laſt is very extraordinary, it ſcarcely ever happening to this Bone; the reaſon of which I take to be the more than ordinary Compactneſs of its *Vertebræ*, whereby it is leſs liable to be injur'd by any Impreſſion. For we obſerve, that in Adults they grow ſo cloſe together, as to unite and form one large and ſolid Bone.

As the Caſe before us is a *Vitium Conformationis* owing to the Mother's Imagination, ſo the Same ſometimes is occaſion'd by *Matter* lodging upon the *Spine*, and eroding the *Vertebræ* by its Acrimony: but then you find a *carious Bone*, whereas in theſe *præternatural* Caſes, there remain no ſuch Footſteps. This was obſerv'd in the Body of the late Lord *Peterborough*, as related by Mr. *Cowper*: Upon the opening of which, He found a large Tumour, from whence flow'd a browniſh colour'd Matter; and under it, the upper and fore part of the ninth, and the lower part of the eighth *Vertebræ* of the *Thorax* were conſumed and gone, the *Medulla Spinalis* being cover'd with its *Membranes only*. This gave riſe to thoſe Symptoms in that *morbid* Caſe, ſo nearly reſembling the above recited in this *prӕternatural* One.

It is manifeſt from what has been ſaid, that theſe Caſes are incurable, and muſt in a little time kill the Patient. But it is almoſt immediate Death to open the Tumour; which every Surgeon will naturally do, that has not ſeen or read concerning it: the Fluid ſeeming to require a Diſcharge. It is for the ſake of thoſe Gentlemen that I have taken upon me to deſcribe it, who poſſibly may not have the above-mention'd Authors in their Hands; the only Ones that have obſerv'd it with any tolerable Accuracy.

IV. *An Account of Two Obſervations in Gardening, upon the change of Colour in Grapes and Jeſſamine. In a Letter from Mr.* Henry Cane.

Hillingdon *near* Uxbridge,
Oct. 31. 1720.

BEING a little curious in the Collection of all my ſorts of Fruits, amongſt other ſorts, I have three or four of white Muſcadine Grapes, very diſtinguiſhable by their Taſtes, one of which pleaſed me above the reſt; a Cutting of which about ſix Years ſince I planted againſt a Wall, on an Eaſtern Aſpect, where it has the Sun from its riſe till half an hour after Twelve. The Soil is a ſtiff Clay, but to make it work the better, I meliorated that, by mixing ſome Rubbiſh of the Foundation of an old Brickwall, where it now grows. In *January* laſt was twelve Month I pruned it, and the Figure was thus,

Left Hand T Right Hand *Black*

At time of Year it ſhot at both Hands about twenty two Inches of a ſide, before it came to a Joint; that on the Right was a very luxurious exuberant Branch, as big as the body of the Tree, the other ſide not half ſo thick or big, and the Leaves on the right were as big again as the other on the Left-hand, and I fancy the largeſt that were ever ſeen. The Right-hand bears a very large and good black Grape, and large Bunches; the Left-hand very good white Grapes, and I had laſt Year more Bunches of the White than of the Black; and whereas in all Vines bearing black and blue Grapes the Leaves die red, theſe died white on the black ſide as well as t'other. Laſt *January* I pruned the Tree again, but tack'd up more of the Right-hand (being Black) than I did on the Left, for which reaſon I had this Year a great many more of the Black than I had of the White, and they ripen'd for the Seaſon of the Year very well; divers Gentlemen of the Country both ſaw them laſt Year and this Year, and taſted of them. I gather'd the laſt about eight Days ſince, and the Leaves die White this Year alſo, being the ſecond Year that ever it bore. I think to prune it pretty cloſe on both ſides this Year, and to plant out divers Cuttings of both ſorts of it.

I will mention one thing more to you, which I have experienced about 28 Years ſince; I do it becauſe Mr. *Lawrence* in his firſt Tract of Gardening, makes mention of the Plant, but I take mine to be a much different Caſe from his; I mean the yellow and green ſtrip'd *Jeſſamine*. In the Month of *April*, *An.* 1692, having a ſmall Plant of our common white Jeſſamine, which ſtood in the Ground, and was no bigger than a Tobacco-Pipe, I cut it off at two Joints above the Ground, and grafted it with a Cutting of the Yellow-ſtrip'd; it took and ſhot a ſmall weak Shoot, and in a Month or five Weeks after, it was blighted, and I perceived it had killed the Graft, and ſome part of the Stock below, ſo I took my Knife and cut it to the quick, which was near the next Knot or Joint to the Ground, and let it ſtand, thinking to graft it again at Spring, as before, but forgot it till the Seaſon was paſt. At length going that way by it, I ſaw it had broke out at the next Joint with ſeveral Shoots of the yellow and green ſtrip'd; and not only there, but it had alſo made a ſtrong Shoot from the Root, of yellow and green ſtrip'd; after a while I took it up with Mold to the Root, and put it in a Pot, and it flouriſh'd all the Summer: And going to ſee my Son at *Magdalen College, Oxon,* I took it with me, and made a Preſent of it to the Preſident of the College, where it flouriſh'd two or three Years, and then for want of ſhifting the Pot in time, it was matted ſo to the bottom and ſides of the Pot, that it killed it; I alſo at that time gave the Fellows of that College, and ſeveral others of my Acquaintance an account of the Circulation and Deſcent of the Sap in that Plant, and I have try'd ſeveral other ſorts of variegated Plants, but do not find any of them to tranſmute, as that Jeſſamine will do.

I am, &c.

H. Cane.

V. *An Account of ſome new Electrical Experiments. By Mr.* Stephen Gray.

HAVING often obſerved in the Electrical Experiments made with a Glaſs Tube, and a Down Feather tied to the end of a ſmall Stick, that after its Fibres had been drawn towards the Tube, when that has been withdrawn, moſt of them would be drawn to the Stick, as if it had been an Electrick Body, or as if there had been ſome Electricity communicated to the Stick or Feather; this put me upon thinking, whether if a Feather were drawn through my Fingers, it might not produce the ſame Effect, by acquiring ſome degree of Electricity. This ſucceeded accordingly upon my firſt trial, the ſmall downy Fibres of the Feather next the Quill being drawn by my Finger when held near it: and ſometimes the upper part of the Feather, with its Stem, would be attracted alſo; but not always with the ſame Succeſs. I then proceeded to try whether Hair might not have the ſame Property, by taking one from my Wig, and drawing it 3 or 4 times through my Fingers, or rather between my Thumb and Fore-Finger, and ſoon found it would come to my Finger at the diſtance of half an Inch; and ſoon after I found that the fine Hair of a Dog's Ear was ſtrongly Electrical; for upon taking the Ear and drawing it through my Fingers, great numbers of them would be attracted to my Fingers at once. I was at this time in the Country, and made no further trial till my return to *London*, (which I did about the middle of *November* laſt) and having repeated what I had before mentioned, the next thing which I thought of, was threads of Silk of ſeveral colours, and of ſeveral fineneſſes, which I found to be all Electrical, but ſometimes I could not ſucceed; the reaſon of which I afterwards found, as will appear in the ſequel of this Diſcourſe.

Having ſucceeded ſo well in theſe, I proceeded to larger quantities of the ſame Materials, as pieces of Ribband both of coarſe and fine Silk of ſeveral colours, and found that by taking a piece of either of theſe of about half a yard long, and by holding the end in one Hand, and drawing it through my other Hand between my Thumb and Fingers, it would acquire an Electricity, ſo that if the Hand were held near the lower end of it, it would be attracted by it at the diſtance of 5 or 6 Inches; but at ſome times the Electricity would be much weaker than at others, the reaſon of which I conjectur'd to be, that the Ribband might have imbibed ſome aqueous Particles from the moiſt Air, which I found to be upon trial the occaſion of it; for when I had well warmed the Ribband by the Fire, it never failed to be ſtrongly Electrical.

After this I made trial of ſeveral other Bodies, as Linnen of ſeveral ſorts, *viz.* Holland, Muſling, *&c.* and Woollen, as of ſeveral ſorts of Cloth and other Stuffs of the ſame Materials. From theſe I proceeded to Paper, both white and brown, finding them, after they had been well heated before rubbing, to emit copiouſly their Electrick Effluvia. The next Body that I found the ſame Property in, was thin ſhavings of Wood; I have only as yet tried the Fir Shavings, which are ſtrongly Electrical. The three laſt Subſtances which I found to have the ſame Property, are Leather, Parchment, and thoſe thin Guts wherein Leaf-Gold is beaten.

All theſe Bodies will not only by their Electricity be drawn to the Hand, or any other ſolid Body that is near them; but they will, as other Electrick Bodies do, draw all ſmall Bodies to them, and that to the diſtance of ſometimes 8 or 10 Inches. Heating them by the Fire before rubbing very much increaſes their Force.

There is another Property in ſome of theſe Bodies, which is common to Glaſs, that when they are rubbed in the dark, there is a Light follows the Fingers through which they are drawn; this holds both in Silk and Linnen, but is ſtrongeſt in Pieces of white preſſing Papers, which are much the ſame with Card Paper; this not only yields a Light as above, but when the Fingers are held near it, there proceeds a Light from them with a crack-ling Noiſe like that produced by a Glaſs Tube, though not at ſo great a diſtance from the Fingers; to perform this, the Paper before rubbing muſt be heated as hot as the Fingers can well bear.

A Down Feather being tied to the end of a fine thread of Raw Silk, and the other end to a ſmall Stick, which was fixed to a Foot, that it might ſtand upright on the Table; there was taken a piece of brown Paper, which by the above-mentioned method was made to be ſtrongly Electrical, which being held near the Feather, it came to the Paper, and I carried it with the ſame till it came near the Perpendicular of the Stick; then lifting up my Hand till the Paper was got beyond the Feather, the Thread was extended and ſtood upright in the Air, as if it had been a piece of Wire, though the Feather was diſtant from the Paper near an Inch. If the Finger were held near the Feather in this Poſition, the greateſt part of the Fibres next the Paper would be repelled, when at the ſame time if a Finger were held to the Fibres that were more remote from the Paper, they would be drawn by it.

I then repeated this Experiment without the Feather, *viz.* by a ſingle thread of Silk only of about 5 or 6 Inches long, which was made to ſtand extended upright as above-mentioned, without touching the Paper; then placing my Finger near the end, it wou'd avoid, or was repelled by it, but when I had placed my Finger at about the ſame diſtance from a part of the Thread, that was about two Inches from the end, it was then attracted by it.

An Enumeration of the ſeveral Bodies mentioned herein, that are found to be Electrical.

1. Feathers, 2. Hair, 3. Silk, 4. Linnen, 5. Woollen, 6. Paper, 7. Leather, 8. Wood, 9. Parchment, 10. Ox-Guts, wherein Leaf-Gold is beaten.

VI. *A Letter to Dr.* Halley, *Aſtronomer Royal, R. S. Soc. in anſwer to ſome Objections made to the Hiſtory of the Antiquity of the* Venereal Diſeaſe. *By Mr.* Beckett, *Surgeon, F. R. S.*

SIR,

I Was of opinion that what I had ſaid in my two former Letters, had been ſo full and ſatisfactory that I ſhould have had no occaſion to have given my ſelf or any body elſe, any farther trouble upon this head: But foraſmuch as I find there have been two Objections made againſt what I have advanced by one or two learned Gentlemen, I ſhall take upon me to anſwer them, and endeavour to prove they do not in the leaſt invalidate the Authorities I before produced. The firſt is, that the *Venereal Diſeaſe* ſo well known among us now, and the *Leproſy* of former Ages, could not be the ſame Diſeaſe, becauſe the *Leproſy* is not to be conquered by Salivation, which the other generally very readily yields to. In anſwer to this, I am to obſerve, that the *Leproſy*, which we have among us at this time, affects only the ſurface of the Body, the Skin generally appears ſcaley, with a certain deep red colour, or ſmall Sores upon removing the Scales, and ſometimes a Scabbineſs, with a redneſs of the Skin, which affects different parts of the Body. I have known both the Cheeks only affected, both the Arms for the breadth of the Palm of the Hand, ſometimes the Breaſt, the Legs, and other Parts; but this may continue upon the Patient during his Life, as it frequently does, and never make any farther Progreſs;

which ſhews it to be a cuticular Diſeaſe: In theſe Caſes upon Salivating the Patients, the Scales generally fall off, the redneſs diſappears, and the Cure ſhall ſeem to be compleated: but in a Month or two, the ſame inconveniencies generally attend them as before. But one ought not to conclude, that becauſe our *Leproſy* will but rarely be cured by Salivation, and the *Pox* generally will, that many of thoſe Perſons the Ancients judged to be *Leprous*, were not really *Venereal*; for their *Leproſy*, as they call'd it, was a quite different Diſeaſe from ours. Had there been any Proof brought that Perſons had been Salivated in their *Leproſy*, and failed of Cure, it would have determin'd the Caſe; but on the contrary, we are aſſured by the learned Dr. *Pitcairn*, in his Diſſertation concerning the Ingreſs of the *Lues Venerea*, That the *Leproſy*, before the *Neapolitan* Diſeaſe was talked of, was cured by *Mercury*, and now ſince it changed its Name, it is no longer heard of. Thus we find that their *Leproſy* and our *Venereal Diſeaſe* would be cured by the ſame Method, but their *Leproſy* and ours, being abſolutely different Diſeaſes, we by no means ought to expect the Succeſs, from the ſame proceſs of Cure, ſhould be the ſame. I dare be poſitive that no body ever obſerved our *Leproſy* to be attended with falling of the Hair, hoarſneſs of the Voice, the Patient ſpeaking as though he ſpoke through the Noſe, Conſumption of the Fleſh, Ulcers all over the Body, corruption of the fleſhy Parts, and of the Bones themſelves, filthy Ulcers of the Throat, corroſion and falling of the Noſe, all which are reckon'd as Symptoms of their *Leproſy*; on the contrary, ours is a mild and almoſt inoffenſive Diſeaſe, which a Perſon may be affected with during his Life, and never become worſe; whereas the other by diſplaying it ſelf under the Symptoms before enumerated, brings the Patient to the moſt miſerable end; beſides this, their Diſeaſe was got by Coition as their Authors aſſure us, but in our *Leproſy*, a diſeaſed Husband may cohabit with his Wife as long as he lives, and he ſhall never be able either by Coition, or the immediate contact of the diſeaſed Parts with thoſe that are ſound, to communicate any Evil. Had what our Predeceſſors called the *Leproſy* been the ſame Diſeaſe we call by that Name now, they had not been ſo ſolicitous of making ſuch large Proviſion for them, or ſhutting them up from Humane Society; for one of our *Leprous* Perſons might have been among them, and no body have known he laboured under any Infirmity at all. From hence it is evident the Diſeaſe ſo common among them, was entirely different from our *Leproſy*, the Appearances of which bear no manner of Analogy with the former: 'Tis from the Symptoms of the Diſeaſe, and the manner of its being received, that we generally know one Diſeaſe from another; but the Symptoms of moſt of their *Leprous* Perſons, and the manner whereby the Diſeaſe was gotten, will be found in no other Diſeaſe that attacks the humane Body, but in the *Venereal Diſeaſe* only; for here they ſo exactly agree, that we muſt in a manner do violence to our own Reaſon, if we deny them to be the ſame. I proceed now to anſwer the ſecond Objection, which indeed was long ago falſly aſſerted by Dr. *Fuller* the Hiſtorian; which is, that the *Leproſy* was brought into *England* from the Holy War, by ſome of our Countrymen, and that the Diſeaſe was altogether unknown among us before. This, as I take it, does not ſo immediately concern me, ſince all I take upon me to prove is, that what *They* called the *Leproſy*, is not ſame Diſeaſe we call by that Name now, but another. However, I ſhall in a few Words make it appear that this Objection is likewiſe groundleſs, by obſerving that the firſt *Engliſhmen* that went over to the Holy War, made their firſt Voyage in the Year 1096, as our Hiſtorians generally agree, and that ſome of them returned in 1098, two Years after that Expedition; but moſt certain it is, we had the *Leproſy* among us before, for *Wharton, de Epiſcopis Londinenſibus*, and other Hiſtorians aſſure us, that *Hugo de Orivalle*, one of the Biſhops of *London*, died here of the *Leproſy* in the Year 1084, which proves our Countrymen did not bring that Diſeaſe firſt from the Holy War, becauſe we had it among us before. The account *William* of *Malmesbury* gives of this Biſhop's Diſeaſe, is as follows. *Is poſt paucos ordinationis annos in morbum incurabilem incidit. Siquidem regia Valetudo totum corpus ejus purulentis ulceribus occupans ad pudendum remedium tranſmiſit. Nam credens aſſerentibus unicum fore ſubſidium ſi vaſa humorum receptacula, verenda ſcilicet, exſecantur, non abnuit. Itaque & opprobrium ſpadonis tulit Epiſcopus, & nullum invenit remedium, quoad vixit leproſus.* Now its highly probable, had this been a new Diſeaſe the Biſhop died of, the mention of it as ſuch, would not have eſcaped our Hiſtorian, but on the contrary it ſeems to have been anciently known among us, becauſe the Remedy made uſe of for it was ſo, it having been recommended by *Ætius*, and other Phyſical Writers ſeveral hundred Years before this time; and I think its very plain that the cutting off the *Teſticles*, and with them the Veſſels formed for the receiving the Humours as expreſſed in the former Caſe, was by them looked upon to be of peculiar Service, becauſe its probable that obſerving the Diſeaſe to begin in theſe and the neighbouring Parts, they thought the very *Minera Morbi*, would be by this means deſtroyed, and the Diſeaſe either cured or the ſpreading of it prevented.

I am, SIR,

Yours, &c.

William Beckett.

VII. *An Experiment to compare the* Paris *Weights as they are now us'd at* Paris, *with the* Engliſh *Weights. By the Reverend* J. T. Deſaguliers, LL.D. *F. R. S.*

FInding the Accounts which we have of the *French* Weights different in different Books, I ſent to a curious Gentleman for ſome *Paris* Weights exact to the Standard Weights at the *Chatelet*; and found upon tryal, the *Paris* Ounce, which contains 576 of their Grains, to be equal to 476 of our Grains Troy; from which Experiment all the other Proportions may be deduc'd.

The *French* Pound cont. 16 Ounces.
Ounce —— 8 Drams, or 576 *Paris* Grains.
Dram —— 3 Deniers.
Denier —— 24 Grains.

VIII. *Some*

VIII. *Some Remarks upon the Method of obſerving the differences of Right Aſcenſion and Declination by Croſs Hairs in a Teleſcope. By Dr.* Edm. Halley, *Aſtr. Reg.* R. S. S.

THoſe that are curious in obſerving the Heavenly Motions, and particularly myſelf, whoſe Buſineſs it is, are greatly obliged to the late Signior *Caſſini*, for his Thought of applying Threads at half Right Angles in the common *Focus* of a Teleſcope, to determine thereby the differences of Right Aſcenſion and Declination of any two Stars, whoſe ſituation is ſuch, that by their diurnal Motion they follow each other through the Aperture of the Teleſcope, ſo fixt as that the firſt of them may paſs over the Centre of the Glaſs, and move exactly along one of the Threads, whilſt the interval of time between the Tranſit thereof, and that of the following Star, is exactly meaſured by a *Pendulum* Clock well adjuſted to the mean motion of the Sun, or elſe to the Revolution of the fixt Stars, whereby the difference of Right Aſcenſion is given; as is the difference of Declination, by the time the following Star takes to paſs from one diagonal Thread to the other. This manner of obſerving being long ſince publiſhed, will not in this place need any further Explication; but it may not be amiſs to ſay ſomething of the ſufficiency thereof, and of the exactneſs of which an Inſtrument of ſo little charge and *Apparatus* is capable; eſpecially being at this time obliged to make uſe of it and the Micrometer only, for my Obſervations. I need not ſay with what exactneſs Dr. *Pound*, and his Nephew Mr. *Bradley* did, myſelf being preſent, in the laſt Oppoſition of the Sun and *Mars*, this way demonſtrate the extream minuteneſs of the Sun's Parallax, and that it was not more than 12″, nor leſs than 9″, upon many repeated Trials, it having been ſoon after the time laid before the Society. But being mindful that in *October* next, *Mars* would be again in Oppoſition to the Sun, about the tenth degree of *Taurus*, but would not come very near any fixt Star that has a place in Mr. *Flamſteed*'s Catalogue; I was ſolicitous to ſee if there were any Teleſcopick Stars to which he would very nearly approach; and on the 28th of *February* laſt, the Heavens being very ſerene and clear in the Evening, and *Venus* having nearly the Declination in which *Mars* will move in *October* next, I fixt my Teleſcope on her, at 7h. 28′ equal time, and noted the moment ſhe paſs'd over the Center of my Glaſs, or rather the common interſection of the four croſs Hairs; and in half an Hours time I noted eight very conſpicuous Stars, four of which being within the compaſs of one Degree, fell very nearly in the ſaid way of *Mars*, and from the intervals of Time I then obſerved, with their difference of Declination from *Venus*, I determined their Right Aſcenſions and Declinations, as well as her Place from my Tables, (which by Obſervation I found at this time needed no correction) would allow me; they all falling between the ninth and tenth degree of *Taurus*, with very little Latitude. But what confirm'd me that all was right was, that on *Tueſday* laſt *March* 21. *Mercury* appearing very fair, and newly paſt his greateſt Elongation, I found by *Senex*'s Zodiack that he was nearly in the ſame parallel that *Venus* had before deſcribed; and though the brightneſs of the *Crepuſculum* effaced the ſmaller Stars, yet in a quarter of an Hour I had one paſt 10′ ¼ more Southerly than the Planet, which in leſs than 3′ of Time was ſucceeded by another, which was but one Minute more Northerly than the former; when after an interval of about 14 Minutes of time, in which I was ſurpriſed to find the Sky ſo void of Stars, the four before mentioned Stars paſt ſucceſſively over my Glaſs, with the ſame interval of time in which I had ſeen them follow one another, on the 28th of *February*; whereupon I was deſirous to try, whether, if the place of *Mercury* in my Tables were aſſumed, the ſame Right Aſcenſions and Declinations of thoſe Stars would be deduced from him, as from *Venus*; and to my great Satisfaction, I found on trial by an exact *Calculus*, that I had the ſame Right Aſcenſions now as before, in none of the four differing fully half a Minute, ſo that theſe Stars may ſecurely be added to the Catalogue, and the appulſe of *Mars* to them be obſerved in very long Teleſcopes, in *October* next, to a further aſcertaining the immenſe diſtance between the Sun and Earth.

Hence it will alſo appear that our *Mercurial* Numbers are, at leaſt at this time, and in this part of his Orb, not leſs exact than thoſe of *Venus*. And whereas this Planet ſcarce ever appears with us out of the Sun's Beams, and always low, and therefore under great Refraction; this way of obſerving takes off all the uncertainty, that accrues therefrom; and when once the *Zodiack* ſhall be compleated with the Stars that are wanting to fill up the vacant places, it will be eaſy at any time, by this method, to obſerve *Mercury* or a *Comet* within the Sun's Beams, with the ſame certainty, as if it were remote, and out of the neighbourhood of the Horizon, where the different Vapours that lie near the Earth, render the appearances of the Stars ſomewhat dubious upon the account of the irregular Refractions.

March 23. 1721.

IX. *A Propoſal for meaſuring the height of Places, by help of the Barometer of Mr.* Patrick, *in which the Scale is greatly enlarged. By the ſame.*

SINCE *Torricelli* firſt found that the *Mercury* in an inverted Tube was in *Æquilibrio* with the whole Column of Air that was over it, and that the weight of the incumbent Column was various according to the different Diſpoſitions of the Air, in reſpect of ſerene fair Weather, and of rainy, windy, or otherwiſe tempeſtuous Weather: there have been ſeveral attempts and contrivances to make the minute variations thereof more ſenſible. And firſt the Wheel Barometer was thought of, which certainly ſhews theſe variations with great exactneſs, but is only proper for a fixt Station, nor eaſy to be removed; which Circumſtance is required for the principal uſe this Inſtrument is applicable to, and which I would recommend it for.

The next thought for this purpoſe was that of Mr. *Hubin*, deſcribed in *Phil. Tranſ.* N° 184, who returning the Tube of the Barometer, as an inverted Syphon, made a large dilatation in the aſcending Leg thereof, wherein the *Mercury* aſcended, as its Altitude in the other part thereof abated, and *è contra*: over this he drew out a narrow Glaſs Cane, which he fill'd with a tinged Spirit, and which being about fifteen times lighter than *Mercury*, would aſcend about 15 times as much as the *Mercury* in the Barometer fell. This, beſides that the Spirit would dilate and contract itſelf with heat and cold, had the inconvenience of the former, not to be eaſily removed without great danger of diſorder and breaking, by reaſon of the ſmallneſs of the Tube in which the Spirit was to riſe and fall.

This was ſucceeded by Dr. *Hook*'s Marine Barometer, made of two Thermometers, the one the common ſeal'd Weather-glaſs, having no Communication with the outward Air, wherein the temper as to Heat and Cold was ſhewn by the ſwelling or ſhrinking of the included Spirit; the other the old Thermometer made with an inverted Bolt-head, in whoſe globular part was included Air ſomewhat rarer than the ambient, ſo as to make the Liquor which was to riſe and fall in the ſhank of the Bolt-head, always to ſtand above the ſurface of the *Stagnum*, into which its end was immerſed. This ſhew'd the heat of the Air by its own Dilatation; but at the ſame time, the different preſſure of the Atmoſphere mixed with it, ſo that the graduation of theſe two Thermometers being adjuſted to any given height of the *Mercury*, they would at all times when the *Mercury* was at that height, both ſhew the ſame degree of heat: But at other times when the weight of the Air was different, that difference would ſhew itſelf by the diſagreement of the degree of Heat ſhewed by them. This will be better underſtood from N° 269, of the *Tranſactions*, wherein I have deſcribed this Inſtrument at large. This, tho' of admirable uſe at Sea, to give timely notice of approaching bad Weather, labours under the Objection that it ſuppoſes the Concave of the Tubes of the Thermometers to be Cylinders, or of equal Diameters throughout; and alſo that on account of Heat and Cold the Air and Spirit have a proportional Dilatation and Contraction; the firſt of which I take to be very hard to be found in ordinary Glaſs Canes, and the other I fear ſtill wants to be made out by authentick Experiments.

The laſt contrivance for this purpoſe. is that of Mr. *Patrick*, who ſtiles himſelf the *Torricellian Operator*, by filling a ſmall Glaſs Cane about five Foot long, and ſomewhat, but as little as may be, tapering upwards towards the cloſe end of the Cane; then inverting it, without a ſtagnant ciſtern of *Mercury*, ſo much of the *Mercury* as exceeds the length of the Column the Atmoſphere can then ſupport, will drop off, and leave its length equal to the then preſent height of the common Barometer: now when the Barometer riſes, this length in the Cane becomes greater by the *Mercury*'s being preſt up into the upper and narrower part of the Tube; and when it falls, on the contrary, it ſettles down into the wider part thereof, and becomes ſhorter, being always the ſame in Quantity. By this means, as the Angle of the Concave Cone of Glaſs, of which this Tube conſiſts, is ſmaller, the different ſituation of the *Mercury*, will, upon the alteration of the Airs preſſure, be nicely ſhewn by very large and diſtinct diviſions.

Now the uſe I would apply this contrivance of the Barometer to, is to meaſure by it the different Levels of Places too remote to be come at by the ordinary Inſtruments for levelling, with the certainty one would deſire. For this purpoſe let there be provided two ſmall Glaſs Canes, as near as can be ſimilar, growing very little taper or ſmaller at the cloſed end, ſo that being inverted, the *Mercury* may be ſuſpended in them at the height it ought to have at the time of the Experiment. Let that height be duly noted, and then aſcending the Monument, or ſome ſuch Edifice where the aſcent may be exactly meaſured, let the Scales annexed be divided into Parts by the deſcent of the *Mercury*, at every ten Foot, in both the pendent Barometers, which I conceive may be ſo choſen as to make the Diviſions very diſtinct and ſenſible. Theſe thus prepared, when it is deſired to take the Level of two diſtant Places, let one of them be placed in the lower Place, at a time when the *Mercury* has the ſame height as when they were firſt inverted and graduated; and let the other be carried to the higher Place, where it will be found to ſtand at that diviſion which anſwers the elevation of that Place above the other, the which had before been found by meaſure in aſcending the Monument. Thus may 90 Foot aſcent, which makes but one tenth of an Inch of *Mercury*, be repreſented by two or three Inches, or a ſpace capable of being divided into 90 Parts: Whereas, if the diſtance of the two Places be 20 Miles, a minute of a Degree is equal to above 30 Foot; and by the uſual Sights, whether Teleſcope or otherwiſe, of your Water Levels, I fear it will be very hard to convey a true Level without a greater Error than one Minute in the whole. This Propoſal I humbly ſubmit to the Examination of this Honourable Society.

X. *Obſervations upon the Variation of the Needle made in the* Baltick, Anno 1720. *By Mr.* William Sanderſon. *Communicated by Capt.* Soame.

W*Edneſday, June* the Firſt 1720, being at Anchor near *Revell,* in the Latitude of 58° 58′ *North*, the Magnetical Amplitude at Sun-Set was
Weſt——64° 30 *North.*
And the True was *Weſt*——49 37 *North.*
Variation *North* ——14 53 *Weſt.*

Saturday, July 23, at the Iſle *Gottſand,* in the Latitude of 58° 21′ *North,* at Sun Set, the Magnetical Amplitude was *Weſt* ———— 49° 50 *North*, and the True Amplitude, *Weſt* — 35 00 *North*, which gives the Variation *North* 14 50 *Weſt.*

The Difference of Longitudes of the two foreſaid Places by *Dead Reck.* is 1° 50.

October the 24th at *Bornholme,* in the Latitude of 56° 00′, at Sun Riſing, the Magnetical Amplitude was
Eaſt 43° 15′ *South,* and the True was
Eaſt 28 31 which gives the Variation
North 14 44 *Weſt.*

XI. *An Account of a large Quantity of Alcalious Salt produced by burning rotten Wood. By Mr.* Robie *of* Harward College *in* Cambridge, New-England. *Communicated by the Rev. Mr.* Derham, *Prebendary of* Windſor, F. R. S.

A White Oak Tree, about two Foot Diameter, is in *Cambridge*, of ſo wonderful a nature, as that although about a third of it was decay'd, and ſeem'd really to be rotten Wood, yet this decay'd part, in burning, would turn almoſt wholly into a good white *Alkali*, and it would run down into hard Lumps white and clean. Such a Lump was brought us at College, to know what it was. We taſted it, and found it to be Salt. and very ſtrong. We diſſolved it in clean Water, and upon Decantation and Evaporation, without any Filtration, we produced a very clean, white Salt, exceeding in ſtrength, and whiteneſs, any to be bought at the Shops. We tried it many of the ways of proving the goodneſs of an *Alkali*. Now although *Alkali*'s may be extracted from common Aſhes, yet what was peculiar in this, is,

1. That while it was Burning, the Wood it ſelf would melt, and run down into hard Lumps of Salt; and none of the Wood that was found, would do this, but only that which was decay'd; and what was moſt decay'd, would yield the greateſt Quantity of Salt. And,

2. Whereas all other *Alkali's* of Wood made thus by Incineration, are blackiſh at firſt, and a *Lixivium* made of them, although often filtred, will yet be tinged with a brown colour, occaſioned from a kind of Coal, or Aſhes ſo incloſed, or cloſely united to the *Alkali* in burning, as not eaſily to be ſeparated by Filtration, though often repeated; yet this *Alkali* was very white, even before Solution, and when diſſolved, the *Lixivium* was not in the leaſt tinged, but clear like pure Water, only a very ſmall quantity of Aſhes ſubſided to the bottom of the Veſſel, in which the Solution was made. The *Lixivium* thence decanted needed no Filtration, but when boiled up to Dryneſs, the Salt remained fine and white. And

3. That in the burning of this Wood, as the heat of the Fire grew more intenſe, the Wood did, as it were, melt and clodder together in great Lumps, and did viſibly bubble, and boil, with an hiſſing noiſe, like the Frying of Fat in a Pan.

4. That whereas the Weight of the *Alkali-Salt* produced from other Wood, in the common way of Incineration, is very inconſiderable, in proportion to the weight of the Wood producing it; yet this Salt nearly equalled in weight the Wood from whence it was taken.

5. Whereas the Aſhes of other Wood are never ſo replete with Salt, as that Salt can be ſeen, or in the leaſt cauſe the Aſhes to lump or clodder together; yet this, the whole of it, would gather into hard and ſolid Lumps of white Salt, as eaſily to be diſtinguiſhed from Aſhes (tho' white) as the pureſt Salt of *Tartar* made with *Nitre* would be.

6. That although from other *Rotten Wood* much leſs of an *Alkali* can be produced, than from *Sound Wood*, yet here it is quite contrary, the decay'd part of this

Tree yielding, in quantity, as aforeſaid; and the other, or ſound part, yielding no more than other Wood.

Having thus given you a true and full Account of this ſtrange and unuſual Production, we ſhall give you our Thoughts reſpecting the Solution of it; which we ſhould not attempt, but that being on the ſpot, we have examined the Tree, and conſidered what (by the Marks found on it) hath, in all probability, happened to it; and therefore ſuppoſe ourſelves, in ſome meaſure, capable of giving as true or truer Judgment concerning it, than wiſer and more ingenious Men can be, who have not had thoſe Advantages. All which we do with humility and modeſty ſubmit to your Cenſure. And we would firſt premiſe, that the Tree, in all probability, was ſtruck with Lightning many Years ſince, it being torn from the top of its Trunk to the bottom, on that ſide, which is now decay'd, and which yielded the aforeſaid Salt, there being a Channel from top to bottom, about five Inches wide, as we ſuppoſe, at firſt, which the length of time had cloſed And under this Bark, the Wood next to it was black; which we ſuppoſe was cauſed by the Lightning.

From which things we conjectured, that the Wood having been thus expoſed to the Air and Water, for ſo long a time, this was the occaſion of its becoming defective in that part; and that the Lightning having penetrated the Wood, had ſo altered and diſpoſed the Parts and Pores of it (the Figure and Texture of the Parts appearing much different from other rotten Wood) to attract, receive in, and retain the *Nitrous* Salt of the Air, which through ſo long a ſpace of time, could not but be in great abundance. Even as Salt of *Tartar*, or other *Alkali's*, being expoſed to the Air for ſome conſiderable ſpace of time, will be wholly reduced to a

Nitrous Salt, (as *Glauber* ſays) and its Quantity alſo increaſed very conſiderably. Not that the Lightning had ſo calcined the Wood, as to reduce it to a perfect Salt, but yet by penetrating of it, it had calcined it in ſuch meaſure, as to give it a like propriety or diſpoſition, of attracting the *Nitrous Salt* of the Air, as aforeſaid *Alkali's* of Wood that have been fully calcined.

Now if it ſhould be objected, that the *Nitre* in this Wood being *Volatile*, would flee away in the burning of the Wood; we anſwer, That although *Nitre* can't be fixed, and reduced to an *Alkali-Salt*, by calcining it *per ſe*; yet it may ſo by the addition of the powder of Charcoal (as Chymiſts teach us.) And here we ſuppoſe the Wood ſo altered by Lightning, in which this *Nitrous Salt* was lodged, as ſerved inſtead of Coal in the Burning of it.

FINIS.

LONDON:
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PHILOSOPHICAL TRANSACTIONS.

For the Months of *January*, *February*, *March*, and *April*, 1721.

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I. *An Account of a Fœtus, that continued* 46 *Years in the Mother's Body. Communicated by Dr.* Steigertahl, *Honorary Fellow of the College of Phyſicians, and Fellow of the* Royal Society.

ANna *Mullern*, a Woman of the Village of *Leinzelle* near *Gemund* in *Suabia*, of a dry and lean Conſtitution, but otherwiſe healthful and robuſt, died at the Age of 94, after ſhe had lived a Widow 40 Years.

Six and fourty Years before her death ſhe declared herſelf to be with Child, and had all the uſual Tokens of Pregnancy. At the end of her reckoning the Waters came away, and ſhe was taken with the pains of Labour, which continued upon her about ſeven Weeks, and then went off upon the uſe of ſome Medicines given her by a Surgeon. Some time after this ſhe recovered her perfect Health, except only that her Belly continued ſwell'd, and that now and then upon any Exerciſe ſhe felt a little pain in the lower part of it. She was after this twice brought to bed, the firſt time of a Son, who is now a Huntſman at *Biſchoffſhein*, and afterwards of a Daughter, who is married to a Soldier. But notwithſtanding this, ſhe was firmly perſuaded, that ſhe was not yet delivered of what ſhe firſt went with, and deſired Dr. *Wohnlixe* the Phyſician of *Gemund*, and one *Knauſſen* a Surgeon at *Heubach*, to open her Body after her death. Accordingly after her Death, which happen'd on the 11th of *March* 1720, after four days illneſs, her body was open'd by the Surgeon, the Phyſician before-mention'd being then dead. He found within her a hard maſs of the form and ſize of a large Ninepin-Bowl, but had not the precaution to obſerve, whether it lay in the *Uterus*, or without it, and for want of better Inſtruments broke it open with a blow of a Hatchet. This Ball with the Contents of it, are expreſſed in the following Figures.

Plate I. Fig. I.

A A part of the Tegument, which had adher'd to a ſpongious fleſhy Subſtance. This at firſt ſeem'd to be a Maſs of Cartilage, but was afterwards found to be entirely bony.

B. *B*. *B*. *C*. *C*. *C*. Shews the membranous Part, which was bloody.

D. *D*. *D*. *D*. The opening made by the Hatchet.

E. *E*. *E*. *E*. *E*. Another part of the Tegument appearing entirely bony, with ſeveral prominences.

F. A Contuſion occaſion'd by the rude manner of opening it, where there appear'd ſome putrified Membranes.

Fig. II. *A*. *A*. *A*. The Tegument, or Subſtance encloſing the *Fœtus*.

B. The *Fœtus*.

C. A depreſſion, or hollowneſs on the right Cheek.

α. The Noſe turning up.

β. The Mouth flatted, but not ſo wide, as it is here repreſented.

γ. The Eye cloſed up.

δ. The Ear.

D. *D*. The Arms, of which the right was the larger, and the two Joints of that Thumb were plainly to be ſeen.

E. The protuberance of the Knee.
F. Part of the *Funiculus Umbilicalis* torn, but ſtill adhering to the Navel.
G.G. Part of the ſame *Funiculus*, faſten'd to the bony part of the Tegument.
H. The Breaſt.
I. The mark of an Inciſion into the left ſide, where the Fleſh appear'd red, but was dry, and look'd like ſmoaked Beef.

Fig. III. Repreſents two Ribs from the left Side, of their natural Subſtance, colour and bigneſs.
A. A. The part joining to the *Vertebræ.*
B. B. to the *Sternum.*

This Piece is preſerv'd in the Cabinet of Rarities of his moſt Serene Highneſs the Duke of *Wirtemberg.*

Dr. *Camerarius*, Profeſſor at *Tubingen*, in a Letter upon this Subject, takes notice, that the Surgeon found this Maſs in a cavity on the Woman's left ſide, and that it adhered to the Membranes of that cavity by the intervention of a ſpongious, fleſhy Subſtance. From which particular, and the Woman's having had two Children during the time that this large Maſs lay in her, that learned Profeſſor conjectures, that it was not lodged in the Womb, but in the left *Fallopian Tube*, which by this means had been very much dilated and thickned in its Subſtance.

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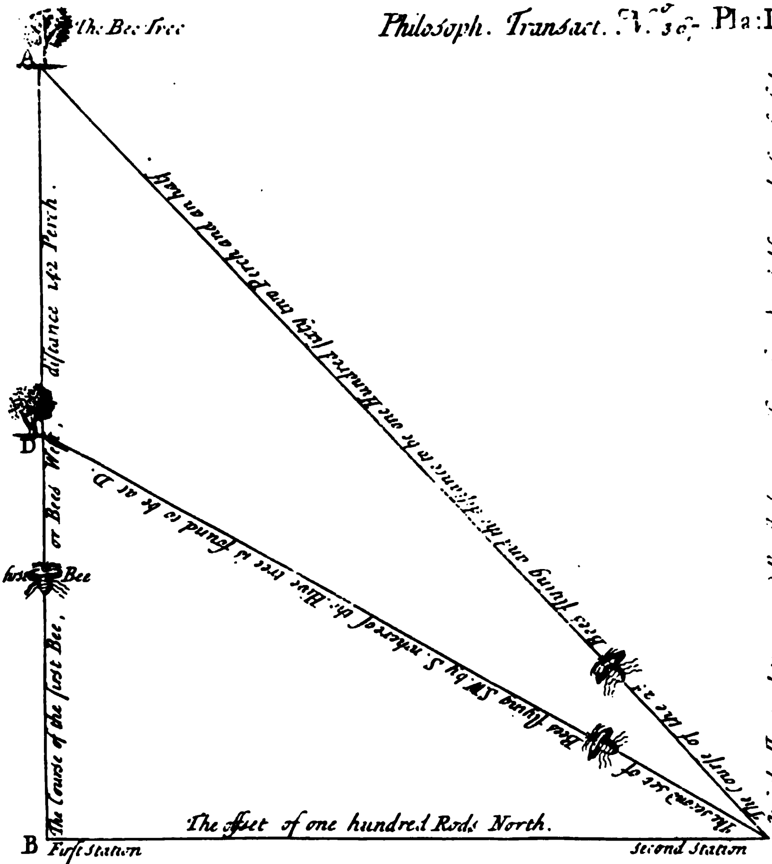
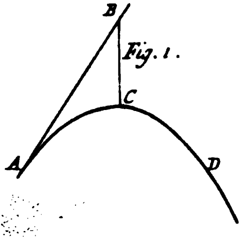

The Bee Tree
A
D
B
C
firſt Bee
The Courſe of the firſt Bee, or Bees West, distance 142 Perch.
The Courſe of the 2d Bees flying and the distance to be one Hundred ſixty two Perch and an half.
The ſecond ſet of Bees flying SW. by S. whereof the Hive tree is found to be at D.
The offſet of one hundred Rods North.
Firſt Station
Second Station

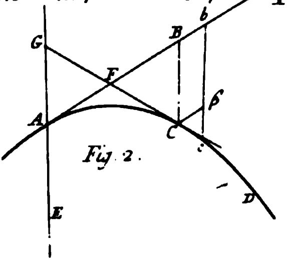
At C the Hunter lets a ſecond Bee fly (or more if neceſſary) and obſerves the Courſe of their flight and finds it to be South 54 degrees West.

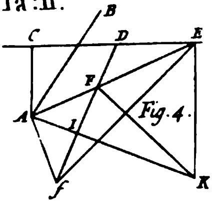
At B the Hunter ſtands and having with his Sugar &c. procured one or more Bees, obſerves the Courſe of the firſt Bee & finds it to be due West.

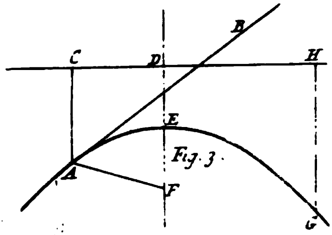
N. B. If the Hunter is not ſatiſfied of the Courſes of (either firſt or ſecond) by the flight of one Bee, he lets more fly untill he is very certain.

Philoſoph: Tranſact: N° 367 Pla: II.









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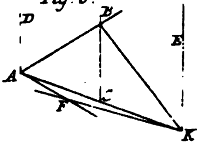

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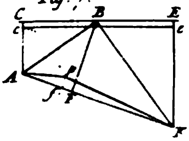

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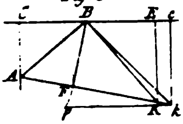

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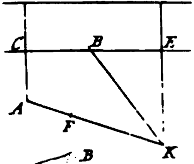

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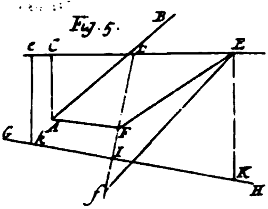

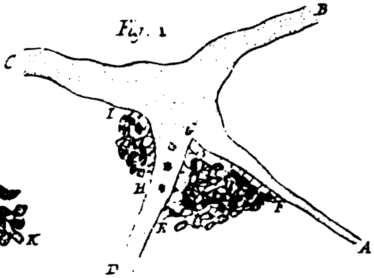

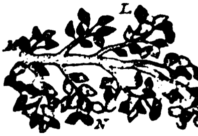
Fig. 2.

be driven out of theſe Veſſels; for all the Particles of Fat, that I have as yet obſerv'd, are incloſed in ſmall Films.

This Original of the Fat is to me much more credible, than that it ſhould be forced out of the Blood Veſſels; and yet how theſe fatty Particles, which conſiſt of ſmall globules, and thoſe out of ſtill ſmaller globules (as it appears to me) are made and formed, I cannot as yet determine: As alſo where theſe Veſſels, which conſtitute what we call Membranes, have their beginning, and how this Fat is brought into them.

I had in my Drawer a piece of Oxe's-fleſh, that I believe had lain there about four Years, wrapp'd up in a Paper, which Piece I found in ſome places to be cover'd with a Membrane; from this I cut off ſeveral ſmall Slices along with the Membrane, and I found that near the Membrane, there lay about 16 or 18 Nervous Fibrils, which, in the drying of the Fleſh, were ſo ſqueez'd together, that they were almoſt twice as long as they were broad. In ſome of which I ſaw very diſtinctly thoſe Veſſels, which are in the Nerves.

Theſe Nervous *Fibrillæ* were encloſed by a ſort of half round, ſeparating them from the Muſcular Fibres, which half round conſiſted of a row of ſmall Tendinous *Fibrillæ*, each of which was about twice as thick as a Hair of a Man's Beard. Without theſe Tendinous *Fibrillæ* lay the Muſcular Fibres, that had been cut through tranſverſly, and in this part of the half round there were ſeveral Apertures, which ſeem'd in the Microſcope to be big enough for Hemp-ſeed to paſs through them, which might well be taken for Veſſels, but that there lay ſo many of them together. But conſidering that the Nerves are commonly cover'd with fatty Particles, I concluded that theſe Apertures were no Veſſels, but meer fatty Particles, which I found to be true when I had cut thro' them, and diſcover'd that the inward Fat was eaten out by the Mites, which had left only the Huſks, or *Cortices*, of the Fat Globules behind: Which *Cortices* I never had as yet been able to diſcover, becauſe the *Cortices* of the Fat Globules would, upon any heat, melt away as faſt as the inward Fat.

III. *Obſervations upon the Veſſels in ſeveral ſorts of Wood, and upon the Muſcular Fibres of different Animals. By the ſame Curious and Inquiſitive Perſon.*

Delft, Jan. 24. 1721.

SEEING ſome reddiſh Boards carry'd into a Houſe in my Neighbourhood, and enquiring what uſe they were deſign'd for, I was anſwer'd that they were bought of the *India* Company to make Cabinets of, and that they came from the Iſland *Amboina*.

I procured a piece of that Wood ſaw'd off at the end of a Board, as likewiſe ſome of the Chips, in order to obſerve the Veſſels therein; and, cutting the Wood through all manner of ways, I found that in one place the Wood appear'd whitiſh, that at a ſmall diſtance it was red, and in another place blackiſh. Upon cutting it tranſverſly, I ſaw the Orifices of the aſcending Veſſels, which ran along the length of the Wood, and which appear'd of ſuch a ſize in the Microſcope, that one would have judged a Pea might paſs thro' them. Where the Wood look'd reddiſh, I found theſe great Veſſels fill'd with a Subſtance of a fine red colour, ſo that I imagin'd, that theſe great Veſſels carried a red Sap into the Horizontal Veſſels, which appear'd ſo very numerous, and ſo thick together, that they caus'd the Wood to appear of the ſame colour with the red Subſtance, which was contain'd in thoſe Veſſels likewiſe.

I afterwards cut off ſome very thin ſlices tranſverſly from this Wood, and putting them into a *China* Cup, I pour'd ſome hot Water upon them, and ſuffer'd them to lie in it for ſome time; then viewing them with a Microſcope, I obſerv'd that the red Subſtance was extracted by the Water, and no red colour was now to be found in any of the Veſſels.

What ſeem'd the ſtrangeſt to me in this Wood, was that cutting thro' the Wood lengthways, as I frequently did, I obſerv'd it to be of a fine red colour for one Hair's breadth, and a Hair's breadth farther it appear'd white; and the aſcending Veſſels ſeem'd to be ſmaller, where the Wood was red, than where it was white: which narrowneſs of the red Veſſels I judged to proceed from the Sap contain'd in them.

I made ſeveral other Remarks upon this Wood, which I ſhall paſs by at this time, and ſhall proceed to ſome Obſervations I have made upon the aſcending Veſſels in Oak, and other Wood.

In viewing the aſcending Veſſels in Oak, I found ſome other Veſſels, which enter'd into their ſides, and appear'd to me like ſo many ſmall round holes, eſpecially where the Horizontal Veſſels lay, which I judged to be united to the aſcending Veſſels, by means of thoſe ſmall Orifices, and thereby to diſcharge part of their Sap into them.

Taking a ſmall Twig of an Oak, which in ſeven Years growth was grown to about the thickneſs of ones Finger, I cut it thro' according to the length both of the aſcending and horizontal Veſſels, which laſt I ſaw lying in great Numbers very cloſe together, and proceeding directly from the Pith of the Twig.

I have likewiſe made ſome Obſervations upon Fir Wood, in which the aſcending Veſſels conſiſt of ſo very fine and thin a Subſtance, that they exhibit a very delightful Spectacle in the Microſcope. In theſe aſcending Veſſels I imagin'd that I ſaw ſome Globules, with a ſmall opening in their middle, which ſeem'd to be of a cloſer and denſer Subſtance than the reſt of the Wood. But I afterwards found myſelf miſtaken, and that theſe ſuppoſed Globules were nothing elſe but the Orifices, whereby the aſcending and horizontal Veſſels were united together, and through which the Sap was carry'd from the one to the other.

From theſe Obſervations I turn'd my Thoughts to the fleſhy Fibres of Animals, and began to conſider with my ſelf, that, ſince the Author of Nature uſually obſerves the ſame Frame and Structure in a great variety of his Creatures, perhaps the fine Membranes, with which every Muſcular Fibre is inveſted, and which are provided with an innumerable

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II. *Obſervations upon the Membranes encloſing the* Faſciculi *of Fibres, into which a Muſcle is divided. By Mr.* Leeuwenhoek, F. R. S. *Tranſlated by Dr.* Sprengell, *F. R. S.*

Delft, Jan. 9. 1720.

SOmetime ago I miſs'd a ſmall piece of Fleſh, which I had left ſtanding before a Microſcope, and endeavouring to procure ſuch another, I cut off ſeveral thin ſlices from a piece of Beef; in doing which, whenever I cut the fleſhy Fibres thro' tranſverſly, I could plainly diſcover the Membrane, as it is commonly call'd, which runs between and envelopes the fleſhy Fibres, and eſpecially the larger *Faſciculi* of them, as they run lengthwiſe along the Muſcle. Between theſe *Faſciculi* the Membrane is of a conſiderable thickneſs, but ſpreads out every way into Ramifications exceedingly ſmall. I had obſerv'd ſome Weeks before, that this Membrane was compoſed of an inconceivable number of very ſmall Veſſels, which were plainly to be diſcern'd not only where the Membrane appear'd of ſome conſiderable breadth, but even where it was not ſo broad as a ſingle Muſcular Fibre; but how far this held, I could not determine, foraſmuch as theſe ſmall Ramifications of the Membrane, did again ſpread themſelves into other Ramifications ſo exceedingly fine, eſpecially where they encloſed the ſingle Muſcular

Fibres, that they were in a manner inviſible even through my beſt Microſcopes.

The very ſmall Veſſels, which compoſe this Membrane, (as it is call'd) are doubtleſs framed to convey ſome nutritious Juices, yet they are ſo ſmall, that the Globules of Blood cannot paſs through them.

That this might be the better underſtood, I cauſed a ſmall piece of the Membrane to be deſigned, as at Fig. 1. *A. B. C. D.* which, with the adjoining fleſhy Parts, is cut thro' tranſverſly, and ſince it was impoſſible for the Deſigner to draw the extraordinary number of Veſſels, which compoſed it, on account of their being ſo exceeding ſmall, he has repreſented them only by Points.

Altho' in my former Letters I gave ſome Draughts of the Carnous Fibres cut thro' croſs-way along with the Membranes, yet for the ſatisfaction of thoſe who have not ſeen my other Letters, I have given them here another Draught of the ſame. See Fig. 1. between *E. F. G.* and *H. I.*

Theſe carnous Fibres, when wet, lay ſo cloſe to each other, that the ſpace between *E. F. G.* and *H. I.* was quite fill'd up; but when dry'd, the Fibres were ſo ſhrunk, that one might ſee ſuch Spaces between them as are here delineated.

Now as we ſee, upon the drying of the Membranes *A. F. G.* and *D. E. G.* with the Muſcular Fibres between them, what a number of ſmall Ramifications proceed from the Membranes, as is here repreſented between the muſcular Fibres; we muſt not imagine, that theſe Ramifications proceed only from the points here repreſented, but that they are continued the whole length of the Fibres, and ſubdividing themſelves into ſtill finer Ramifications, they encloſe every ſingle Fibre in the whole Muſcle.

Amongſt ſeveral pieces of Fleſh, where the carnous Fibres were cut tranſverſly, I happen'd on one piece with its Branches ſo plain, that the Membranes and Fibres look'd like ſo many Boughs of Trees, with the Leaves on them, as may be ſeen at Fig. 2. *K. L. M. N.* where *M.* ſhews the ſo call'd Membrane torn off from another, as alſo how many Branches it runs into, and the many Fibres it covers.

All theſe carnous Fibres, with the ſo call'd Membranes, lay very compact together, when I cut them off from the piece of Fleſh, as likewiſe when I laid them on the Glaſs, and moiſtened them; but as the moiſture dry'd away, they ſhrunk again, in the manner here repreſented, and altho' the Deſigner could plainly diſtinguiſh the ſmall Veſſels which were cut thro', the largeſt of which appear'd at *M.* Fig. 2. yet he was obliged to mark them only with Points. Here you may obſerve, that all the carnous Fibres, having been cloſely tied together by the ſaid Membranes, by which they were enveloped, which are nothing but a congeries of Veſſels, could not be ſeparated from each other upon drying, but by tearing aſunder thoſe Membranes.

The carnous Fibres along with the ſo call'd Membrane, at Fig. 2. *K. L. M. N.* do not take up ſo much room, but that a grain of Sand may cover it, and yet one might very diſtinctly obſerve, in ſome of thoſe carnous Fibres, the parts of which they were compoſed.

This Obſervation I was reſolved to purſue in the Fleſh of a Whale, of which I had kept two pieces by me, for about 7 or 8 Years, of about a Span long, and two Inches thick; from theſe I cut ſeveral Slices tranſverſly, but found that the carnous Fibres ſo cut through did eaſily ſeparate from one another, ſo that I could not find my account in this, but thought that the Membranes were rotten. Therefore I did cut off the outſide with a Table-Knife, and then with a very ſharp Knife I cut the inner part into very fine Slices, and there I found the Excrements of Mites, which were very ſmall, but globular, and ſome of them as ſmall as I had ever ſeen before, and ſo going on, I found theſe Excrements every where, eſpecially where the Membranes were thickeſt; then looking into ſuch places where the Membranes were thinneſt, inſomuch that I was ſatisfied, that a Mite, tho' juſt come from the Egg, could hardly find room there, there it was that I diſcover'd in the ſo nam'd Membranes, the aforeſaid Veſſels, and that in as great a number as I had ſeen them in the Oxe's Fleſh, and as plain as one can ſee the holes in a Thimble with the naked Eye.

After the former Diſcoveries that I had made concerning the Circulation of the Blood, particularly that the Blood Veſſels had no Endings, I began to conſider how the Fat-Particles could be formed, ſince I did not think that they were ſeparated from the Blood, and came out of the Blood-Veſſels. But having now plainly diſcover'd, that the ſo call'd Membranes were nothing but very ſmall Veſſels, and believing that they were created for no other end but to tranſport Nutriment, as alſo that there was no Circulation in theſe Veſſels, I imagin'd that the Matter which we call Fat, was brought into them, which, when there was too great a ſupply of Nutriment, ſo that it could not be forced farther on, muſt

be

rable multitude of ſmall Veſſels, might carry Nouriſhment in the ſame manner, thro' every carnous Fibre in a healthful Body.

In this view I cut off ſome very ſmall thin Slices from the fleſh of an Ox, directly acroſs the length of the Fibres, and having placed them upon Glaſſes, and moiſtned them with clean Rain-water, I obſerv'd them with a very good Microſcope, and continued viewing them ſo long, that the fleſhy Fibres began to grow dry. I then ſaw, that in ſome Places the exceeding ſmall and fine Veſſels, which compounded the Membranes, wherewith the fleſhy Fibres were encloſed, were broken off from the fleſhy Fibres, by the unequal ſhrinking of the thin ſlice of Fleſh upon the Plate of the Microſcope. I ſaw at the ſame time ſome other of theſe ſmall Veſſels, which were ſomething ſtronger than the former, and were not broken off from the fleſhy Fibres, but yet were ſtretched and drawn from them to the diſtance of the Diameter of a Blood Globule. I ſaw likewiſe ſome fleſhy Fibres, which adhered ſo cloſe to other Fibres, that the ſmall Veſſels of Communication were not broken off or ſtretched, ſo that nothing was to be ſeen there, but only the Membrane encompaſſing the Fibres.

I likewiſe placed before the ſame Microſcope ſeveral other carnous Fibres, which I had ſeparated according to their length from the Fleſh of an Ox. In each of theſe I obſerv'd a great number of extreamly ſmall Apertures, by which I judged that the ſmall Veſſels of the Membranes had enter'd the Fibres; which Veſſels having been moiſtned with Water, as ſoon as the little moiſture, which had been left in thoſe Apertures, was evaporated, I could ſee them very plain and diſtinct.

Now ſince of late there have been two Perſons of note, who have maintain'd, that the Blood circulated thro' the carnous Fibres; in order to examine into the Truth of this Hypotheſis, I prick'd my Thumb with a fine Needle, and placed a little Blood upon the Glaſs, where the carnous Fibres lay, with deſign to obſerve with my Microſcope, what was the proportion between the Diameters of a Globule of Blood, and of the abovefaid Apertures, which I had ſeen in a Fibre.

While I was employ'd in theſe Obſervations, in came my Painter, who for theſe many Years has drawn all my Diſcoveries, and not being willing to truſt too much to my own Eyes, he being much younger and better ſighted than I, I placed before him the ſmall Apertures in the Fibres, which he ſaid were plain enough to be ſeen, and when he had view'd them to his ſatisfaction, I placed before him likewiſe the Globules of Blood, which lay together in great numbers, and yet ſo diſtinct and ſeparate one from another, as one ſhall ſeldom ſee them. I then ask'd him, what he thought to be the proportion between the Diameter of a Globule of Blood, and the Diameter of one of thoſe Apertures in a fleſhy Fibre. After a little pauſe, he gave me for anſwer, that the Diameter of a Blood-Globule, was four times as large as the Diameter of one of thoſe Apertures. If ſo, then according to the known Rule, a Globule of Blood muſt be divided into 64 parts, before it can enter through one of theſe Apertures into a fleſhy Fibre.

This diſcovery appear'd to me very wonderful; and I am apt to think, that it will be very difficult to penetrate any deeper into the hidden Structure of the muſcular Fibres, and the manner by which they receive their Nouriſhment. I muſt confeſs that this Obſervation gave me an inward pleaſure and ſatisfaction, which made me amends for ſome Reflections lately thrown upon me by a certain Foreign Gentleman, who, if this ſhould come to his knowledge, would perhaps write to me again, as he did once before, that I related things which no Eye had ever ſeen.

Having committed theſe things to paper, my Thoughts ran again upon the muſcular Fibres; and conſidering what I had ſaid formerly concerning them, that they were compoſed of long, ſmall Filaments, now I was not ſatisfy'd therewith, and therefore endeavour'd to diſcover, whether theſe ſmall Filaments, which compoſe a carnous Fibre, might not really be ſo many ſmall Veſſels. With this deſign, I took part of the fleſh of a Whale, which I had kept ſome Years by me, and cut it into very thin Slices directly acroſs the Fibres, and having moiſtned theſe thin Slices with fair Water, I placed them upon ſeveral Glaſſes, and before ſeveral Microſcopes, when I obſerv'd that what I had formerly taken for ſmall Threads or Filaments, were in reality exceeding ſmall Veſſels. I then cut part of the Whale's Fleſh lengthwiſe, in order to diſcover the Veſſels, which convey the nutritious Juice out of the Membranes into the muſcular Fibres, which Veſſels then appear'd to me in great plenty and very diſtinct.

I afterwards took another piece of the fleſh of a very fat Ox, which I cut thro' tranſverſly, and looking upon it with ſome of my beſt Microſcopes, I could plainly ſee, that how ſmall ſoever theſe Fibres were, they were ſtill vaſcular, for I could ſee the light thro' the apertures of theſe Veſſels, as I had done before in thoſe of a Whale; but if I happen'd to cut the Fibres never ſo little obliquely, inſtead of cutting them directly acroſs their length, the light was not to be ſeen thro' them.

I had in a Drawer the hind Quarter of a Mouſe, which had lain there ſome Years; from the largeſt Muſcle of which I cut off tranſverſly ſome ſmall Slices, as thin as poſſibly I could. Then placing theſe before my Microſcope, I not only ſaw, that the carnous Fibres were of the ſame thickneſs with thoſe of an Ox, but beſides I could ſee the apertures of the Veſſels compoſing the carnous Fibres, as plainly as in the fleſh of a Whale. The Veſſels in the Muſcular Fibres of a Whale, are indeed ſix times more in number, than in thoſe of an Ox, or a Mouſe, but then the Fibre of a Whale is alſo ſix times as thick as the other.

Hereupon I conſider'd after what manner the Veſſels, of which the muſcular Fibres moſtly conſiſt, receiv'd their nouriſhment from the Veſſels of the Membranes; ſince the muſcular Fibres, when they are at reſt, have many alternate corrugations, by which I judged, that the Veſſels in the Fibres muſt have their ſides preſs'd together, and their cavities cloſed up. But if we call to mind, that in walking a Man may move both his Feet above 3600 times in an Hour, (for he may make two ſteps in the time of one pulſation of the Artery,) and that in that ſpace of an Hour the muſcular Fibres muſt be ſo many times extended and contracted, and will therefore require great Supplies; we ſhall likewiſe find that this is ſufficiently

ciently provided for, ſince upon every extenſion of the Muſcle the apertures of thoſe ſmall Veſſels are free and open for the carriage of Nouriſhment into the Fibres. This wonderful ſtructure of the Membranes, and the vaſt number of Veſſels they conſiſt of, as likewiſe the ſmall Veſſels of which the muſcular Fibres are compoſed, has never yet to my knowledge enter'd into the Thoughts of any Man, and with many will hardly find Credit. Perhaps they will ſay, they have as good Glaſſes as I, and yet cannot ſee what I have related.

I am, &c.

P. S. I humbly deſire that what I have here ſaid, may be receiv'd with your uſual Candour, and that a favourable allowance may be made for my great Age. A certain Gentleman, who was with me ſome Months ago, intreated me to go on in making Obſervations, adding that the Fruit which ripen'd in Autumn, was the moſt laſting. This is now the Autumn of my Life, I being arriv'd to the age of 88 ½ Years, and having committed theſe Obſervations to Paper in the Months of *November* and *December* laſt.

IV. *Experiments relating to the Reſiſtance of Fluids, made before the* Royal Society *on* Thurſday, March *the* 30*th*, 1721. *By the Reverend* J. T. Deſaguliers, LL. D. *F. R. S.*

I Took a Ball of Gold of an Inch in Diameter, that had a little Stem of the ſame Metal, with a place on it to faſten a String to; and having ſuſpended it by a ſilken Thread too ſtrong to lengthen by ſtretching, I made the Diſtance between the Center of the Ball, and the Point of Suſpenſion equal to 12,5 Inches, then cauſing the Ball to vibrate in a Trough full of Water, (which had an upright Piece of Wood in the middle of one ſide with Pins or Keys from which the Ball hung, that the Center of Suſpenſion might always be in the ſame place) I obſerv'd by looking from a Pin on one ſide of the Trough to a mark made oppoſite to it on the other ſide, whereabouts the String of the *Pendulum* (juſt above the Surface of the Water; in which the Ball was quite immers'd) went after 14 Vibrations; and by another Pin and oppoſite mark, alſo obſerv'd where it went to, after 28 Vibrations. Taking out the Water, I fill'd the Trough with Mercury, the length of the *Pendulum*, Point of Suſpenſion and all other things remaining as before: then letting go the Ball in the Mercury from the ſame place whence it was let down when the Trough was full of Water; (which was mark'd by a String ſtretched a croſs to prevent miſtakes) after one whole Vibration, it came very little ſhort of the ſame mark as it had come to in Water after fourteen Vibrations, and when it vibrated twice in Mercury, it came to the ſame place it had done after between 26 and 28 Vibrations in Water; and this it did exactly ſeveral times.

Afterwards filling an upright Copper Pipe of four Inches Diameter with Mercury to the height of 3 Foot 10 Inches, and ſuſpending the golden Ball in it by a ſhort String about an Inch long, ſo as to have the Ball juſt immers'd under the middle of the ſurface of the Mercury; I caus'd it to be let down ſuddenly, and obſerving how long it was falling down to the bottom of the Tube, I found that the Experiment was diſturb'd by the Ball's ſtriking againſt the ſides of the Tube, which retarded the fall of the Ball, and the more ſo the oftner the Ball ſtruck. When the Ball was leaſt retarded, it was only two Seconds and a half in falling, which muſt be taken as the true time of the fall of the Ball in an height of Quickſilver equal to 3 Foot 10 Inches; becauſe when I try'd the Experiment again at home the firſt Day of *April* following, the Ball fell in the Mercury once or twice without ſtriking the ſides of the Tube at all, but not in leſs time than 2 $\frac{1}{2}$ Seconds.

I alſo repeated the other Experiments at home, making the Golden *Pendulum* 39,2 Inches long, ſo as to make it vibrate but once in a Second, and then I found that it wou'd vibrate 5 or 6 times in the Mercury before the Vibrations became ſo ſmall as not to be obſerv'd; and then the firſt Vibration in the Mercury ended very near where the 14th in Water had done; the ſecond in Mercury ended where the 27th in Water had done, and obſerving the third Vibration in Mercury, it ended exactly at the mark where the 40th in Water ended; and this was obſerv'd by ſeveral Perſons as well as myſelf.

Then I weigh'd 14 penny-weight of the Mercury (in which I made the Experiments) firſt in the Air, then in Water, where it loſt only one penny-weight and one Grain of its Weight; that is, it weigh'd in Air 336 Grains, and in Water 311, ſo that its ſpecifick gravity was to that of Water as 13,44 to 1.

As to the golden Ball which had Varniſh and Cement upon it to keep the Mercury from ſinking into it, I found it to weigh as follows,

	Ounces	dwt.	gr.	
It weigh'd in Mercury	1	00	18 or	498 gr.
in Water	5	01	00 or	2424 gr.
in Air	5	07	09 or	2577 gr.

I took the Wire and *Pendulum* of a long *Pendulum* Clock, and having faſten'd the golden Ball at the end of the Wire under the pendulous Weight that ſerv'd for the Clock, in order to make the Vibrations of the golden Ball in the Mercury continue longer, I did not find it to keep on the motion above one ſwing or two the longer for that help; neither did a round Ball of Lead placed upon the ſaid Wire, juſt above the ſurface of the Mercury, help any more; and as I found ſome Inconveniences in theſe two laſt ways of making the Fxperiment, I rather chuſe to rely upon thoſe made with the Golden Ball hanging by a ſilken Thread of 39,2 Inches long, meaſuring from the point of ſuſpenſion to the Center of the Ball.

V. *An*

ling it. And a Gentleman in the Country, ſitting by his Fire-ſide in the Winter, was ſwelled for ſeveral Days with the Smoak or Flame of ſome Poyſon-Wood that was in the Fire.

2. A ſecond thing to be remarked of the Poyſon-Wood is, that it has this effect only on ſome particular Perſons and Conſtitutions; for I. have ſeen my own Brother not only handle, but chew it without any harm at all. And ſo by the ſame Fire one ſhall be poyſoned and another not at all affected.

3. But then Thirdly, this ſort of Poyſon is never Mortal, and will go off in a few Days of it ſelf, like the Sting of a Bee; but generally the Perſon applies Plantain Water, or Sallet-Oyl and Cream.

4. As to its Operation, within a few Hours after the Perſon is poyſoned, he feels an itching Pain that provokes a Scratching, which is followed by an Inflammation and Swelling; ſometimes a Man's Legs only have been poyſoned, and have run with Water.

My Neighbour that was ſo ſadly poyſoned with handling it, told me one thing very remarkable of the Wood, and that is, that when he touched it, he plainly perceiv'd it to differ from the other Wood, that he was throwing up into his Cart; for it was as cold as a piece of Ice; and withal aſſured me, he could diſtinguiſh it blindfold, or in the dark, from any other Wood in the World, by its Coldneſs; but the poor Man is as much afraid of it, when he goes into the Woods, as of a Rattle-Snake. He further tells me, that he felt an itching in a few Hours after he had handled the Wood, but the ſwelling did not come on till in about three Days.

Paul Dudley.

VI. *A farther Account of the ſame Tree. By* William Sherard, *LL. D. R. S. S.*

THE Account I had of the Poyſon-Tree from Mr. *More* (which probably he had from Mr. *Dudley*) is as follows.

The Poyſon-Tree grows to the bigneſs of Elder; I never ſaw the Leaf; the Wood is as cold as Ice; when laid on the Fire, of 5 or 6 Perſons ſitting by it, ſome will fall a ſwooning, fainting, or yawning, continuing ſo for ſome Days, others but a few Hours, and others of the Company not at all. I handle, cut and burn it with impunity; and ſo it is with ſeveral others, I ſuppoſe, according to their ſeveral Conſtitutions. It was never known to kill any body, but only to do hurt to ſome Perſons. I have ſent you all the Seeds of it, I can get.

The Seeds he ſent were but few, but I had a good quantity from Mr. *Catesby* in *Carolina.* He calls it a Water Shrub, of which he never ſaw Leaf or Flower. Tis a ſpecies of *Toxicodendron*, tho' not nam'd by Dr. *Tournefort* in his Inſtitutions, p. 610. but I believe it to be *Arbor Americana alatis foliis, ſucco lacteo venenata. Pluknet. Almag.* 45. Tab. 145. Fig. 1. which is a Species of *Toxicodendron*, that grew formerly at *Chelſea* Garden. What makes me think it to be this, is Mr. *Dudley*'s writing 'tis like a *Sumach*, and that it is by ſome call'd the *Swamp Sumach*; this in its manner of growing and alated Leaves, very

much

have any hole for the Bees to enter by, and would have diſſwaded the Gentleman from felling the Tree, but he inſiſted on it, and offered to lay them any Wager that the Hive was there, and ſo it proved to the great ſurprize of the Country-men.

I cannot diſmiſs this Subject, without acquainting you, that all the Bees we have in our Gardens, or in our Woods, and which now are in great numbers, are the produce of ſuch as were brought in Hives from *England* near a hundred Years ago, and not the natural produce of this part of *America*; for the firſt Planters of *New England* never obſerved a Bee in the Woods, until many Years after the Country was ſettled; but that which proves it beyond queſtion is, that the *Aborigines* (the *Indians*) have no word in their Language for a Bee, as they have for all Animals whatſoever proper to, or aboriginally of the Country, and therefore for many Years called a Bee by the name of *Engliſh Man's Fly*.

Our People formerly uſed to find out Honey in the Woods, by ſurprizing and following one Bee after another by the Eye, till at length they found out where the Bees hived.

I will mention another thing with reſpect to Bees, tho' I don't know but it may have been commonly obſerved; and that is, when they Swarm they never go to the Northward, but move Southward, or inclining that way.

I ſhould have taken notice in the proper place, that when one Bee goes home from the Sugar-plate, he returns with a conſiderable number from the Hive.

VIII. *Propoſitiones aliquot de Projectilium motu Parabolico, Scriptæ An.* 1710. *Per* B. Taylor, *LL. D. R. S. S.*

PROPOSITIO I.

Vi gravitatis, ejuſq; directione datis, motus corporis projecti, in medio non reſiſtente, ſit in Parabolâ.

DEMONSTRATIO.

PRojiciatur corpus de loco *A* (Fig. 1.) in directione lineæ *AB*, ſitque ejus trajectoria curva *ACD*. Ad trajectoriæ punctum quodlibet *C*, duc rectam *CB* in directione vis Gravitatis, rectæ *AB* occurrentem in *B*; atq; reſolvetur motus projectilis per *AC* in partes *AB*, *BC*, quarum *AB* oritur a motu projectionis uniformi, atq; *BC* a vi gravitatis accelerante. Eſt ergo recta deſcripta *AB* tempori proportionalis ab initio motûs in *A*, atq; eſt *BC* in duplicatâ ratione ejuſdem temporis, ſicut olim demonſtravit Galilæus; adeoque in duplicatâ ratione rectæ *AB*. Cum ergo ſit *BC* in duplicatâ ratione rectæ *AB*, conſtat curvam *ACD* eſſe Parabolam. *Q. E. D.*

ea eſt, qua movetur projectile in puncto *A*, & velocitas altera qua deſcribitur *β c*, ea eſt quam corpus acquirit cadendo per altitudinem *B C* quartæ partis parametri ad punctum *A*. Eſt ergo velocitas projectilis in quolibet puncto *A* æqualis velocitati, quam corpus acquirere poteſt cadendo per altitudinem quartæ partis parametri ad punctum illud. *Q. E. D.*

PROP. III.

Datis velocitate & directione projectionis, invenire Trajectoriam corporis projecti.

1. Projiciatur corpus de loco *A* (Fig. 3.) in directione rectæ *A B*. Duc *A C* in directione vis gravitatis, (hoc eſt Horizonti perpendicularem,) ejus longitudinis, ut ſit *C* punctum, unde corpus cadendo acquirere poteſt velocitatem in *A*, quâ fit projectio. Duc *A F* æqualem *A C*, angulum *F A B* conſtituentem cum lineâ directionis *AB*, æqualem angulo *C A B*. Duc *C D* perpendicularem ad *A C* (hoc eſt horizonti parallelam,) eiq; occurrentem *FD*, ipſi *A C* parallelam. Biſeca *F D* in *E*; atq; erit *E F* axis, atq; *E* vertex principalis Parabolæ, per quam movetur projectile. Unde deſcribetur Trajectoria per notas proprietates Parabolæ. *Q. E. F.*

DEMONSTRATIO.

Eſt enim *A C* quarta pars parametri ad verticem *A*. Unde conſtant cætera ex conicis.

2. Ad punctum Trajectoriæ quodvis *G*, duc *GH* ipſi *A C* parallelam, & ipſi *C D* occurrentem in *H*; atque iter *H G* altitudo, per quam corpus cadendo acquirere

poteſt velocitatem, quâ movetur projectile in puncto *G*. *Q. E. F.*

Hoc item conſtat ex Prop 2. & ex conicis.

Scholium. Si ad puncta *A*, & *C* (Fig. 2.) ducantur tangentes *A B*, *C G* occurrentes rectis horizonti perpendicularibus *C B*, *A G*, in *B* & *G*; velocitates in *A* & *C* erunt inter ſe ut tangentium partes interceptæ, *A B*, *C G*.

PROP. IV.

Unico facto experimento invenire velocitatem projectionis.

Projiciatur corpus de loco *A* (Fig. 2.) in directione qualibet *A B*, atq; obſervetur punctum percuſſum *C*. In directione vis gravitatis ducatur *C B*, ipſi *A B* occurrens in *B*, atque ipſis *C B*, *A B*, fiat tertia proportionalis *L*. Erit quarta pars longitudinis *L* altitudo, per quam corpus cadendo acquirere poteſt velocitatem projectilis in *A*. *Q. E. I.*

DEMONSTRATIO.

Eſt enim *L* parameter Trajectoriæ ad punctum *A*; unde conſtat ſolutio per propoſitionem ſecundam.

Scholium. Commodiſſimè inſtituitur experimentum, erectâ ad horizontem perpendiculari *A G*, & directionem ſumendo *A B*, quæ biſecet angulum *C A G*, rectâ etiam *A C* exiſtente horizonti parallelâ. Nam in iſto caſu altitudo quæſita æqualis eſt dimidio diſtantiæ *A C*.

PROP.

PROP. V.

Datis directione & velocitate projectionis; invenire occurſum Trajectoriæ cum rectâ tranſeunte per punctum unde ſit projectio.

Projiciatur corpus de loco *A* (Fig. 4.) in directione rectæ *A B*. In directione gravitati contrariâ, fiat *A C* æqualis altitudini, per quam corpus cadendo acquirere poteſt velocitatem, quâ fit projectio, atq; ducatur *C E* ipſi *A C* perpendicularis. Fiat *F A* æqualis ipſi *C A*, atq; angulum conſtituens *F A B* æqualem angulo *C A B*. Sit *A K* recta, cujus occurſus cum Trajectoriâ quæritur. Duc *F I* ipſi *A K* perpendicularem, atq; ipſi *C E* occurrentem in *D*. In *C E* ſume *E D* æqualem *C D*, atq; ducatur ipſi *C E* perpendicularis *E K*, ipſi *A K* occurrens in *K*. Erit *K* punctum quæſitum.

DEMONSTRATIO.

In *F I* productâ fiat *f I* æqualis *F I*, atq; ducantur *f A*, *f E*, *F E*, *F K*. Quoniam eſt angulus *F I A* rectus, atq; *f I* æqualis *F I*, eſt etiam *f A* æqualis *F A*. Sed per conſtructionem eſt *F A* æqualis *C A*, atque angulus *D C A* rectus. Sunt ergo puncta *C*, *F*, *f* ad circulum centro *A* deſcriptum, quem tangit recta *D C* in *C*. Sunt ergo *F D*, *C D*, *f D*, continue proportionales. Sed eſt *E D* æqualis *C D* (per conſtructionem) Sunt ergo *F D*, *E D*, *f D* continuè proportionales; adeoque ob angulum communem ad *D*, triangula *F E D*, *F f D* ſunt ſimilia, atque angulus *D E F* æqualis angulo *E f F*.

Puncta itaq; tria *F*, *E*, *f* ſunt ad circulum, quem tangit recta *E D* in *E*. Sed quoniam eſt *f I* æqualis *F I*, atq; angulus *F I K* rectus, centrum iſtius circuli eſt in rectâ *I K*; item quoniam eſt angulus *D E K* rectus, centrum illud eſt etiam in rectâ *E K*. Eſt ergo *K* centrum iſtius circuli, adeoq; *F K* æqualis eſt ipſi *E K*. Jam (*per Prop.* 3.) ſunt *F* focus Trajectoriæ, atq; *C A* quarta pars parametri ad punctum *A*. Unde cum ſit *C E* ad *A C* & *E K* perpendicularis, atq; *F K* æqualis *E K*, erit punctum *K* ad Trajectoriam (per conica). *Q. E. D.*

PROP. VI.

Iiſdem datis, invenire occurſum Trajectoriæ cum rectâ, quâlibet poſitione datâ.

Projiciatur corpus de loco *A* (Fig. 5.) in directione *A B*, ſitq; *G H* recta cujus occurſus cum Trajectoriâ quæritur. Duc *A C* in directione gravitati contrariâ, atq; æqualem altitudini, per quam corpus cadendo acquirere poteſt velocitatem, quâ ſit projectio; & duc *AF* æqualem ipſi *AC*, ita ut ſit angulus *FAB* æqualis angulo *C A B*; & ducatur *C E* perpendicularis ipſi *C A*. Ducatur *F I* ipſi *GH* occurrens ad angulos rectos in *I*, atq; ipſi *C E* in *D*; & in *F I* fiat *f I* æqualis *F I*. In *C E* fiat *E D* media proportionalis inter *F D* & *f D*; & ipſi *C E* ducatur perpendicularis *E K*, ipſi *G H* occurrens in *K*. Erit *K* punctum quæſitum. *Q. E. I.*

DEMONSTRATIO.

Conjungendo *f E* demonſtratur ad modum propoſitionis præcedentis.

Scholium. Quoniam punctum *E* ſumi poteſt ad utramlibet partem puncti *D*, duo ſunt puncta *K*, *k*; ubi recta *G H* occurrit Trajectoriæ.

PROP. VII.

Datâ velocitate projectionis, invenire directionem, quæ faciat, ut Trajectoria tranſeat per punctum datum.

Projiciatur corpus de loco *A*, (Fig. 4.) & ſit *K* punctum, per quod tranſire debet Trajectoria quæſita. Fiat *A C*, in directione gravitati contrariâ, æqualis altitudini, per quam corpus cadendo acquirere poteſt velocitatem projectionis. Ducatur *C E* ipſi *A C* perpendicularis, & ad eam duc *K E* perpendicularem. Centris *A* & *K*, & radiis *C A*, *E K* deſcribantur duo circuli ſibi mutuo occurrentes in *F*. Duc *F A*, & biſeca angulum *C A F* rectâ *A B*. Erit *A B* directio quæſita, in quâ fieri debet projectio, ut tranſeat Trajectoria per punctum *K*. *Q. E. F.*

DEMONSTRATIO.

Eſt *C A* æqualis quartæ parti parametri ad punctum *A* (per *Prop.* 2.) Et per conſtructionem ſunt *F A*, *C A* æquales, item *F K*, *E K*. Eſt ergo *F* focus Parabolæ per puncta *A*, *K*, deſcriptæ: Sed illam tangit recta *A B* in *A*, propter angulos *F A B*, *C A B* æquales. Corpore itaq; projecto de puncto *A*, in directione *A B*, eâ cum velocitate, quam corpus acquirere poteſt cadendo per altitudinem *C A*, tranſibit Trajectoria per punctum *K*. *Q. E. D.*

NB. Cum circulorum centris *A*, *K*, & radiis *C A*, *E K*, deſcriptorum duo ſint concurſus, *F*, *f*, biſectis angulis *F A C*, *f A C*, duo etiam erunt directiones, quæ faciant, ut Trajectoria tranſeat per punctum datum *K*.

PROP. VIII.

Datâ directione projectionis, invenire velocitatem, quæ faciat ut Trajectoria tranſeat per punctum datum.

Projiciatur corpus de loco *A* (Fig. 6.) in directione rectæ *A B*, & faciendum ſit ut tranſeat Trajectoria per punctum *K*. Duc *A K*, eamq; biſeca in *C*, & in directione gravitatis duc *C B*, ipſi *A B* occurrentem in *B*; & junge *B K*. Duc *A D*, *K E*, ipſi *C B* parallelas, & ducantur *A F*, *K F* ſibi mutuo occurrentes in *F*, ita ut ſint anguli *F A B*, *D A B* æquales, item *F K B*, *E K B*. Erit *F A* æqualis altitudini, per quam corpus cadendo acquirere poteſt velocitatem quæſitam, quâ projectio fieri debet in directione *A B*, ut tranſeat Trajectoria per *K*. *Q. E. F.*

DEMONSTRATIO.

Quoniam *C B* eſt in directione gravitatis, eſt diameter Parabolæ; & quoniam *C A* æqualis eſt *C K*, eſt *C B* diameter ad ordinatam *A K*. Unde cum ſit *A B* tangens ad parabolam in *A*, erit etiam *K B* tangens ad punctum *K*. Hinc quoniam *A D* eſt in directione diametrorum, atq; angulus *F A B* æqualis angulo *D A B*, tranſit *A F* per focum parabolæ. Eodem argumento tranſit etiam *K F* per focum. Eſt ergo *F* focus parabolæ, adeoq; *F A* quarta pars parametri ad punctum *A*, quæ proinde æqualis eſt altitudini, per quam corpus cadendo acquirere poteſt velocitatem ad hoc neceſſariam, ut projecto corpore de *A*, in directione *A B*, tranſeat Trajectoria per punctum *K*.

P R O P. IX.

Invenire velocitatem minimam & directionem ei congruam, quâ fieri poteſt, ut tranſeat Trajectoria per punctum datum.

Projiciendum ſit corpus de loco *A* (Fig. 7.) cum velocitate omnium minimâ & directione ei congruâ, quâ fieri poteſt ut perveniat in locum *K*, hoc eſt ut tranſeat Trajectoria per punctum *K*. Ductis *A C*, *K E* in directione gravitati contrariâ, & ductâ *A K*, biſeca angulos *C A K*, *E K A*, rectis *A B*, *K B*, ſibi mutuo occurrentibus in *B*. Duc *B C* ipſi *A C* occurrentem ad angulos rectos in *C*, atq; erit *C A* altitudo, per quam corpus cadendo acquirere poteſt velocitatem quæſitam; eritq; *A B* directio quæſita. *Q. E. F.*

DEMONSTRATIO.

Ducatur *B F* ipſi *A K* occurrens ad angulos rectos in *F*, atque occurrat *C B* ipſi *K E* in *E*. Propter angulos *C A B*, *B A F*, item angulos *E K B*, *B K F*, æquales atq; angulos rectos in *C*, *E*, & *F*, erunt æquales *C A*, *F A*, item *E K*, *F K*. Hinc conſtat puncta *A*, *K* eſſe ad parabolam, quam tangit recta *A B* in *A*, cujuſq; parameter ad punctum *A* eſt quadruplum altitudinis *C A*, foco exiſtente *F*. Corpore itaque projecto de *A* in directione *A B*, eâ cum velocitate, quam corpus acquirere poteſt cadendo per altitudinem *C A*, Trajectoria erit dicta Parabola (per Prop. [illegible].) Dico autem illam eſſe velocitatem minimam, ſeu eſſe *C A* partem quartam parametri omnium minimæ, quâ Parabola deſcribi poteſt, quæ tranſeat per puncta *A*, *K*.

Si fieri poteſt, in *C A* capiatur altitudo *c A* minor, quæ ſit quarta pars parametri ad punctum *A* Duc ipſi *C A* perpendicularem *c e*, ipſi *K E* occurrentem in *e*, & centro *A* & radio *A c* deſcribatur circulus ipſi *A K* occurrens in *f*. Quoniam *c A* dicitur quarta pars parametri ad punctum *A*, focus Parabolæ erit punctum aliquod *p*, in circumferentia circuli *c p f*, centro *A* & radio *A c* deſcripti. Si ergo ſit punctum *K* ad parabolam illam, erit *p K* æqualis *e K*. Eſt vero *F K* æqualis *E K*. Unde cum ſit *e K* minor ipſâ *E K*, erit etiam *p K* minor ipſâ *F K*. Sed eſt *p K* major ipſâ *f K*, atq; eſt *f K* major ipſâ *F K*, (quoniam eſt *f A* minor ipſâ *F A* per hyp.) unde fit *p K* major ipſâ *FK*. Sed jam dicebatur *p K* minor ipſâ *F K*; quæ repugnant. Nequit ergo Parabola deſcribi, quæ tranſeat per puncta *A*, *K*, minori parametro quam in ſolutione definitum eſt. *Q. E. D.*

PROP. X.

Datâ velocitate projectionis, invenire directionem, quæ faciat, ut corpus projiciatur ad diſtantiam omnium maximam in plano dato; atq; diſtantiam illam definire.

Sit planum datum *A K* (Fig. 8.) atq; invenienda ſit diſtantia maxima *A K*, ad quam corpus projici poteſt in plano illo.

Duc *AC* in directione gravitati contrariâ, æqualem quartæ parti parametri ad punctum *A*. Tum biſecto angulo *CAK* rectâ *AB*, erit *AB* directio projectionis quæſita. Duc *CB* ipſi *CA* perpendicularem, rectæ *AB* occurrentem in *B*, atque in *CB* productâ fiat *BE* æqualis ipſi *BC*. Tum ductâ *EK*, ipſi *CA* parallelâ, quæ occurrat plano *AK* in *K*, erit *AK* diſtantia maxima quæſita.

DEMONSTRATIO.

Centro *A* & radio *AC* deſcribe circulum, ipſi *AK* occurrentem in *F*, & ducantur *BF*, *BK*. Quoniam anguli *CAB*, *BAF* ſunt æquales (per conſtructionem) atque *AF* æqualis *CA*, erit *BF* æqualis *CB*, æqualis *BE* (per conſtructionem) atq; anguli ad *F* recti. Unde etiam fit *FK* æqualis *EK*. Sunt ergo puncta *A*, *K* ad parabolam foco *F* deſcriptam, quam tangit *AB* in *A* (propter angulos *CAB*, *FAB* æquales) quartâ parte parametri ad punctum *A* exiſtente *CA*. Corpore igitur projecto de loco *A*, in directione *AB*, eâ cum velocitate, quam corpus acquirere poteſt cadendo per altitudinem *CA*, Trajectoria tranſibit per punctum *K* (per Prop. 2.) *Q. E. D.*

Dico autem, quod ſit *KA* diſtantia omnium maxima, ad quam corpus projici poteſt de loco *A* eâdem cum velocitate.

Si fieri poteſt, eâdem parametro, ad *A* deſcribatur parabola, quæ tranſeat per punctum diſtantius *k*; hoc eſt projiciatur corpus ad diſtantiam majorem *kA*. Duc *Bk*, atq; ipſi *KE* parallelam *ke*, ipſi *CE* occurrentem in *e*. Quoniam *FB*, *EB*, item *FK*, *EK* ſunt æquales, ſunt etiam anguli *FBK*, *EBK* æquales. Angulus ergo *FBk* major eſt angulo *kBe*; unde ſit *k F* major ipſa *k e*. Sed quoniam eſt *A C* quarta pars parametri ad punctum *A*, focus parabolæ erit alicubi in circumferentiâ circuli centro *A*, & radio *C A* deſcripti. Sit focus ille *p*, & ducatur *p k*. Tum quoniam *p k* major eſt ipſâ *F k*, erit etiam *p k* major ipſâ *k e*. Sed ut parabola tranſeat per punctum *k*, debet eſſe *p k* æqualis *k e*. Nequit ergo parabola duci in circumſtantiis propoſitis, quæ tranſeat per punctum *k* diſtantius puncto *K*; adeoq; nec corpus projici ad diſtantiam majorem ipſâ *K A*. *Q. E. D.*

PROP. XI.

Iiſdem poſitis, invenire locum puncti K, *ſeu Curvam deſcribere, quæ tangat omnes parabolas eodem vertice* A *& eâdem parametro deſcriptas.*

Sit *A* (Fig. 9.) vertex datus, atq; in directione gravitati contrariâ ducatur *A C* æqualis quartæ parti parametri datæ. Tum deſcriptâ parabolâ, cujus vertex principalis ſit *C*, atq; focus *A*; erit ea curva quæſita.

DEMONSTRATIO.

Duc quamlibet *A K*, atq; in eâ ſume *F A* æqualem *C A*, & ducatur *C B* ad *C A* perpendicularis, ſitq; *K* punctum in propoſitione præcedente inventum. In *A C* productâ, factâ *C c* æquali *C A*, ducatur *c e* parallela ipſi *CE*; ducatur etiam *K E* parallela ipſi *AC*, ipſis *C E*, *c e* occurrens in *E* & *e*. Per propoſitionem præcedentem eſt *K E* æqualis ipſi *F K*; unde cum ſit etiam *F A* æqualis ipſi *A C*, æqualis ipſis *C c*, *E e* (per conſtructionem) eſt ergo *Ke* æqualis *KA*; unde eſt punctum *K* ad parabolam foco *A* & vertice principali *C* deſcriptam. *Q. E. D.*

Biſecto autem angulo *AKE* à rectâ *KB*, tanget hæc utramq; parabolam, tam foco *F* per *A* & *K*, quam foco *A* per *K* deſcriptam. Unde ſe mutuo tangunt parabolæ. *Q. E. D.*

Errat. *Pag.* 152. *l. ult.* pro *βc. l. Bb.*

FINIS.

LONDON:
Printed for *W.* and *J. Innys.* Printers to the *Royal*
Society; at the Sign of the *Prince's* [illegible]
End of St. *Paul's-Church-Yard.*

Fig: 1.

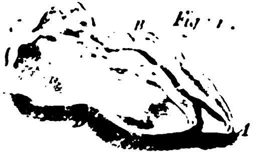

Fig: 2.

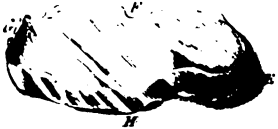

Fig: 3.

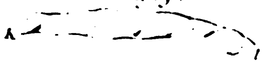

Fig: 4.

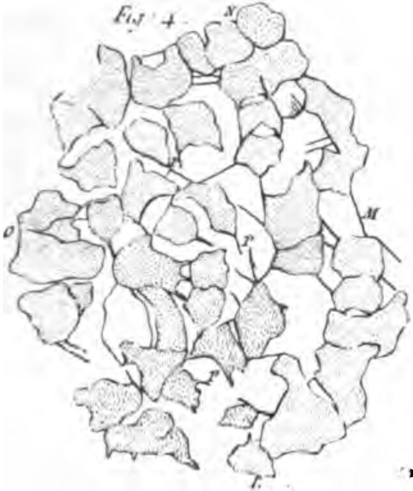

Fig: 5.

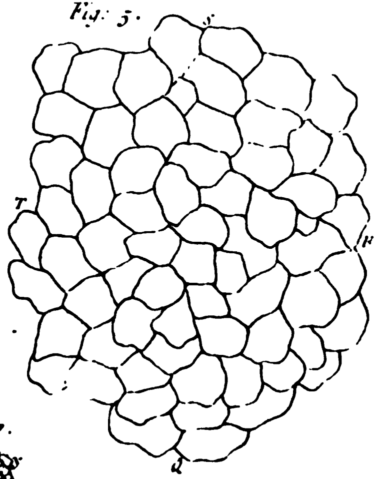

Fig: 7.

Fig: 6. [illegible]

Numb. 368

PHILOSOPHICAL TRANSACTIONS.

For the Months of *May*, *June*, *July*, and *Auguſt*, 1721.

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I. *A Deſcription of the Mooſe-Deer in* America.

By the Honourable Paul Dudley, *Eſq;* F. R. S. *Communicated by* John Chamberlayne, *Eſq;*

AMong other things the *Royal Society* have had from *America*, I find, in Mr. *Lowthorp*'s Abridgment of the *Philoſoph. Tranſact.* a ſhort Account of the Mooſe, taken from Mr. *Joſelyn*'s Hiſtory of *New England*, but in my opinion ſo imperfect, that I ſhall not ſcruple to preſent you with the Natural Hiſtory of that fine Creature, partly from my own Knowledge, and partly from the Information of Men of Ingenuity and Probity, that are better acquainted with it.

The Mooſe is a Creature, not only proper, but it is thought peculiar, to North *America*, and one of the nobleſt Creatures of the Foreſt; the *Aborigines* have given him the Name of *Mooſe*, *Mooſuk* in the Plural.

Of Mooſe there are two ſorts, the *Common light grey Mooſe*, by the *Indians* called *Wampooſe*; theſe are more like the ordinary Deer, ſpring like them, and herd ſometimes to thirty in a Company. And then there are the *large, or black Mooſe*, of which I ſhall now give you the following Account.

Firſt, That he is the Head of the Deer-kind, has many things in Common with other Deer, in many ny things differs, but in all very ſuperiour. The Mooſe is made much like a Deer, parts the Hoof, chews the Cudd, has no Gall, his Ears large and erect. The Hair of the black Mooſe is a dark Grey, and upon the Ridge of his Back to ten and twelve Inches long, of which the *Indians* make good Belts. He has a very ſhort Bob for a Tail. Mr. *Neal*, in his late Hiſtory of this Country, ſpeaking of the Mooſe, ſays they have a long Tail; but that Gentleman was impoſed on, as to other things beſides the Mooſe.

Our Hunters have found a Buck, or Stagg-Mooſe, of fourteen Spans in heighth from the Withers, reckoning nine Inches to a Span; a quarter of his Veniſon weighed more than two hundred pounds. A few Years ſince, a Gentleman ſurprized one of theſe black Mooſe, in his Grounds within two miles of *Boſton*; it proved a Doe or Hind of the fourth Year: After ſhe was dead, they meaſured her upon the Ground, from the Noſe to the Tail, between ten and eleven Feet, ſhe wanted an Inch of ſeven Foot in height.

The Horns of the Mooſe, when full grown, are between four and five Foot from the Head to the Tip, and have ſeven Shoots or Branches to each Horn, and generally ſpread about ſix Foot. When the Horns come out of the Head, they are round, like the Horns of an Ox; about a Foot from the Head, they begin to grow a palm broad, and further up ſtill wider, of which the *Indians* make good Ladles, that will hold a Pint. When a Mooſe goes through a Thicket, or under the Boughs of Trees, he lays his Horns back on his Neck, not only that he may make his way the eaſier, but to cover his Body from the

Bruiſe

The Mooſe being very tall,and having ſhort Necks, do not graze on the Ground,as the common Deer, Neat Cattel, *&c.* do, and if at any time they eat Graſs, 'tis the top of that which grows very high, or on ſteep riſing Ground. In the Summer they feed upon Plants, Herbs, and young Shrubs, that grow upon the Land, but moſtly, and with greateſt delight on Water-Plants, eſpecially a ſort of wild Colts-foot and Lilly, that abound in our Ponds, and by the ſides of the Rivers, and for which the Mooſe will wade far and deep, and by the noiſe they make in the Water, our Hunters often diſcover them. In the Winter they live upon Browſe, or the tops of Buſhes and young Trees, and being very tall and ſtrong, they will bend down a Tree as big as a Man's Leg, and where the Browſe fails them, they will eat off the Bark of ſome ſorts of Trees, as high as they can reach. They generally feed in the Night, and lie ſtill in the Day.

The Skin of the Mooſe, when well dreſs'd, makes excellent Buff; the *Indians* make their Snow-ſhoes of them: Their way of dreſſing it, which is reckoned very good, is thus: After they have hair'd and grain'd the Hide, they make a Lather of the Mooſe's Brains in warm Water, and after they have ſoaked the Hide for ſome time, they ſtretch and ſupple it.

II. *Some Remarks on the Allowances to be made in Aſtronomical Obſervations for the Refraction of the Air. By Dr.* Edm. Halley, R. S. S. *Aſtronomer Royal. With an accurate Table of Refractions.*

WERE the *Medium* of our *Air* much more in Quantity, or the Force of *Gravity* much greater than it is, or in a word, were the Refractive Power of the Air much more ſenſible than we find it, nothing could have been a greater Impediment to Diſcoveries in Aſtronomy: For all Objects appearing by Refraction higher than really they are, till ſuch time as the Laws and Quantity of that Refraction had been aſcertained, it would have been impoſſible to have been ſecure of the true obſerved Place of any Cœleſtial Object. But as it falls out to be ſo little, that none but nice Inſtruments can perceive its Effects, it was not diſcovered to be at all, till *Bernard Walther*'s time, about the Year 1500; nor brought to any ſort of Rule till *Tycho Brahe*; nor aſcertained, till our worthy *Preſident* made the firſt accurate Table thereof: The *Curve* which a Beam of *Light* deſcribes, as it approaches the *Earth*, being one of the moſt perplext and intricate that can well be propoſed, as Dr. *Brook Taylor* in the laſt Propoſition of his *Methodus Incrementorum* has made it evident. By this Table it follows that the *ratio* of

Tabula Refractionum Siderum ad Altitudines apparentes.

Alt. Appar. deg.	m.	Refractio m.	ſec.	Alt. Appar. deg.	Refractio m.	ſec.	Alt. Appar. deg.	Refractio m.	ſec.
0	0	33	45	16	3	4	46	0	52
0	15	30	24	17	2	53	47	0	50
0	30	27	35	18	2	43	48	0	48
0	45	25	11	19	2	34	49	0	47
1	0	23	7	20	2	26	50	0	45
1	15	21	20	21	2	18	51	0	44
1	30	19	46	22	2	11	52	0	42
1	45	18	22	23	2	5	53	0	40
2	0	17	8	24	1	59	54	0	39
2	30	15	2	25	1	54	55	0	38
3	0	13	20	26	1	49	56	0	36
3	30	11	57	27	1	44	57	0	35
4	0	10	48	28	1	40	58	0	34
4	30	9	50	29	1	36	59	0	32
5	0	9	2	30	1	32	60	0	31
5	30	8	21	31	1	28	61	0	30
6	0	7	45	32	1	25	62	0	28
6	30	7	14	33	1	22	63	0	27
7	0	6	47	34	1	19	64	0	26
7	30	6	22	35	1	16	65	0	25
8	0	6	0	36	1	13	66	0	24
8	30	5	40	37	1	11	67	0	22
9	0	5	22	38	1	8	68	0	22
9	30	5	6	39	1	6	69	0	21
10	0	4	52	40	1	4	70	0	20
11	0	4	27	41	1	2	71	0	19
12	0	4	5	42	1	0	72	0	18
13	0	3	47	43	0	58	73	0	17
14	0	3	31	44	0	56	74	0	16
15	0	3	17	45	0	54	75	0	15

III. *The Variation of the Magnetical Compaſs, obſerved by Capt.* Rogers, *Commander of the Ship* Duke, *in his Paſſage from* Cape St. Lucar *in* Calefornia *to the Iſle of* Guam *or* Guana, *one of the* Ladrones, *with ſome Remarks thereon. Communicated by the ſame.*

HAVING lately had the Opportunity of peruſing Capt. *Woods Rogers*'s original Journal, who in 1709-10, in eight Weeks time traverſed the great *South-Sea*, or *Pacifick Ocean*, I was highly pleaſed to find the Care he had taken to ſet down the Variations of the *Magnetical Compaſs* in his Paſſage from the South Cape of *Calefornia* to the Iſland of *Guana*, being about ſeven Hours or 105 Degrees of Longitude. This might have been long ſince expected from Capt. *Dampier*, who had three times made the *Tour* of the *World*, and thrice gone this very ſame Track.

It were to be wiſht that the *French*, who have had frequent Opportunities to do it, would beſtow upon us an account of the Variations they have lately found in their Voyages from *Peru* and *Chili* to *China*; and that the *Spaniards* would tell us how the Needle varies at this time in the North Part of that great Sea, through which they return from the *Manilla's* to *New Spain*. With theſe helps, having three Points in each Curve, we might be enabled

By this it appears, that at about 250 or 300 Leagues Weſt from the South-head of *Calefornia*, the *Eaſt Variation* diminiſhes to about ¼ of a degree; That for 1300 Leagues from thence, the ſame *Eaſterly Variation* gradually encreaſes to about 12 degrees, where it becomes greateſt. And that at the Iſle of *Guam*, five hundred Leagues ſtill more *Weſterly*, it is again decreaſed to 5 degrees 40 minutes.

As far as this ſingle Inſtance can direct us, I am inclinable to think, that in all that ſpace of Sea which lies to the Northwards of our Track, between *Japon* and *Calefornia*, there reigns an *Eaſterly Variation*, which is ſtill greater and greater as the North Latitude encreaſes. But that to the Southward of our Track, and eſpecially to the Southward of the Equinoctial, a *Weſterly Variation* ariſes, of no great extent or quantity, but which is greateſt about 1000 Leagues Weſt from the Coaſts of *Peru* and *Chili*, about the ſame Meridians where Capt. *Rogers* found the *Eaſt Variation* ſmalleſt. This is agreeable to the *Theory* of the *Variation* I laid down in No. 148. of theſe *Tranſactions*, about 40 Years ſince; and I then expreſly mentioned, in my ſeventh Remark on the Obſervations there cited, that there was undoubtedly ſuch a Tract of *Weſt Variation* in the Southern Parts of the *South-Sea*, it being the neceſſary Conſequence of the Site of the four Magnetical Poles there ſuppoſed, though at that time I wanted Experiments to prove it.

IV. *An Addition to the Deſcription of the Art of Living under Water, publiſh'd in* Phil. Tranſact. No. 349. *By the ſame.*

IN No. 349. of the *Philoſophical Tranſactions*, I did, as I ſuppoſe, ſufficiently explain the Method I had practiced and found effectual to furniſh Air at any reaſonable Depth under Water, and in any quantity deſired, for the Subſiſtance of men that ſhall have occaſion to work on Wrecks, or otherwiſe at the bottom, under a great Preſſure of Water. This I did by means of the Diving-Bell, which, being from time to time repleniſhed with freſh Air, I had found ſufficient to maintain five Men for near two Hours together in ten fathom Water, without the leaſt Hurt or Inconvenience. But the Bell being not to be moved from place to place, but by moving the Veſſel from which it hung ſuſpended, was a great Impediment to the Work that was to be done below; and therefore I bethought my ſelf how to enable the Diver to go out from the Bell to a conſiderable diſtance, and to ſtay a ſufficient time without it, with full freedom to act as occaſion ſerved. And conſidering that the Preſſure being greater on the Surface of the Water in the Bell, than on any other Surface that was higher than it, the Air would by a Pipe paſs from the Bell into a Cavity of Air over that higher Surface; I concluded, that putting on a Cap of Lead made weighty enough to nk empty, and in form reſembling the Bell it ſelf, I might by flexible Pipes, which a man might carry coiled on his Arm, receive a conſtant Stream of Air from the Magazine thereof in the great Bell, ſo long as the Surface of the Water in the Caps was above the Level of that in the Bell.

Following this Idea, I procured Pipes to be made, which anſwered all that was hoped from them. They were ſecured againſt the Preſſure of the Water, by a ſpiral braſs Wire, which kept them open from end to end, the Diameter of the Cavity being about the ſixth part of an Inch. Theſe Wires we coated with thin Glove-leather, curiouſly ſowed on, and then dipt the Leather into a Mixture of Oyl and Bees-Wax hot, which, filling up the Pores of the Leather, made it impenetrable to Water. Then we drew ſeveral Folds of Sheeps Guts over them, which when dry, we painted with a good coat of Paint, and then ſecured the whole with another coat of Leather, to keep them from fretting. The Pipes of which we made ſeveral, were much about forty Foot long, the ſize of a half Inch Rope; the one end thereof being fixt in the Bell, at ſome height above the Water, and the other end faſtned to a Cock, which opened into the Cap. The uſe of the Cock being to ſtop the return of the Air, whenever there was occaſion to ſtoop down, or go below the Surface of the Air in the Bell, which was neceſſary as often as there was occaſion to go out or return into the Bell.

The Diver therefore putting on his Cap, and coyling his Pipe on his Arm, like a Rope, as ſoon as he is diſcharged from the Bell, opens his Cock, and marches on the bottom of the Sea, vearing out the

Coiles of his Pipe, which ſerve as a Clue to direct him back again: and this I have ſeen practiſed, without any ill Incident attending it.

But there are two things to be remark'd in this affair; firſt, That the Weight of a Man being very little more than that of his Bulk in Water, he cannot act with any Strength, nor ſtand with any Firmity, eſpecially where any thing of a Stream runs, without a conſiderable addition of Weight; and therefore the leaden Caps were made to weigh about half a hundred weight, to which I added a Girdle of large Weights of Leads, of about the ſame Weight in the whole, this being to be worn about the Waſt; and two Clogs of Lead for the Feet, of about 12 pound each. With this Acceſſion of Weight I found a Man could ſtand well in an ordinary Stream, and even go againſt it. The other thing neceſſary to be provided againſt, was the Cold of the Water, which though it could not be wholly taken off, ſo that a Man could endure it long, yet it was much eaſed by Habits of Waſtecoat and Drawers, made cloſe to the Body, of that thick ſort of woollen Stuff they make Blankets of: This being full of Water, would be a little warm'd by the Heat of the Body, and keep off the Chill of new cold Water coming on it.

As to Sight under Water, as long as the Water is not turbid, things are ſeen ſufficiently diſtinct; but a ſmall degree of Thickneſs makes perfect Night, in no great depth of Water: In my leaden Caps, which from their uſe I call'd *Caps of Maintenance*, I at firſt fixt a plain Glaſs before the Sight, but ſoon found that the Vapour of the Breath would make ſuch a Dew on the Surface of the Glaſs, that it hindred its Tranſparency: To remedy which, I found it neceſſary to prolong that Side of the Cap that was before the Eyes, and thereby enlarged the Proſpect of what was under us.

V. *An Account of an* Aurora Borealis, *obſerved at* Dublin, *on* Monday *the ſixth of* February, 1720-1. *By* J. W.

THE *Royal Society* having done me an unexpected Honour, in cauſing a ſhort Account, which I ſent 'em of the *Auroræ Boreales*, that were ſeen the 10th and 24th of *November*, in the Year 1719, to be printed in their Journals; I now preſent them with an Extract of what was obſerved on *Monday* the 6th of *February* laſt. The Air was all that Day, as it had been for ſome time before, very clear and ſharp; about half an Hour paſt four in the Evening, ſome flying Clouds appear'd, and the Sky was ting'd with a very unuſual yellowiſh Colour, which perhaps might be reflected from a great Quantity of Snow, that ſoon after fell for near a quarter of an Hour. However that might be, I'm willing to date the beginning of the enſuing *Phænomena* from the firſt appearance of this uncommon Light. About a quarter paſt ſix, a thin Vapour, which was as yet very ill defin'd, and in all appearance reſembl'd an exceeding black Cloud, had fix'd it ſelf in the Northern *Hemiſphere*; its Edges were ting'd with a reddiſh Yellow, that by degrees, as it approach'd the

Vertex, grew more dilute, till at laſt it ended in a faint Whiteneſs. That in reality it was no Cloud, but only a Vapour exceeding pure and limpid, was manifeſt, becauſe ſeveral of the fix'd Stars ſhone thro' it, without having their Light in any degree effac'd. In the midſt of this dark *Baſis*, about half an Hour paſt ſix, a lucid *Area* ſhew'd it ſelf due N. Eaſt, about 35 deg. above the *Horizon*, and in leſs than a Minute from the time I firſt diſcover'd it, emitted a very large *Pyramidal* Stream of ſhining Vapour, which with an incredible Swiftneſs aſcended obliquely towards S. S. Weſt, ſo as to leave the *Zenith* conſiderably to the Weſtward, and very ſoon after, about the ſame Place, ſix others aroſe at the ſame inſtant almoſt to the *Zenith*. From this time till 48 Minutes paſt ſix, we had repeated Projections of theſe lucid Rays, without any order as to Time, Place, or Magnitude. They did not only ariſe from behind the dark *Baſis*, but ſometimes as it were out of the pure Sky; and tho' ſome of them continu'd viſible more than a Minute, yet the greater part of them only juſt ſhew'd themſelves and died away. I had now got to the Top of a convenient Obſervatory, where (though deſtitute of Inſtruments) I had a free Proſpect of the *Horizon*; and in company of another Gentleman, fix'd my ſelf with great Attention, to expect the enſuing *Phaſes* of this *Phænomenon*.

About 6 h. 55 m. between N. W. by North, and W. N. Weſt, we found the Repreſentation of a very bright *Crepuſculum*, ſuch as that which appears about 20 Minutes after Sun-ſet; from which aroſe ſeveral very large Beams of Light, not exactly erect towards the *Vertex*, but ſomewhat declining to the South;

among theſe, one which aroſe about N. Weſt, and in three or four ſecond Minutes paſs'd over 50 or 60 degrees of a great Circle, was above all others that had preceded, the moſt ſplendid: Its ſides were inclin'd to each other with an Angle of about 8 or 10 Degrees, and were ting'd with a brisk lively Red, which by degrees, as it approach'd the *Axis*, became more intenſe and dirty: On the other hand, receding from the *Axis*, its Colour was a pale Yellow, that ſoon loſt it ſelf in a faint Whiteneſs.

From this time no Moment paſs'd without ſuch Variety of different *Phaſes*, that it was impoſſible for the Eye of any ſingle Perſon to purſue it thro' the ſuddenneſs of its Alteration. While ſome of the lucid Beams ſeem'd to ſtand fix'd, as it were, among the Stars, others mov'd ſlowly from Eaſt to Weſt, by which they ſeem'd to meet each other, ſometimes to recede from each other, and ſometimes by a kind of appoſition, great ones were produc'd from others of an inferior order.

The lucid *Area*, which I firſt diſcover'd in the N. Eaſt, had now form'd it ſelf into a *Parallellogram*, whoſe upper and lower Edges were 5 or 6 Degrees diſtant from each other, and nearly parallel to the *Horizon*: In this, as if behind a Curtain, vaſt Waves of Light, whoſe Extremities did not reach the *Periphery* of the dark *Baſis*, ſeem'd to meet and pervade each other; at other times, while ſome of 'em, with a remarkable Velocity, mov'd Eaſtward; others, as if behind them, would fly towards the Weſt; by which variety of different Motions, as often as any Interval paſs'd between the Colliſion of theſe erect Waves, a beautiful Undulation was produc'd, and its Pulſes, by the adjoyning Parts of the Fluid, were propagated to a vaſt diſtance.

While we ſtood amaz'd at this ſurprizing Sight, the *Axis* of the colour'd *Pyramid*, which aroſe in the N. Weſt, had mov'd conſiderably toward the Weſt, and at 7h. 25m. was about 23 or 25 Degrees to the North of *Venus*. The dark *Baſis* of this Meteor had now extended almoſt to the Eaſt Point of the *Horizon*, and at half an Hour paſt ſeven, between E. N. E. and E. by N. ſeveral large Columns aſcended in an inſtant to the *Zenith*; the moſt Eaſtward whereof was remarkably convex toward the South, and ting'd with a pale Red, as were moſt of thoſe which aſcended with it. They were met by others, that aroſe at the ſame time beetween the North and Weſt, and in the *Zenith* form'd a vaſt Collection of Vapour, that pretty much reſembl'd Smoke inlightned by the Sun's Beams; its Waves reflected a brisk, lively red Colour, and in ſome places a pale Yellow; they roll'd indifferently any way; and in little more than a Minute, when the firſt Efforts of their Congreſs were ſpent, and all ſeem'd fix'd and ſerene, the *Corona* projected ſeveral ſmall Rays, which with a ſlow uniform Velocity deſcended between W. by N. and N. W. ſoon after which it died away.

We had not much time to lament the Abſence of our *Spectrum*, for at 7 h. 40 m. ſeveral other *Striæ* were diſcharged from behind the dark *Baſis*, which interſecting with others, that at the ſame time aroſe about the Eaſt and Weſt Points, form'd in the *Zenith*, or rather 6 or 8 Degrees to the South thereof, a ſecond much more elegant and ſurprizing than the former, and indeed than any thing that had yet appeared: it was not only ting'd with different Orders of red and yellow, but alſo with blue and violet, the laſt of which, by a Mixture with the white Light, appear'd faint and inclin'd to Purple. Tho' the Vapour, of which this and the preceding *Corona* were form'd, was ſo exceeding thin and pure, that ſeveral of the fix'd Stars were viſible thro' it, yet it reflected a Light ſo copious, that I could thereby perfectly diſtinguiſh the time of night by a ſmall Gold Watch. While thus delighted, our *Phænomenon* ejected four or five large Columns toward the N. W. (beſides others toward the South) which appear'd pointed at the Top, and their Sides inclin'd to each other with an Angle of 5 or 6 Degrees. When their Baſes were extended about 30 or 35 Degrees from the *Vertex*, the lower Parts of two or three of 'em broke, as it were, by the meer Weight of the Vapour, ſeparated from the upper, and deſcended with a ſlow Motion, in the Form of truncate Cones: they were gradually follow'd by their upper Parts, and in about a Minute were loſt in a large Body of Light that was ſettl'd between the N. W. by N. and W. N. W. The *Corona*, as if exhauſted by theſe great Diſcharges, became immediately more dilute and languid, its lively Colours faded, and were ſucceeded by a whitiſh vibrating Light, that in leſs than two Minutes intirely diſappear'd.

The dark Vapour, which continu'd to poſſeſs the Polar Regions, had now extended it ſelf from the Eaſt to the N. W. by N. point of the *Horizon*, and was form'd in a large Segment of a Circle, whoſe Center was about 20 Degrees below the *Horizon*; its upper Edge was ting'd with a pale Red, which was ſoon loſt in a florid Yellow, and this again, as it approach'd the *Zenith*, became more effœte and languid. In this dark Segment ſeveral lucid *Areas* frequently diſcover'd themſelves, with a vibrating Light, which inſtantly diſappear'd, as if a Curtain were drawn over them;

and from it Rays of very different Magnitudes continu'd to aſcend without any Uniformity as to time and place, till 48 or 49 minutes paſt ſeven, when a third *Corona*, very little, if at all, inferior to the preceding ones, either in the Variety of its Colours, or in the quantity of Light it emitted, was form'd in the *Zenith*. As the preceding were both produc'd by the Northern *Striæ*, ſo this was augmented by two or three large ones, that aroſe due South, out of the pure Sky, and were, in all probability, part of the Vapour, which had been projected beyond the *Zenith*, or which had ſubſided from the two former: they caus'd the Vapour, of which this Image was compos'd, to move with great Violence, in different Directions, not unlike Waves of Smoke, confin'd in a reverberating Furnace; this Motion being abated, the Vapour acquir'd a kind of Stagnation, in which State it continu'd but a very ſhort time, before it projected ſeveral lucid Beams, (an inevitable Fore-runner of its approaching Diſſolution,) between the North and Weſt, and ſoon after, pardon the levity of the Expreſſion, *Nocti ſe immiſcuit atræ.*

About this time, the great Beam, which aroſe in the N. W. and had preſerv'd its Colours in their original Beauty, for more than three quarters of an hour, began to fade, and at 7 h. 53 m. was abſorb'd in a vaſt body of Light, which ſeem'd fix'd in that part of the *Horizon*: it had mov'd in that time 15 or 20 degrees to the Weſtward of the Place from whence it aroſe. The *Impetus* of the Vapour being now pretty much abated, we had nothing extraordinary but ſucceſſive Diſcharges of pointed Rays between the N. Weſt, and E. N. Eaſt; without any Order

or

or Uniformity as to time or place; ſetting aſide theſe, there was very little difference in the general face of affairs for 20 minutes; neither had we much reaſon to hope for any, becauſe the ſeverity of the Cold was ſuch, that it oblig'd us to remove to a better Climate, and by that means we unfortunately loſt the enſuing *Phaſes* of our dying *Meteor*.

Dublin, June 28th. 1721.

I am, &c.
J. W.

VI. *A Deſcription of an* Aurora Borealis, *ſeen on the ſame Day at* Cruwys-Morchard, *in* Devonſhire. *By* Samuel Cruwys, *Eſq*; F.R.S.

M*Onday* the 6th of *Feb.* 1720-21, a little before 7 in the Evening, there aroſe out of the North, or a little towards the Eaſt, a bright *Crepuſculum*, which ſoon ſpread it ſelf a great way through the Northern part of the Hemiſphere. About 7 (when I firſt ſaw it) it began to leave behind it, at due North, or a few Degrees to the Eaſt, part of a very clear Sky (which look'd like a black Cloud, but the Stars ſhone in it clearly) being a Segment of a Circle, into which Figure, the *Crepuſculum* (or expanded Body of lucid Vapour) had now formed its upper Limb alſo, making a kind of broad *Iris*, terminated at each end by the Horizon.

All this while the ſtreaming Lights appear'd in great Variety as to Figure, Place, Magnitude and Colour, but for the moſt part of a redder Colour (eſpecially towards the Weſt) than the *Crepuſculum* it ſelf, out of which they ſeemed to be formed, and tho' for the moſt part the greateſt Appearances had been within 20 Degrees of the North on each ſide, yet at due N. W. there were very many conſiderable ones.

About 8, this *Crepuſculum*, (which had been conſtantly, though ſlowly, carried further from the North) had, with the upper part of its outer Limb, reached to about 10 or 12 Degrees beyond the Pole-Star towards the Zenith, being now above 30 deg. broad, with a circular Segment of black clear Sky to the North, of about 25 deg. when the whole *Crepuſculum* or Vapour was all ſuddenly formed into aggregate Bodies like Vapours, and gave one of the moſt pleaſing Appearances, that perhaps has been ſeen of this kind. The *Baſes* of the Cones ſeemed to reſt on the upper Limb of the Segment of clear Sky which was extended near 60 deg. on each ſide the N.) and the Vertices of the Cones,pointing all towards the Zenith, approached within a few Degrees of it, and terminating there, formed the greateſt part of a Semicircle incloſed, as it were, with Golden Palliſadoes,which ſhining all at once as bright almoſt as Flame, and being of a prodigious Length and Number, exhibited a moſt agreeable Spectacle.

This laſt *Phænomenon* convinced us, that theſe Cones were Collections of the very ſame Particles, whereof the *Crepuſculum* had conſiſted: becauſe when it appeared every where alike and equal, the great Stars ſhined through it but very faintly; whereas afterwards, thoſe Stars, that remained between the Cones, ſuddenly appeared very bright, whilſt thoſe, that were covered by them, could hardly be perceived: and indeed all the ſtreaming Lights this Evening ſeem'd to flow from this *Crepuſculum* downwards, as from a Fountain or Store, and not to ariſe from the Horizon, few approaching it nearer than 10 Degrees, and many not within 20 or 30 deg.

After this fine Appearance had continued about 2 Minutes, the Matter ſeemed to be exhauſted, and the Scene almoſt at an end, the ſtreaming ſhining Lights being moſtly extinguiſhed, and the remaining Parts of Vapour left, like broken Clouds; when the flaſhing Lights began to appear of a moſt prodigious Swiftneſs, both from N. E. and N. W. pointing to the Zenith, or a little more to the South. 'Twas obſervable, that over the Tracts, where theſe flaſhing Lights paſſed, the remaining parts of Vapour (which now lay ſcattered every where, like white broken Clouds) pointed, or ſeemed to have a Tendency, conformable to the ſame Motion; whereas, towards the due North, where no Flaſhes appeared, theſe whitiſh Clouds lay confuſed and irregular as before.

This continued about 20 or 25 Minutes, when the Wind began to ariſe a little at N. E. and the Scene was quite at an end, dark Clouds ſucceeding all over the North, and by nine a Clock we had a ſevere Storm of Snow.

N B. That all the time of theſe Appearances, many broken parts of the extinguiſhed Vapours, like white Clouds, were carried beyond the Zenith, ſome 50 or 60 deg. and others, even to the Horizon it ſelf, at S. S. W.

During the whole Continuance, there ſeemed to be a ſmall, eaſy breath of Wind, ſcarce perceivable, at N. N. E. which the Motion of the Clouds abovementioned alſo confirms, but as ſoon as it began to blow a little brisker, the remaining parts of the Vapour were all diſſipated.

An Account of the Weather both before and after this Phænomenon.

January 30. HARD Snow in the Morning, and Froſt all Night.

Jan. 31. and *February* 1, 2, 3 4. Pleaſant Sun-ſhine Days, very calm, but the Snow ſtill lying, and at Nights very hard Froſts.

5. Very violent Snow in the Morning, and ſome Thaw in the Afternoon, hard Froſt at Night.

6. Hard Froſt in the Morning, and the Wind exceeding cold and ſharp, but not hard; the afternoon pleaſant, Sun-ſhine and calm, but it froze all Day out of the Sun, and continued to do ſo all the Evening, and the Ground was ſtill covered with Snow.

7. Very hard Froſt in the Morning, and Froſt and Storms of Snow all Day. Wind N. N. W.

8, 9, 10, 11. Pleaſant calm Days, but hard Froſt, and very hard Froſt at Night.

12. Hard Froſt. Exceeding cold Wind at S. E.

The 12th at Night theſe Lights are ſaid to have appeared again, as alſo on *Saturday* the 18th, to a very great degree, but I ſaw them not; the Weather ſtill froſty with little Snow.

Feb. 22. At Night. Hard Snow.

23, 27. A Thaw and ſome Rain, and but little Froſt afterwards, only dry cold Winds, till the 27th, when the Froſt returned very ſharp, with exceeding cold Winds, at N. E. and S. E. for a Fortnight or more, without any Snow, and did a great deal of hurt.

VII. *Obſervations on the Muſcular Fibres of Fiſh.*
By Mr. Leeuwenhoeck, F. R. S.

Delft, April 11. 1721.

IN my Letter to this Honourable Society of the 24th of *January* laſt, I treated of the great Number of Veſſels that I had obſerved in the Fibres of the Muſcles. I now take the liberty of preſenting you with the following Obſervations, hoping there may be ſomething in them for your Entertainment.

It has been affirmed to me by ſeveral Perſons, and in particular, by a certain very learned Foreigner, that Nature, in all her various Productions, conſtantly obſerves the ſame Courſe and Manner of Operation. To this Aſſertion my Obſervations do by no means agree; neither thoſe, that I have made upon the Generation of Animals, and the Seeds of Plants; nor yet thoſe that I have made upon the Muſcles and Muſcular Fibres of different Animals, foraſmuch as the Muſcles of Fiſhes are not provided with any Tendons. On this occaſion, I have lately examin'd anew the Muſcles of the Cod-fiſh, upon which I have made the following Obſervations.

After the late Diſcoveries I had made of the ſmall Veſſels in the Muſcular Fibres of the Whale, the Ox, the Sheep, and the Mouſe, I was apt to imagine, that the ſame Fabrick would hold in the Muſcular Fibres of Fiſh likewiſe: but as this could not be certainly concluded, having at that time in my houſe a part of a freſh Cod, I cut off a piece of the Fiſh, with Intent to examine it with my Microſcope ſome Days afterward.

This piece of Fiſh I cut into ſmall Slices, ſome according to the length of the Fibres, and others directly acroſs them, in order to obſerve, whether theſe Muſcular Fibres were compoſed of great Numbers of ſmall Veſſels running according to the length of the Fibre. And in effect I found, that, when I had cut the Fibres dexterouſly through, there appear'd in the Microſcope as great a number of ſmall Veſſels running along theſe Fibres, as I had formerly ſeen in the Muſcular Fibres of a Whale.

But what appear'd to me the moſt remarkable, was, that in a great number of Fibres, in which I was not able to diſcover any Veſſels running according to their length, I obſerved abundance of ſmall Veſſels, which ſeemed to me to proceed from the Membranes encompaſſing the Fibres. For in one Fibre theſe Veſſels appear'd to come out of the Circumference, or circular Tunicle of the Fibre, and to paſs on to the oppoſite part of the Tunicle. And in another Fibre cut tranſverſly, I ſaw Veſſels ariſing from the Circumference, and dividing themſelves into ſmaller Branches about the middle of the Fibre; all which, as far as I could perceive, ended again in the Circumference of the Fibre. In one Fibre I ſaw at leaſt fifty of theſe Veſſels running through one another.

Upon this Diſcovery, I found I had been miſtaken in what I had at firſt imagin'd, which was, that the Veſſels, which aroſe from the Membranes, proceeded no farther, than juſt through the Tunicle of the Fibre, and ſo diſcharged the Fluid into the Fibre for its Nouriſhment. Whereas, now I perceiv'd, that the Veſſels, which aroſe from the Membrane, and entred into the Fibre, did not end there, but ſpread themſelves into ſmaller Branches proceeding every way from the inſide to the Tunicle of the Fibre. This caus'd me to think, that the nutritious Juice might circulate in theſe ſmall Veſſels, juſt as the Blood does in the Veins and Arteries; and that what the Muſcular Fibres received from them, might be no more than what ouzed thro' the Tunicles of theſe ſmall Veſſels, as I have ſaid of the ſmall Veſſels in Land-Animals, which have no other end than the Artery coming from the Heart, and the Vein terminating in the Heart; the Artery and Vein thus making one continued Veſſel.

Having now a great number of Fibres lying before me, in which I could ſee very plainly the Veſſels juſt now treated of, yet I could not diſcern in the tranſverſe Sections of the Fibres any appearance of thoſe Veſſels, which run along their length, and compoſe the greateſt Part of the Body of each Fibre. This I imputed to the cutting of thoſe Veſſels not directly acroſs, but ſomething obliquely, by which their Apertures had been cloſ'd in ſuch a manner, that I could not perceive them, nor the leaſt Reſemblance of them.

I have ſeveral times obſerv'd, between the Muſcular Fibres of the Fiſh, a great number of Veſſels lying together, which compos'd what is commonly call'd a Membrane, which Veſſels ſurrounded the Muſcular Fibres, and lay ſo many of them together, that the thickneſs of the whole *Faſciculus* of Veſſels was equal to that of a Muſcular Fibre, and, as I imagin'd, was afterwards to be diſpers'd in ſmaller Ramifications between the Fibres.

In taking a View of an entire Muſcle of a Codfiſh, and the Fibres of which it was compos'd, I found the thick end of the Muſcle to equal the Back of an ordinary Knife, and the thinner end not to exceed the thickneſs of a ſingle Fibre. Many of theſe Fibres are twice as long as the thickneſs of the Muſcle, and between the Muſcles lie what are commonly call'd Membranes, which are nothing elſe but a *Congeries* of Veſſels. Theſe Veſſels do not only run between the Fibres, but into the very Subſtance of every Fibre, as we ſee, when the Fibres are cut tranſverſly. By theſe Veſſels the Muſcular Fibres, and the entire Muſcles themſelves are ſo firmly bound together, that they ſerve inſtead of Tendons to one another.

In like manner the Muſcular Fibres are united to the Bones, by the Veſſels proceeding from the Bones, which Veſſels compoſe what in Land-Animals is called the *Perioſteum.*

In order to give a clear notion of what I mean, when I ſpeak of the Muſcles of a Cod-fiſh, I have here caus'd two of thoſe Muſcles to be delineated, lying cloſe together, as they are united to one another, and ſeparated from the other Muſcles, as is repreſented by *A*, *B*, *C*, *D*, Fig. 1. the Part deſign'd by *A*, *B*, *C*, having been cover'd with the Skin near the Head of the Fiſh. And it is my Opinion, that the Body of the Cod-fiſh, from Head to Tail, conſiſts of a continued *Series* of ſuch Muſcles.

I have likewiſe caus'd a ſingle Muſcle of the Fiſh to be repreſented by *E*, *F*, *G*, *H*, Fig. 2. where *E*, *H*, *G*, ſhews the Thickneſs of the Muſcle; and its thin Edge, which is no thicker than the Edge of a Knife, is marked by *E*, *F*, *G*.

When theſe Muſcles had lain ſeveral days upon a Paper, yet they were not dry'd ſo hard, but that I could ſplit them into thin Shivers, one of which is deſign'd in Fig. 3. between the Letters *I* and *K*, in order to ſhew the oblique Courſe of the Fibres, which are repreſented by ſmall Lines.

I now turn'd my Thoughts to the River-Fiſh, and particularly to the Pearch; and, as I imagin'd that an old Pearch had no greater number of Muſcular Fibres than a young one, but only that the Fibres encreas'd in bigneſs during the Growth of the Fiſh, and that the larger theſe Fibres were, the more plain and diſtinct muſt be the ſmall Veſſels, of which the Fibres were compoſed; I ſent orders, upon a Market-day, to bring me the largeſt Pearch that was to be found in the Market; and accordingly, I had one brought me the largeſt that I had ever ſeen, weighing three pounds and an half, and ſeventeen Inches and an half in length *Delft* Meaſure, which is the ſame with the *Rhinland*.

I cut off four pieces from this Fiſh, as two from the Back near the Head, and two others from the Belly in the thick part of the Fiſh, with deſign to make my Obſervations upon them the next Day.

Accordingly, at that time, I took a view of the Muſcular Fibres both in length and breadth, and found that the Fibres of this great Pearch were not ſo thick as thoſe of the Cod-fiſh. Upon cutting them thro' lengthwiſe, I ſaw the Apertures of the ſmall Veſſels in ſo great a number, as I could hardly have believ'd, if I had not ſeen them. I next cut ſome of the Fibres tranſverſly, and plainly found them thinner in this Pearch, than in a middling Cod-fiſh, and ſaw the ſmall Veſſels,that compoſe the greateſt part of the Bulk of the Fibre, lying as cloſe together, as ever I ſaw them in any kind of Fiſh or Fleſh.

To give a better notion of theſe Muſcular Fibres, and of the great number of ſmall Veſſels,of which they are chiefly compos'd ; I had,ſome Weeks before,placed ſome of them cut tranſverſly in pieces before a Microſcope, with deſign to have them drawn by my Painter, but had been obliged to defer it,by reaſon of the great Severity of the Weather. Theſe had been a little moiſtned before they were placed upon the Glaſs, in order to make them ſtick to it the better ; and I have cauſed a ſmall portion of theſe Muſcular Fibres of the Fiſh, cut thro' tranſverſly, after they were grown dry, and in their ſhrinking had been torn off from the ſmall Veſſels, that encompaſt them, to be repreſented, as at *L*, *M*, *N*, *O*, Fig. 4. The openings of the ſmall Veſſels in theſe Fibres were diſtinctly to be ſeen, but appeared in ſuch great numbers, and were ſo exceedingly ſmall, that it was impoſſible for the Painter to repreſent them any otherwiſe than by Points.

In this Figure are repreſented what we call the Membranes, but which indeed are nothing elſe but a *Congeries* of ſmall Veſſels, which not only ſurround the Fibres, but enter into their very Subſtance. Theſe, in the drying and ſhrinking of the Object upon the Plate, had been torn off from the Fibres, as may be ſeen at *P*, *P*, *P*.

When this was done, I put a ſmall Drop of Water, about the ſize of a Pin's head, on this ſmall Portion of Fibres, into which it immediately inſinuated and ſwell'd them to the ſame bigneſs, as when they were firſt laid upon the Plate: After which, I deſir'd the Painter to draw them, as they then appear'd to him, but to omit repreſenting the ſmall Veſſels, and only to deſign the Circumference of every Fibre, which he did, as appears at *Q*, *R*, *S*, *T*, Fig. 5.

I then clove a grain of Millet thro' the middle, and placing one half of it upon the Glaſs, beſide the portion of Fibres repreſented in Fig. 4. I deſir'd the Painter to obſerve the difference in bigneſs between the half grain of Millet-ſeed, and that portion of Fibres; who told me, that the half grain appear'd larger than the portion of Fibres, and ſo ſaid likewiſe a ſecond Perſon that view'd them. By which one may eaſily imagine, in how ſmall a ſpace that number of Fibres is comprehended, each of which conſiſts of ſo many Veſſels. I caus'd the Painter to repreſent the half grain of Millet at Fig. *6*.

I likewiſe made my Obſervations upon the Muſcular Fibres of a Pike, a Roach, Schar, and Flounder, in each of which I found the Fibres to be compoſed of ſmall Veſſels, like thoſe of a Cod and Pearch.

I had now a mind to examine the Muſcular Fibres of the Smelt, to ſee whether they were likewiſe compoſed of Veſſels, but not being able at that time to meet with any Smelts, it came into my Head to obſerve the ſame in dried Sprats. Accordingly I took the largeſt of theſe I could light on, which was a little more than five Inches in length, and I found that the Fibres of the Sprat were but little thinner than thoſe of the large Pearch ſpoken of before, and that the Veſſels of which the Fibres were compoſed, were nearly as numerous as in the Fibres of the Pearch.

From theſe Obſervations ſome Perſons may be apt to conclude, that the Muſcular Fibres of Land-Animals are of the ſame thickneſs with thoſe of Fiſh. But for the ſatisfaction of thoſe, who have not ſeen the Objects here ſpoken of, I have cauſed a ſmall portion of the Muſcular Fibres of a large Ox to be delineated, as they appear'd through the ſame Microſcope with the former, to ſhew the thickneſs of the dried Fibres, and the Veſſels that compoſe them, as is repreſented in *Fig.* 7. by *XYZ*.

I deſir'd the Painter to tell me, how many Veſſels he could ſee in the tranſverſe Section of one of theſe Fibres; who, after ſome pauſe, reply'd, That he counted five and twenty Veſſels in one Fibre.

Some time after this, I had a ſmall Smelt brought me, of the length of about two Joints of my Finger; and cutting ſome of its Muſcular Fibres tranſverſly, I placed them before a Microſcope, and ſaw not only that theſe Fibres were twice as thick as thoſe of an Ox, but likewiſe that they were provided with as great a number of Veſſels as the Fibres of other Fiſh.

Upon thus obſerving that the muſcular Fibres of Fiſhes were much larger than thoſe of Beaſts, I ſet my ſelf to conſider, for what Reaſons the great Creator of the Univerſe had made this Diſproportion between them. All the Satisfaction I could meet with, in my Thoughts upon this Subject was that, as the Fiſh ſwim in the Water, their muſcular Fibres need to exert very little force, in order to ſupport their Bodies in the Water, becauſe they are very nearly of the ſame ſpecifick Gravity with the Element, in which they ſwim. All the force they exert is in their progreſſive Motion, in purſuit of their Food. Whereas the muſcular Fibres of Land-Animals exerciſe a great force, not only in ſupporting and moving their own Bodies, but in carrying Burthens and other Labour they are put to. And we muſt allow, that the ſmaller and finer the Fibres are, to make a Body of any determinate thickneſs, the ſtronger will be the Compoſition, and therefore the Muſcles in Fleſh muſt be ſtronger than thoſe of Fiſh. But this I leave to better Judgments.

It juſtly claims our Admiration, to ſee the wonderful and amazing Structure of theſe minute Parts in the Muſcles of Beaſts and Fiſh, which never enter'd into our Thoughts; and no doubt there are many other ſurpriſing things there incloſed, which will perhaps for ever eſcape our Penetration.

I am, &c.

P. S.

I Happen'd yeſterday to take up a boil'd Grey Pea, out of which I took a little of the mealy Subſtance, and laid it before a Microſcope, where it appear'd to conſiſt of ſuch like Parts as are found in Rats Dung, every one of which Parts conſiſted of a great number of very ſmall Particles. But I could not diſcover any Membranes enveloping thoſe Parts, from whence I concluded, that thoſe Membranes were deſtroy'd and diſſolv'd by the hot Water.

Upon this, I took another Grey Pea, which had not been boil'd, and cut it into very thin Slices, when I not only ſaw the Membranes, in which the Parts of the mealy Subſtance had been incloſed, but found likewiſe, that thoſe Membranes conſiſted of nothing elſe but a great number of very ſmall Veſſels, like the Membranes, as they are commonly call'd, which ſurround the Muſcles and muſcular Fibres in Beaſts and Fiſh.

I intend to proſecute this Subject farther for my own Diverſion.

VIII. *Obſervations upon the Seeds of Plants. By the ſame. Tranſlated by* John Chamberlayne, *Eſq;*

Delft in *Holland*, 27. *June*, 1721.

I Have often turn'd my Thoughts to the obſerving the ſo called Membranes, in which the Subſtance of Meal, or Flower, is incloſed, like little Packets in Cells or Boxes, which is alſo the caſe of all kinds of Beans, Peaſe, Wheat, Barley and other Grain. I at length, with aſtoniſhment, diſcover'd very plainly, that what I call the Membranes, were endued with an unſpeakable number of little Holes, thro' which, in many places, one might perceive the Light; which Holes we muſt ſuppoſe to be nothing elſe but little Veſſels, which had been torn or cut off, and which do partly compoſe the Membranes, which I call Little Cells, and which partly ſerve for the Production of the *Farina*, of which there are an infinite number of Particles in a Pea or Bean; which, as ſmall as they are, I imagine that each of thoſe mealy Particles receives its increaſe from a little Veſſel, which proceeds from the foreſaid Cell, and that thoſe Veſſels are imperceptible through their Smallneſs.

Theſe Veſſels, of which the little Cells, or Caſes, do moſtly conſiſt, are more eaſy to be diſcover'd in Beans and Peaſe, than in any ſort of Legumens or Grains; but in Wheat the Veſſels are difficultly tra-ced in the Cells, and I have been obliged to make very many Obſervations and Experiments, before I could fully ſatisfy my ſelf, that I ſaw the torn or broken Veſſels; the Reaſon of which is, that the little Veſſels, of which the Cells or Skins of the Grains of Wheat are compoſed, are exceeding thin and brittle.

Moreover, I have found, upon obſerving the Veſſels, of which the Cells are compoſed, that ſeveral of the Globules in Wheat were broken in pieces in the Operation, and that in one of thoſe ſingle Globules, there were other ſmall Globules encloſed.

I have likewiſe obſerved that the Membranes, or little Cells, in *Barley*, in which the Globules, or Parcels of the Meal are ſhut up, and receive their increaſe, are thicker and ſtronger than thoſe of *Wheat*.

Although I conclude, that almoſt all Seeds and Grains, as well as their Membranes, or Skins, are of one and the ſame Texture and Configuration, yet for Experiment ſake, I took a large *Almond*, and cut off ſeveral thin Slices from it, and dug out of thoſe Slices, as well as I could, the Subſtance that lay in the little Cells, and viewing them, as nicely as poſſible, with a Microſcope, I obſerved that thoſe Cells, in which the Oyl of the ſaid *Almond* was for the moſt part contain'd, conſiſted alſo of nothing but little Veſſels.

My Intentions were indeed, if it had been practicable, to view the ſmaller ſorts of Seeds, in order to find out, whether the little Cells, in which the farinaceous matter might lie, were likewiſe compoſed of ſmall Veſſels: but I changed my mind, believing

that

that my Endeavours therein would be ineffectual, by reaſon of the Smallneſs of thoſe little Cells, and conſequently of the Veſſels, of which thoſe Cells were made up; though I do not queſtion but what we diſcover in the greater Seeds, is analogous to the Structure of the ſmall ones.

Now when I perceiv'd, that the before-mention'd little Cells proceeded from the Bark, or Skin, which ſurrounds the Kernel of the Seed, or Grain, I was thinking that, as the mealy Subſtance receives its increaſe from the Veſſels, which are in the little Cells, and as the Plant is formed between the Cells, during the time that the Seed lies in the Earth, and as the little Orifices in the Skin of Animals and Fruits, are formed in order to diſcharge thereby the Superfluity of their Moiſture, and do ſhut in ſuch a manner, that no Moiſture, nor common Air, can get into the ſame, as I have formerly advanced: So on the contrary, the Orifices of Seeds are ſo form'd, that many of their little Veſſels do admit Moiſture to paſs inwards, and accordingly Water is driven into them by the preſſure of the Air, and cauſes the Seed to ſwell; upon which, a Warmth and Fermentation ſucceeding in the Seed, it requires a greater ſpace, and by the particular Formation of the Particles, which lie in the Cells, and which have derived their Encreaſe from the Cells, the mealy Subſtance, of which they conſiſt, is partly driven out of them into the body of the young Plant, which by this means encreaſes ſo much in bulk, that the Root is now able to ſupply it with Nouriſhment from the Earth, at which time the Seed is found to be diminiſhed in its bigneſs.

As often as I have view'd Seeds, for ſeveral Years paſt, with the Microſcope, yet I never imagin'd, that the little Cells were endued with ſo many Veſſels, tho' I have often been conſidering, how the Intruſion of the Particles of the Meal, or Flower, into the Membranes was effected ; nor ſhould I ever have attained thereto, but by continual Labour in the inveſtigation of things, which are concealed from our naked Eyes, and towards which I have a much greater inclination, than what I obſerve in moſt other Men.

IX. *Extract of a Letter from Dr.* Brook Taylor, F. R. S. *to Sir* Hans Sloan, *dated* 25. June, 1714. *Giving an Account of ſome Experiments relating to Magnetiſm.*

AFter having given an Account of an Experiment made with the large Magnet in the Repoſitory of the *Royal Society*, (which Experiment is deſcribed in *Philoſoph. Tranſact.* No. 344. Article 4.) The Letter goes on with the ſame Subject as follows. -- If it were known what point within the Stone, and what point in the Needle are the Centers of the Magnetical power, it would be eaſy to find the true powers of the Magnet at all the diſtances obſerved. For want of that Knowledge, I have computed the Forces from the Center of the Needle, and the Extremity of the Loadſtone, and find, that at the diſtance of nine Feet, the Power alters faſter, than as the Cubes of the diſtances, whereas at the diſtances of one and two Feet, the Power alters nearly as their Squares. To try whether the Law, by which the Magnetiſm alters, could be reduced at all diſtances to any one certain power of thoſe diſtances, I ſought thoſe points in the Needle and Stone, which being uſed as the Centers of the power, might have that property. But in that caſe, I found the Center of the Stone muſt be carried quite out of its Figure, to make the diſtances large enough for this purpoſe. From whence it ſeems to appear, that the power of

Magnetiſm does not alter according to any particular power of the diſtances, but decreaſes much faſter in the greater diſtances, than it does in the near ones.

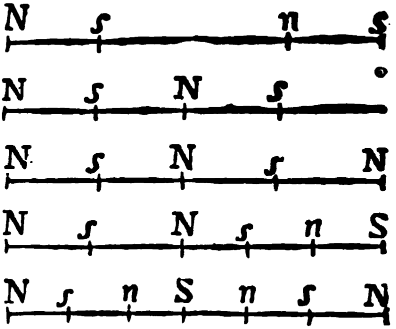
This ſeems to be confirmed by other Experiments I made. The firſt Experiment was thus; I made a Needle $\frac{3}{4}$ of an Inch long, of very fine Steel-wire (a Foot length of which weigh'd but a Grain) which I lengthen'd by ſticking a light piece of Ruſh to it, ſo that I could obſerve the Direction of the Needle in all the trials with a *Radius* of two Inches. Inſtead of a Magnet I uſed a touch'd Needle of Steel-wire, which I ſet on a perpendicular to the Horizontal Plane I made the Obſervations on by means of a Frame I made to tranſport it from one place to another; the North end of the Needle being placed downwards, and made a little ſharp, that it might mark the Paper it was ſet upon in every poſition, by preſſing the top of the Needle gently with the Finger. The Obſervations were made in this manner; after having taken notice of the natural direction of the ſmall Compaſs Needle, I brought the perpendicular Needle as near to it as I conveniently could, ſetting it in ſuch a manner, that a Line from the upright Needle to the Center of the Compaſs might be perpendicular to the Compaſs Needle. Then obſerving the ſame caution (which was convenient to make the Center of the Compaſs ſerve ſufficiently well to be eſteem'd its Center of Power) I placed the upright Needle at ſeveral greater diſtances, every time marking the place in the manner already deſcribed, and obſerving the Variation of the Compaſs. By this means I got a Curve pretty regularly and fairly drawn by points on the paper. And by examining this

Curve, compared with the Variations of the Compaſs correſponding to its reſpective points, I found that the Magnetical power decreaſed faſter at the greater diſtances than at the nearer. It is of little uſe to be very particular in the account of the ſeveral Obſervations. I ſhall only take notice, that at about two Inches and a quarter diſtance, the Force did not alter ſo faſt as the Squares, and at ten Inches diſtance (where the Variation was one degree only) it alter'd faſter than the Cubes, the Index of the Power being about $3\frac{1}{4}$. The Needle of the Compaſs was ſo ſhort, that to ſuppoſe its Center of Force to be either in the middle or at the extremity of it, would not alter the Index of the Powers of the diſtances $\frac{1}{10}$ of an Unite.

I made another Experiment to the ſame purpoſe, with a Compaſs Needle made of a ſlight piece of Straw, with a ſmall piece of Steel-wire faſten'd to one end of it, which was always kept in the ſame poſition, being balanced between two perpendicular Needles, one of which was moveable, and the other fix'd. The Event was much the ſame as in the former Experiment.

Endeavouring to find the true Poles, or Centers of the magnetical Power in touch'd Needles, I made a Needle of two Inches long, of the fine Steel-wire, which I touch'd with the South point of a ſmall capt Loadſtone, applying the point of the Cap only to the Extremity of the Needle, without drawing it along. The Needle ſo touch'd, being laid gently on the Surface of a ſtagnant Water, floated. I then applied to it ſucceſſively the two ends of a touch'd

Needle, as near as I could, without letting the Needles touch. The reſult was, that the floating Needle reſted under the reſpective Poles of the other Needle mark'd with the ſmall Letters *s*, *n*, *s*. So that by one Touch with the Loadſtone, which gave the Needle a North-pole at *N*, where it was touch'd, it acquir'd three other Poles, *s*, *n*, *s*, which we may not therefore improperly call its conſequential Poles. Having diſcover'd theſe conſequential Poles, I made ſome other Experiments to diſcover more of the Nature of them, as they are deſcrib'd in the Scheme annexed. The Needles were all of them two Inches long, made of the ſame fine Steel-wire, and the Letters *N*, or *n*, and *S*, or *s*, denote the Character of North or South belonging to the points mark'd; the great Letters ſignifying the points the Loadſtone was applied to, and the ſmall Letters ſhewing the conſequential Poles.



There are two other Experiments deſcribed in the ſame Letter, relating to the Attraction of Fluids, one of which (*viz.* that of the *Hyperbola*, made by the Surface of the Water between two Glaſs-planes) being already deſcribed in theſe *Tranſactions* (No. 336.) we ſhall only tranſcribe the Account given of the other.

I took ſeveral very thin pieces of Fir-board, and having hung them ſucceſſively in a convenient manner to a nice pair of Scales, I tried what Weight was neceſſary, (over and above their own, after they had been well ſoak'd in Water) to ſeparate them at once from the Surface of ſtagnating Water. I found 50 Grains to ſeparate a Surface of one Inch ſquare; and the Weight in every trial being exactly proportional to the Surface, I was encourag'd to think the Experiment well made. The diſtance of the under Surface of the Board from the Surface of the ſtagnating Water, at the time they ſeparated, I found to be $\frac{16}{100}$ of an Inch; though I believe it would be found greater, if it could be meaſured at a greater diſtance from the Edge of the Board, than I could do it, the Water riſing a little before it came quite under the Edge of the Board.

FINIS.

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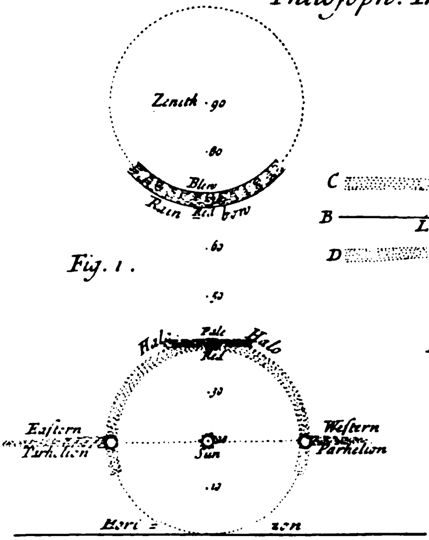

Fig. 1.

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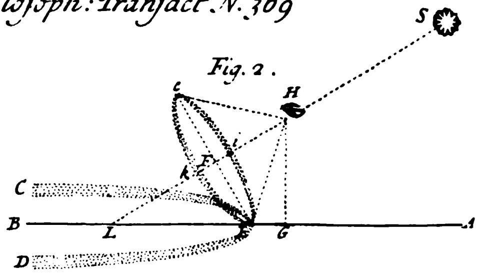

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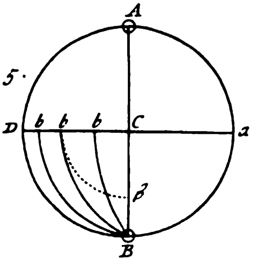

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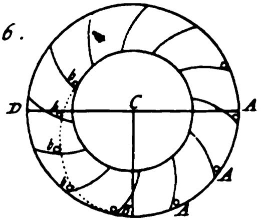

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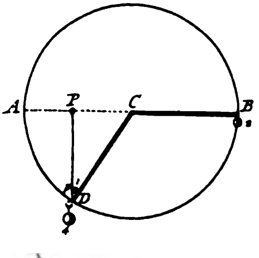

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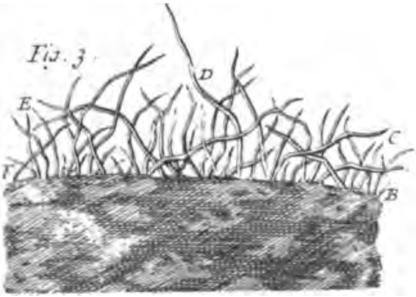

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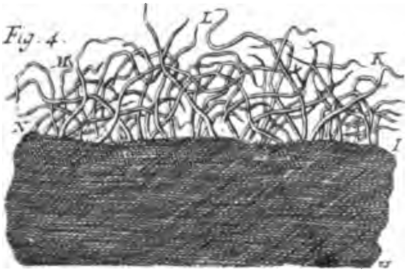

Numb. 369.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *September*, *October*, *November*, and *December*, 1721.

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I. *On the Method of determining the Places of the Planets by obſerving their near Appulſes to the Fixed Stars. By* Edmund Halley, *LL. D. Aſtron. Reg. & Reg. Soc. Soc.*

OF all the Cæleſtial Obſervations that have hitherto been made, none are ſo capable of perfect Exactneſs, as the near Appulſes of the Moon and Planets to the Fixed Stars, ſuch as lately we had of *Jupiter* to two ſmall Stars in *Gemini*, and of *Mars* to the Forehead of the *Scorpion*; for though the Places of the Stars have not as yet attained an ultimate Preciſion, yet theſe Sorts of Obſervations are ever good, and the Places of the Planets are thereby aſcertain'd, in Proportion to the Correctneſs of the Catalogues that may hereafter be made: But the ordinary Number of the Stars, with which the Planets may be thus compared, being ſmall, the Opportunities of obſerving are conſequently rare: Whence appears the great Uſe of a full Catalogue of Teleſcopical Stars, at leaſt within the Limits of the *Zodiack*; *viz.* that thereby theſe Opportunities may be more frequent: And wherever ſuch Obſervations have formerly been made to theſe ſmall Stars, we may be enabled to find them out, and by determining their Places, to be certain of the Places of the Planets alſo: Of which I have given a notable Inſtance in finding the Place of the great Comet of 1680, in its firſt Appearance, even before it had a Tail viſible to the naked

Eye, of which an Account is given in *Numb.* 342. of the *Tranſactions.* And ſince the *Royal Obſervatory* at *Greenwich* has been put under my Care, I have endeavoured to put myſelf into a Condition to ſupply the many and great Vacancies to be met with in the preſent Zodiack; and particularly I have ſought out and ſettled the Places of two Teleſcopick Stars, to one of which, *Jupiter* was obſerved to apply by *Galileo* at the Beginning of *March* 1610, *New Stile*, and which is the very firſt Obſervation of that kind that was made with the Teleſcope. (*a*) On the 28th of *February*, one Hour after Sun-ſet, a ſmall fix'd Star was in Conjunction with the fourth Satellite, (as it ſince appears to have been) being then Eaſtwards of the Planet. The next Day, *Mart.* 1°. at the ſame Hour, the Center of ♃ was in the Angle of an equilateral Triangle with the fourth Satellite and the Star: And again, *March* 2°. *Jupiter* being retrograde, had paſt the Conjunction of the Star, and a Line from the Star, perpendicular to that of the Satellites, fell on the firſt Satellite then two Minutes to the Weſt of the Planet, and in Latitude the Star was more Southerly than the Satellite eight Minutes. This Star, by the Direction of the Place of *Jupiter* at that Time, I found out, and, by comparing it with others in the Catalogue, having nearly the ſame Declination, I ſettled its Place in ♊ 13° 24′ ½ to the Time of the *Britiſh* Catalogue with 0° 25′ South Latitude.

Another remarkable Obſervation of *Saturn* is recorded in *Riccioli* (*b*), ſaid to have been made at *Modena* by the Marquis *Malvazzo*, on *July* 3° *N. S.* 1662, when the Eaſtern *Anſa* of *Saturn* touched a fix'd Star. By the then Place of *Saturn*, I look'd out for this Star, to which

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(*a*) Nuncius Syder, *pag.* 27. Edit. *prin.* 1610.

(b) Aſtron. Reform. [illegible] 286

which *Saturn* is at this Time very near, and after the ſame Method I ſettled its Place, *ineunte Anno* 1690, (the Epoch of the *Britiſh* Catalogue) in 29° 34′ of *Scorpio* with 2° 0′ ½, North Latitude. By this it will appear, how defective the obſerved Place of *Saturn* is ſtated in *Riccioli*, there being above ſeven Minutes erred in the Latitude thereof.

II. *Obſervation of a* Parhelion, Oct. 26*th*, 1721. *By the ſame.*

THIS Morning, 26th of *October*, being on the River coming up to *London*, about half an Hour paſt Ten, the Sun being then about twenty Degrees high, I obſerved a Circle about the Sun, which is by no means unuſual, when the Air in chilly Weather, ſuch as it is now, is replete with ſnowy Particles; which Circle was of the Size it always appears in, about 23 Degrees from the Sun, and faintly ting'd with the Colours of the *Iris*. When this Circle happens, I always look out, to ſee whether any other of the Phenomena that ſometimes attend it did at that Time appear, ſuch as *Parhelia*, and other colour'd Circles, concentrick with the Sun, and ſometimes, as once I ſaw it, excentrick; as alſo a white Circle round the *Zenith*, in equal Altitude with the Sun: But this Time, the Air being thickned with a hazy Vapour, and the Smoke of the Town, I could only ſee to the Eaſtward a luminous white Patch, which for about twenty Minutes ſhone through the thick Air very conſpicuouſly, of about two Degrees diameter, as near as I could eſtimate it, and about the ſame Altitude with the Sun: and from it, towards the Sun, there ſeemed to proceed a long white Tail, much narrower than the Mock-Sun, but which I took to be a Segment of the white Circle which I once ſaw entire in *London*. Had the Air been clear, I doubt not but much more of the Phenomena of the *Parbelia* might this Time have been obſerved: and I hope, that from our Neighbourhood ſome Member of the *Society* may furniſh us with a fuller Relation. But how to explain theſe Appearances, and account for the Magnitude of theſe Circles, is what ſeems ſtill wanting.

III. *An Account of two* Mock-Suns, *and an* Arc of a Rainbow inverted, *with an* Halo, *and its brighteſt Arc, ſeen on* Sunday *and* Monday, Octob. 22, & 23. 1721. *at* Lyndon, Comitat' Rutland, *communicated by the Rev. Mr.* William Whiſton, *M. A. ſometime Profeſſor of the Mathematicks in the Univerſity of* Cambridge.

ABOUT Ten o'Clock in the Morning, on *Sunday, Octob.* 22. 1721. being at the Houſe of *Samuel Barker*, Eſq; of *Lyndon* in the County of *Rutland*, after an *Aurora Borealis* the Night before (Wind W. S. W.) I ſaw an Attempt towards two *Mock-Suns*, as I had done ſometimes formerly, of which I immediately inform'd Mr. *Barker*, though without any great Expectations of what followed. About $\frac{1}{2}$ or $\frac{3}{4}$ of an Hour after, I went to view the Heavens, and then found the Appearance compleat; and when Mr. *Barker* and others of the Family were call'd, we all ſaw it, and all ſaw indeed what we had none of us ſeen before; I mean two plain *Parhelia*, or *Mock-Suns*, tolerably bright and diſtinct; and that in the uſual Places, *viz.* in the two Interſections of a ſtrong and large Portion of an *Halo*, (*Fig.* 1.) with an imaginary Circle, parallel to the Horizon, paſſing through the true Sun. I call this Circle here *imaginary*, becauſe it was not itſelf viſible, as it ſometimes has been at ſuch Appearances. Each *Parhelion* had its Tail, of a white Colour, and in direct Oppoſition to the true Sun; that towards the Eaſt was 20 or 25 Degrees long; that towards the Weſt about 10 or 12 Degrees; but both narroweſt at the remote Ends. The *Mock-Suns* were evidently red towards the Sun, but pale or whitiſh at the oppoſite Sides, as was the *Halo* alſo. Upon caſting our Eyes upward, we ſaw an Arc of a curious *inverted Rainbow*, about the Middle of the Diſtance between the Top of the *Halo* and our *Vertex*. I mean this, when Allowance is made for the uſual Inequality, that appears between the ſame Number of Degrees, nearer to and remoter from that *Vertex*. This Arc was as diſtinct in its Colours as the common Rainbow; and, with the like Allowance as before, of the ſame Breadth. The red Colour was on the Convex, and the blue on the Concave of the Arc; which ſeemed to be about 90 Degrees long: Its Center in or near our *Vertex*. On the Top of the *Halo* was a kind of inverted bright Arc, though its Bend was not plain. The lower Part of the *Halo* was among the Vapours of the Horizon, and not viſible. The Angles, eſpecially as more exactly meaſured on *Monday*, near Noon, when the ſame Appearance return'd again, but more faintly, were as follows: Sun's Altitude $22^{\circ}\frac{1}{3}$; perpendicular Semidiameter of the *Halo* $23^{\circ}\frac{1}{3}$; Diſtance of the *Rainbow* from the Top of the *Halo* $23^{\circ}\frac{1}{3}$; Semidameter of the Arc of the *Rainbow*, if our *Vertex* be ſuppos'd its Center, 21°. The *Phænomenon* laſted each Day for an Hour and an half, or two Hours. What was moſt remarkable on *Monday* was that the Wind, which on *Sunday* had been almoſt inſenſible, was now become ſenſible,and changed to N. N. E. that the *Halo* was ſenſibly become *oval*; its ſhorter Axis parallel to the Horizon; and the two *Mock-Suns*, which were then but juſt viſible, eſpecially that on the Eaſt, were not in the *Halo*, but a Degree or two without it, which I aſcribe to the unuſual Shortneſs of the Horizontal Diameter; which Poſition of the *Mock-Suns* does not appear to have been hitherto taken Notice of by any, though it was now very ſenſible.

On *Thurſday* Morning, *Octob.* 26. as I was coming in the *Northampton* Coach towards *London*, about 9 o'Clock, the *Halo* returned larger and clearer than before; and the two *Mock-Suns* juſt attempted an Appearance therein, as on *Sunday*; but the Air becoming thicker and thicker towards Rain, I ſaw them no more. I add nothing to this Account, but only, that *Aug.* 30. before, I ſaw at the ſame Place *Rutland* a remarkable *Halo*, whoſe upper Part had its inverted Arc reddiſh within, and pale without, but brighter and more vivid than ever I ſaw in my Life: That we had there, *Sept.* 11. in the Evening, the lighteſt and moſt remarkable *Aurora Borealis*, with its unaccountable Motions and Removals, that ever I ſaw; excepting that original one, *March* 6, 171$\frac{5}{6}$: That it was ſeen in *Northamptonſhire*, at the *Bath*, and elſewhere: That the *Vertex* of the Columns which ſhot upwards, was not our *Vertex*, but evidently 15 or 20 Degrees diſtant towards the South; and that the Wind was in *Rutland* North, as I obſerved myſelf; at the *Bath* Weſt, as Mr. *Molyneux* obſerved; and, as I am inform'd by Sir *Robert Clarke*, in *North-*

amptonſhire South; all at the ſame Time, which deſerves particular Reflection. But then, if any Reader expects here the *Solution* of all theſe *Phænomena*, he is to know, that as to theſe *Northern Lights*, Dr. *Halley* has communicated his Thoughts to the Publick in the *Philoſophical Tranſactions* ſoon after the firſt Appearrance; and I communicated mine about the ſame Time in a ſmall Pamphlet. And as to the *Halo's*, *Mock-Suns*, *Inverted Arcs of Rainbows*, and other *Phænomena* of the like Nature, Monſ. *Huygens* has moſt accurately explained them in his *Poſthumous Works*, from *p.* 293 to *p.* 366. and Sir *Iſaac Newton* himſelf has touched upon them in his *Opticks*, 1ſt *Edit.p.* 134. to which the *Inquiſitive Reader* may have Recourſe for his Satisfaction. Only if any enquire farther, Why the *Northern Lights* have of late been ſo unuſually frequent, I muſt declare, I am far from having ſatisfied myſelf, and ſo ſhall not pretend to offer any Thing for the Reader's Satisfaction.

London, Nov. 6. 1721,

Will. Whiſton.

IV. *Obſervations upon the Generation of Plants, in a Letter to Sir* Hans Sloane, *Bart. Pr. Coll. Med. By* Patrick Blair, *M. D. F. R. S.*

Boſton, Dec. 31. 1721.

Honoured Sir,

IT is no ſmall Satisfaction, that what I advanced in my *Botanick Eſſays* is now ſo fully confirm'd by Experiments made by ſome curious Gardeners, among whom is Mr. *Philip Miller*, who writes me,

November 11, 1721.

1. That in Purſuance of my Advice he ſeparated the Male Plants of the Spinage from the Female; the Conſequence was, that the Seeds did ſwell to the uſual Bigneſs; but when he ſow'd it, it did not grow afterwards. He ſearched into the Seed, and found it wanted the *Punctum Vitæ*, which perhaps might have been the Caſe with Mr. *Geoffroy*; but if not, the female *Embryones* might have been impregnated another Way, as he experimented with twelve Tulips, which he ſet by themſelves about ſix or ſeven Yards from any other, and as ſoon as they blew, he took out the *Stamina* ſo very carefully, that he ſcattered none of the Duſt, and about two Days afterwards, he ſaw Bees working on Tulips, in a Bed where he did not take out the *Stamina*, and when they came out, they were loaded with the Duſt on their Bodies and Legs: He ſaw them fly into the Tulips, where he had taken out the *Stamina*, and when they came out, he went and found they had left behind

them ſufficient to impregnate theſe Flowers, for they bore good ripe Seed; which perſuades him, that the *Farina* may be carried from Place to Place by Inſects, and when they happen upon a Flower, whoſe *Uterus* is capable to be impregnated by ſuch a Duſt, it may be thus effected.

I am of Opinion, this will not ſuit with Mr. *Morland*'s Scheme. For tho' we may ſuppoſe the *Stamina* of every Flower to be loaded with a due Proportion of the *Farina*, yet this accidental Conveyance of it to a neighbouring Flower, may be rather leſs than greater than is neceſſary: So that, if wanting, then thoſe *Embryones*, which had not received its determined Particle into their Boſom, muſt be defective in Bulk, or barren in growing, but here all were equally fill'd.

2. By a Second Letter, *October* 19. 1721. he informs me, that he bought a Parcel of *Savoy* Seeds of a Neighbour, which he ſowed, and planted out the Plants; but was ſurprized to ſee the Production: For he had half of them red Cabbages, and ſome white Cabbages, and ſome Savoys with red Ribs, and ſome neither one Sort nor other, but a Mixture of all Sorts together in one Plant. He went to the Gardiner and told him his Tale, who ſhew'd him, that he was in the ſame Condition, but did not know how it ſhould come to paſs, for he was ſure he took ſpecial Care in ſaving of the Seed. Being ask'd how and where he planted them for Seed, he ſhew'd him them under a *South-Weſt* Hedge, and told him the Manner in which he planted them. Firſt, a Dozen of white Cabbages, then a Dozen of Savoys, and then a Dozen of Red. Then he immediately thought how it came to paſs, by the *Effluvia*

impregnating the *Uterus* of one another; and it is very common for our Gardiners to plant white and red Cabbages together for Seed, and they are as often diſappointed by having a Degeneracy of both Kinds, which they attribute to the Soil, and think that is the Cauſe: They ſend to *Holland* for a freſh Supply of Seeds, and ſay our Soil will not continue that Sort Good. He told them his Opinion, and they laugh at him for it, and will not be turn'd out of their Road, although they ſhould have never ſo many Experiments ſhew'd them.

This Experiment is a moſt convincing Argument for the *Effluvia*; for did each Grain of the *Farina* enter the *Piſtillum* to its proper *Uterus*, this mongrel Kind would never be produced. For if the individual Plant be in each Grain of the Male *Farina*, how can it be ſo far diſmember'd, as that one Part ſhall go to the making up of the Ribs of red Cabbage, and another to compoſe the reſt of a Savoy Plant. Analagous to this, is what I lately obſerv'd in a Spaniel Bitch, of ſo good a Kind, that when ſhe became proud, Care was taken to let her have good Dogs. The Litter ſhe produced, conſiſted of Puppies ſome Piebald, like one of the Dogs that had lin'd her, of the ſame Shape, Colour and Spots; others like another; and a third partaking of both, with Spots from the Bitch interſpers'd. This is a farther Confirmation of what I have advanc'd, Eſſay 4. where Page 310. I only aſſert, that ſeveral Fœtus's partake equally of Male and Female; but here two Males concur with one Female in the Compoſition of a fourth Body, made up of all the three: And one Seed produces a Cabbage conſiſting of three different Species, which could never happen, did theſe organiz'd *Animalcula*, or Granules of the *Farina*, become a *Fœtus*, or contain the *Folia Seminalia* of a Plant. This methinks is ſufficient to anſwer what the ingenious Mr. *Bradley* has ſo ſtrenuouſly contended for, *Works of Nature, p. 9. & ſeq.* But ſince that worthy Gentleman has not thought fit to anſwer what I have already advanc'd upon that Subject, I may hereafter anſwer his Objections more at large.

I could deſcant yet more upon this Obſervation, and conſider how far this may lead us into the infinite Variegations and Stripes, in not only annual Flowers, ſuch as Poppies, *Conſolida Regalis*, and Bottles, but alſo in perennial Roots; ſuch as Auricula's, Couſlips, *&c.* of a lower Size, which is hinted by Mr. *Bradley*; he having received that Notion from the ingenious Mr. *Du Bois*, as I have been credibly informed; and in Plants of a larger Size, not of a Bulbous, but Carnous Root, ſuch as Columbines; where there is a vaſt Variety: And in this Plant it is moſt eſpecially to be obſerved, that though the indigenous one, from which all the other ſeem only to be Variations, and not determinate Species, be of a blue Colour, conſiſting of ten Alternate Petala, *viz.* five corniculate, and five plain; yet into how many other Kinds of Flowers is it ſubdivided; ſuch as pale yellow, with bluiſh red, purple, dark Stripes vaſtly double, blue, blackiſh red, *&c.* Some with Corniculate *Petala*, and ſome only with plain, and how in ſingle Flowers it imitates all the Colours we ſee Pigeons endow'd with. I ſay it is worthy of Conſideration, whether the *Farina* may do this, ſince I do not underſtand there has been much Art uſed in making theſe Flowers break, as Tulips, or to cultivate a Set of Breeders; but that a richer Soil may produce a double Flower; and a ſuitable Loam may produce the Variety of Colours; the *Farina*

from

from ſeveral Flowers may occaſion the Stripes, and the *Stamina* ariſing from the plain *Petala*, rather than the *Cornicula*, pouring out the *Farina*, may cauſe the Flowers with the plain *Petala*. So that were I to extend this to a great many other Plants, and were there proper Obſervations made upon them, conſiderable Improvements might be made upon this Doctrine of the Sexes of Plants. For after the Flowers, we come next to the Variegation of the Seed of ſome Plants, particularly the *Phaſeoli*, whoſe various Spots and Colours, and even the Bigneſs too, may very much depend upon the *Effluvia* from the *Farina*, when ſeveral Kinds are ſown together. For do but conſider three plain Colours, a White, Red, and dark Blue, and you may obſerve how many Deſcendants, and what a Variety of Spots may proceed from them. The Lupines alſo in ſome Meaſure may be brought in here, and I know not but that the *Medica Cochleata Falcata Lunata*, may be multiply'd in its Variations after the ſame Manner. But it is Time to proceed to another Experiment of my Correſpondent Mr. *Miller*.

Being perſuaded to it by an ingenious Gardiner, he pull'd off all the Male-Flowers of ſome Melon Plants ſo ſoon as they appeared ; but inſtead of finding, as his Friend informed him, that theſe Flowers exhauſted the Nouriſhment from the Fruit ; he found that, without theſe Flowers, none of the Melons would grow, ſo that he was deprived of the Fruit which he expected.

As this Experiment is a plain Indication of the Neceſſity of the *Fariaa*, ſo it confirms the Uſe I have aſſign'd to the Leaves, *viz.* that by entring the Capillaries of the Leaves, and returning, the nutritive Particles may be more attenuated : So here, the *Petala* of the Male-Flowers may ſerve for the ſame Purpoſe, for by the Largeneſs of the *Tubuli* in theſe *Pomiferæ Scandentes*, a groſs viſcid Sap is received, which even the Leaves themſelves are not ſufficient to attenuate, ſo as to be fit for compoſing the more ſubtile Part of the Fruit; until by repeated Circulation through the *Petala* of the Male-Flowers, it may be render'd fit for ſuch a Purpoſe. Indeed, the Female-Flowers upon the Top of the *Rudimentum Fructus*, may in ſome Meaſure ſerve for this Purpoſe. But as the Male-Flowers are, generally ſpeaking, more numerous than the Female, ſo their being remov'd muſt deprive the *Embryones* of a very great Aſſiſtance towards its being perfected: I may add, that the Orifices of the Pedicles, when the Flowers are pull'd off, muſt loſe ſo much of the Sap, that the whole Plant muſt be thereby ſo impoveriſhed, as not to be able to bring forth the deſign'd Fruit; all this, beſide the Want of the conſiderable Supply of the *Farina Fæcundans*.

I deſign'd to have given a few Thoughts concerning the Variegation of Leaves and Flowers, being unwilling to admit of Mr. *Bradley*'s Sickneſs or Weakneſs of the Sap: But I ſhall reſerve that to a more convenient Opportunity, being at preſent intent upon making ſome farther Improvements upon the Generation and Nouriſhment of Plants, which I hope to have well confirm'd by Experiments made by my good Correſpondents the Gardiners, eſpecially this Mr. *Miller* and my ſelf. I ſhall expect your Sentiments of theſe, which will be a great Encouragement to proceed, to

Your Moſt Obliged,

P A. B L A I R.

V. *Obſervation of an extraordinary Height of the Barometer*, December 21. 1721. *By* Mr. George Graham, *Watchmaker*, F. R. S.

Jan. 11th, 1721.

UPON *Thurſday* the 21ſt of *December* laſt, obſerving the Barometer much higher than uſual; that Evening, between Seven and Eight a Clock, I fill d a Tube with very clean Quick-ſilver, and found the Height a little to exceed $30,7\frac{1}{2}$ Inches. By Eight the next Morning, a Wheel-Barometer, which hung in the ſame Room, had riſen One tenth of an Inch higher than it was the Night before, when the Experiment was made; at Ten a Clock, One fifth of an Inch more: At which Time it was at the higheſt, being a little above $30,8\frac{1}{2}$ Inches; for about Twelve at Noon it was ſenſibly lower, and continued falling all the reſt of the Day.

When the lower End of the Tube was firſt immers'd in the Ciſtern, the Quick-ſilver for ſome Time adher'd to the Crown of the Glaſs, but upon ſhaking, it fell to the Height above-mention'd.

VI. *A Caution to be uſed in examining the Specifick Gravity of Solids, by weighing them in Water.* *By* James Jurin, M. *D.* *R. S. Secr.*

AS it is oftentimes of good Uſe to know the Specifick Gravity of ſolid Bodies, a great Number of Experiments have been made upon this Subject by Members of the *Royal Society*, and other Curious Perſons; the Reſult of which has been publiſh'd in ſeveral Tables in the *Philoſophical Tranſactions*, and elſewhere. But, as it is neceſſary that Experiments of this Nature ſhould be made with great Exactneſs, if we would ſo far depend upon them, as to draw any Inferences from them in Natural Philoſophy, it may not be amiſs to mention a Caution, which is oftentimes neceſſary in the making of them, and which I have Reaſon to think has been generally very little regarded. It is this; That when a dry, porous Body is to be weigh'd in Water, in order to diſcover its Specifick Gravity, it is neceſſary, by ſome means or other, to extricate the Air out of all the ſmall Pores and Cavities within it, that the Water may have free Liberty to enter and pervade them. Unleſs this Care be taken, it muſt needs happen, that the Air, which poſſeſſes thoſe ſmall Cavities, and keeps the Water out, will render the Solid of leſs Weight in the Water, and conſequently of leſs apparent Specifick Gravity than it really is. The beſt way of avoiding this Inconvenience, is, to ſet the Veſſel of Water, in which the ſolid Body is immerged, under the Receiver of an Air-Pump, and to extract the Air out of the Body by that means; which will be more eaſily and exactly done, if the Water be firſt heated over the Fire. And where the Conveniency of an Air-Pump cannot be had, the ſame Thing may be done almoſt as well, by letting the ſolid Body continue ſome Time in boiling Water over the Fire.

But no ſolid Body muſt ever be put into hot Water, that will in any meaſure diſſolve, or give a Tincture to the Water.

One Inſtance of the Neglect of this Caution, may be ſeen in the Accounts we have of the Specifick Gravity of the Stones taken out of Human Bladders, which have been commonly found to be but about one half, and ſome of them have been no more than a fourth Part heavier than an equal Bulk of Water. From this it has been too haſtily concluded, that theſe Stones are very improperly call'd by that Name, as not at all approaching to the Specifick Gravity of even the lighteſt real Stones, that we have any Account of.

Whereas it is much more reaſonable to ſuppoſe, that thoſe Stones, which have been found to be ſo light, were ſuch as had been a conſiderable Time taken out of the Bladder, and conſequently had loſt much of their Weight by the Evaporation of the Urine, with which they had at firſt been ſaturated, and that they had afterwards been tried without the Caution above-mention'd. I would therefore beg Leave to recommend it to thoſe, who ſhall examine the Specifick Gravity of the Human *Calculus*, that they will either try the Experiment upon Stones freſh taken out of the Bladder, or elſe that they will be pleas'd to uſe the aboveſaid Method, to extricate the Air out of their Cavities. If they do this, I am confident they will meet with ſome *Calculi* (as I have done) exceeding the Weight of ſome Sorts of burnt Earthen Ware and Alabaſter, and approaching very near to that of Brick, and the ſofter Sort of paving Stone. But it is not to be expected, that they ſhould entirely equal the Specifick Gravity of Stone, found in the Earth ; becauſe the Mixture of ſome Portion of the Animal Oil and Volatile Salt, with the ſtony Subſtance of the Human *Calculi*, muſt needs leſſen the Specifick Gravity of the whole Concrete.

I ſhall mention one other Obſervation, relating to this Subject ; which, however trivial it may ſeem, yet to me was very ſurprizing, when I accidentally diſcover'd it. It is, That the Subſtance of all Wood (as Oak, Fir, *&c.*) is ſpecifically heavier than Water. To prevent being miſunderſtood, I muſt obſerve, that in Wood, and other Vegetables, there are two Sorts of Veſſels ; one of which convey the Sap, and the other contain only Air, for which Reaſon they are call'd Air-Veſſels. When Wood floats, or ſwims in Water, this Effect is not owing to the Lightneſs of the Subſtance of the Wood, but only to its being buoy'd up by the Air contain'd in the Veſſels before-ſaid. For when the Air is extracted out of thoſe Veſſels, and inſtead thereof the Water has inſinuated it ſelf into them, the Wood will ſink to the Bottom. As is very eaſily ſhewn in ſmall Chips, or Shavings of Wood, by means of the Air-Pump, or an Infuſion in boiling, or even in cold Water for a ſufficient Time. And the ſame is found to ſucceed in the Roots, Stalks, Leaves, and Seeds of as many other Vegetables as I have yet try'd ; Cork only excepted ; in which laſt I had no Reaſon to expect it, conſidering the particular Structure of that Subſtance, as deſcrib'd by the late Learned Dr. *Hook*, in his *Micrographia*.

VII. *A Lettrr from Mr.* Edward Naiſh, *Surgeon in* York, *to* Claudius Amyand, *Eſq; Serjeant-Surgeon to his Majeſty, and F. R. S. Concerning an Oſſification of the Crural Artery.*

York, Sept. 11. 1721.

MR. *Conſett*, of *Cleveland* in *Yorkſhire*, a Gentleman of Sixty ſeven Years of Age, who all his Life before had enjoy'd a perfect good State of Health, ſent for me on account of a Mortification, which began about a Month before on one of his Toes, and by gradual Advances in that Time had reach'd half way his Leg; and this without any manifeſt Cauſe. This was the State I found him in; *viz.* a perfect Mortification, or *Sphacelus* of his Foot, and half his Leg. In ſuch a Caſe, what was to be done? The Gentleman ſaw himſelf dying daily by Piece-meal; but Heart-whole, as he expreſs'd it, and had a pretty good Pulſe. I propoſed Amputation, as the only Remedy, which (I told him) would give him ſome Chance for his Life; tho' the Odds was againſt him. This he readily conſented to; and as ſoon as I could get my Dreſſings ready, I went about the Operation; aſſiſted by Mr. *Mitford*, a Surgeon of *Northallerton*, and Mr. *Moon* of *Stockton*, who before had attended the Gentleman.

The Leg being taken off at the uſual Place, (which was four Inches above the Mortification) about two or three Ounces of Blood iſſued out from the Muſcular Part; but upon ſlackening the Turniket, in order to look for, and tye the Artery, not one Drop of Blood flow'd out; to my no ſmall Surprize. And upon feeling the Extremity of the Artery, I found it hard and callous; however, I ſecur'd it by a Ligature, as uſual, and dreſs'd the Stump. The Patient (who had born the Operation with the greateſt Reſolution) being put to Bed, I was deſirous to examine the Leg; and having diſſected the Artery, with its two conſiderable Branches as far as the *Tarſus*, I found them for the moſt part oſſified; that is to ſay, the Trunk, where it was amputated, was oſſified about two thirds of its Circumference. About a Quarter of an Inch lower, the whole was bony, leaving ſo ſmall an Orifice, that it would only admit of a Hog's Briſtle; not a very fine Probe, that I endeavour'd to introduce. A little lower, it was on one ſide bony, on t' other membranous; then again an entire Caſe of Bone. Here and there, for the Breadth of a Barly-Corn, there would be no Bone at all. I open'd about two Inches of the internal Branch immediately above the *Malleolus*, it appearing blacker than the reſt. After it had been waſh'd, I found in it about two or three Drops of coagulated Blood; and now 'tis expanded and dry'd, 'tis one entire *Lamina* of Bone, as thick as the Shell of a Pigeon's Egg, and of an unequal Surface. I diſſected three Ramifications of this internal Branch into the Foot; only one of them had a very ſmall Bit of Bone in it, about half an Inch from the Trunk. The other great Branch, that runs on the Ligament that ties the Fociles together, was not ſo much oſſified as that which I have deſcribed.

This Oſſification (which is the compleateſt of any I have yet heard of) was, no doubt, the Cauſe of the Mortification, and of the Death of my Patient, which follow'd four Days after the Amputation, as Mr. *Mitford* inform'd me, being oblig'd to leave him the next Morning; though I ſhould have been curious, had I been preſent, to have ſeen how high the Oſſification reach'd.

I muſt obſerve to you, That this bony Shell, or *Lamina*, was contain'd within the Tunicles, or Coats of the Artery. I don't doubt, but theſe Caſes are more common than we imagine. For when we ſee Mortifications ſeize the Extremities of Aged People, which we commonly attribute to a Decay of Nature, or an Extinction of the Vital Warmth; this, I believe, is often the Cauſe. And I am the more inclin'd to think ſo, from two or three parallel Caſes I have been concern'd in, ſince I came into *Yorkſhire.*

I am, *&c.*

Edward Naiſh.

VIII. *An Account of a Rainbow ſeen on the Ground. Communicated in a Letter from the Reverend* Benj. Langwith, *D. D. Rector of* Petworth, *to Dr.* Jurin, *Secr. R. S.*

Petworth, Dec. 17. 1721.

ON the 7th of *September* laſt, about Nine in the Morning, I was riding with ſome Friends over *Port-Mead* near *Oxford*. The Morning had been Miſty, and the Graſs was very wet with the Dew. We had not been long out, before the Air clear'd up, and the Sun began to ſhine very bright. We ſoon after had the Satisfaction of ſeeing a Rainbow upon the Ground, whoſe Colours were very near as lively as thoſe of the common *Iris*: This was extended upon the Ground for ſome Hundreds of Yards, and the Colours were ſo ſtrong, that it might have been ſeen much farther, had it not been terminated by the Bank, and Hedge of the Field. It is hardly worth while to obſerve, that it continually chang'd its Place as we mov'd along, ſince this is no more than happens in other Rainbows. The more remarkable Particulars were theſe:

1ſt. That the Figure of it was not round, but oblong; being as I conceive, a Portion of an *Hyperbola*.

2. That the Convex Part of it was turn'd towards the Eye, and the *Vertex* was at a ſmall Diſtance before us.

3. That the Colours took up leſs Space, and were much more lively in thoſe Parts of the *Iris* that were near us, than in thoſe at a Diſtance.

Theſe *Phænomena* may eaſily be accounted for, by comparing this *Iris* D C E, Fig. 2. with the common *Iris* k i E e form'd by Drops falling in the Air at a ſmall Diſtance from the Eye of the Spectator, H, and touching the Ground with the lower Part of its Arch in E, the vertical Point of the *Iris* D C E. Produce the Cone H k i E e: Its Interſection with the Plane of the Horizon will give the Figure of the *Iris* D C E. Hence it follows,

1ſt. That as the Angle e H G happens to be greater, equal to, or leſs than 90 deg. the Figure will be an *Hyperbola*, *Parabola*, or *Ellipſis*.

2. That as the Sun was about 30 deg. high, when we view'd the *Phænomena*, the *Iris* was an *Hyperbola*.

3. That the Arches of the ſame *Iris*, conſiſting of Colours of different Refrangibility, may alſo in ſome Caſes be different Sections of the Cone.

4. That ſince the Angle e H F is always given; from the Height of the Point of View H G, and the Sun's Altitude S L A, the Dimenſions of theſe *Iris*'s are eaſily determin'd.

I am, &c.

Ben. Langwith.

IX. *Epi-*

IX. *Epiſtola Domini* Antonij v. Leeuwenhoek, *R. S. S. de Oſculis, ſive Spiraculis Foliorum Buxi, item de lanugine Malorum Perſicorum & Cydoniorum.*

Delphis, 15. Januarij 1721.

NUPER Cogitationem meam converti ad Frondes Arborum noſtratium, tacituſque meditabar num & illæ forſitan Oſculis ſint inſtructæ; cum igitur in Area mea duas haberem Arbuſculas, ſive Plantas illius Buxi, quæ Vulgo *Palma Ceres* appellatur, folium ex iis unum decerpſi, & in partes divulſum ope Microſcopij contemplatus ſum. Tum vero partes illas, per quas Tranſpiratio vel Exhalatio ſit, clariſſime viſu diſtinxi. Adhæc complures percepi exiguiſſimos Hiatus, qui lucem tranſmittebant: Quos tamen commodius majorique numero percepi, cum partes prædicti folij aliquanto eſſent ſicciores.

Cum præterea ſupra porticum Domus meæ, quæ Porticus plum bocontecta eſt, alia ſtaret Arbuſcula Buxi; quædam illius Foliola, partim adhuc virentia, partim exſiccata, mihi adferenda curavi, ut illorum Texturam, quantum poſſibile eſſet, inveſtigarem: Quod eo Succeſſu feci; ut iſtiuſmodi Oſcula, ſive Spiracula, in foliis iſtis clarius Viſu perceperim, quam in ullis unquam Fructibus ante percepiſſem. Ut autem Multitudinem Oſculorum, quæ in tali Foliolo percepi, velut Oculis expoſitam haberem; Folium Buxeum Lineali impoſui cupero, quod in varias partes diſtinctum erat: Comperique Longitudinem Folij parem eſſe octonis partibus Pollicis, in decem partes diſtributi; Folij vero Latitudinem cum medietate pollici, ſive quinque decimis partibus exæquari.

Jam vero ponamus tali Foliolo Figuram eſſe Ovatam; adhæc Latitudinem ejus atque Longitudinem conjungamus: Tum exſurget numerus 13, cujus dimidium ſit 6 ½. Dein ponamus idem Foliolum, poſt illam Latitudinis atque Longitudinis conjunctionem, inſtar Circuli eſſe rotundum; illiuſque Diametrum 6 ½ decimis Pollicis partibus ex æquo reſpondere.

Exinde juxta Foliolum ante dictum, locavi Pilum Porcinum; quem adhibito Microſcopio contemplatus, judicavi duodecim Buxi Oſcula, ſi ſibi contigua jacerent, cum Diametro Pili Porcini Longitudine exæquari; Sexaginta vero Pilos Porcinos judicabam Magnitudinis eſſe pollicaris. Sequitur decimam quamque Pollicis partem ſex Diametris Pilorum Porcinorum Longitudine parem eſſe; dimidiatam vero Diametron Folioli Buxei cum 19 ½ Diametris Pilorum Porcinorum exæquari. Quæ 19 ½ Diametri, ſi duodecies, id eſt juxta numerum Oſculorum, multiplicentur; efficitur numerus 234, quam Longitudinem dimidiatus Folioli Buxei circulus ex antedictis habet.

Ut autem quid tali Circulo contineatur ſupputemus, primo cum Geometris ſtatuendum eſt; quæ proportio eſt numeri 14 ad numerum 11, eandem Proportionem eſſe inter numerum quadratum Diametri cujuſvis Circuli, & ea quæ Circulo ipſo continentur. Sequitur unam Folij Buxei Superficiem Oſculis 172090 præditam eſſe. Cum autem altera Superficies haud paucioribus inſtructa ſit; tandem exſurget numerus

Oſculorum 344180, quorum Ope Perſpiratio & Exhalatio fiat.

Cum Lanuginem illam, quæ Mala Perſica, vulgo Montana conveſtit, nupera Æſtate ſine Microſcopio conſiderarem; Fruſtula quædam Mali Perſici, ex cortice exciſa, ante Microſcopium collocavi. Tum vero judicabam Floccos illos Laneos Multitudine pares eſſe Oſculis per cutem Mali Perſici diffuſis. Et quemadmodum Oſcula Fructuum velut inter duo labiola pateſcere, nec plane rotunda ſed aliquantulum oblonga eſſe, jam ante monui; ſic Flocci Lanei, ex Oſculis dictis effloreſcentes, non omnino rotundi ſunt, ſed nonnihil plani. Sed & complures in medio Ruga quadam notatos eſſe videbam.

Ut autem Multitudinem Oſculorum halitus exſpirantium palam proponam; unàque ingentem numerum exhalantium humorum, qui Aerem ingreſſi in particulas oblongas, ſed alias aliis longiores ſpiſſeſcunt, Oculis ſubjiciam; exiguam Mali Perſici portionem delineari & in Icone 3 per A B C D E F G indicari curavi. Ubi quidem A B F G perpuſillum eſt Fruſtum Mali et Corticis Perſici, per B C D E F lanugo Malo adhæreſcens deſignatur.

Magnitudinem dicti Fruſtuli, per Iconem deſignati, ſic æſtimare poteris: Tres Vultus mei Pilos Microſcopio applicatos habebam, quos Pictori poſt Iconis modo dictæ Delineationem intuendos exhibui. Cum deinde ejuſdem Iconis Longitudinem, a G ad A in 16 Partes diſtribuiſſem; rogavi quot latorum pilorum Diametros in illa Iconis Longitudine contineri judicaret; qui, *ne octo quidem* reſpondit. Quod ſi verum eſſe ſtatuamus, quam incredibili Floccorum Laneorum Multitudine neceſſe eſt Malum Perſicum circumveſtiri!

Cum poſtea Mala ſive Pyra Cydonia permaturuiſſent, quaſdam etiam Mali Cydonij particulas ad Microſcopium applicavi; & Lanuginem, quæ ex Malo Cydonio exhalatur, neque Lanugini Mali Perſici Copiâ cedit, delineandam curavi; quæ omnia in Icone 4 deſignata vides per H I K L M N O. Ubi H I N O perparva eſt portio Mali Corticiſque Cydonij, per I K L M N Lanugo ex Malis Cydoniis exſudans indicatur. Quæ Lanugo, licet in Cydoniis longior quam in Perſicis, non tamen in illis erigitur, ſicut in iſtis; ſed criſpando ſibi invicem implectitur.

X. *Remarks on ſome Attempts made towards a perpetual Motion, by the Reverend Dr.* Deſaguliers. *F. R. S.*

THE Wheel at *Heſſe-Caſſel*, made by Monſieur *Orfireus* and by him called a perpetual Motion, has of late been ſo much talk'd of, on Account of its wonderful *Phænomena*, that a great many People have believed it to be actually a ſelf-moving Engine; and accordingly have attempted to imitate it as ſuch. Now as a great deal of Time and Money is ſpent in thoſe Endeavours, I was willing (for the Sake of thoſe that try Experiments with that View) to ſhew that the Principle, which moſt of them go upon is falſe, and can by no Means produce a perpetual Motion.

They take it for granted, that if a Weight deſcending in a Wheel, at a determinate Diſtance from the Centre, does in its Aſcent approach nearer to it; ſuch

a Weight in its Deſcent will always preponderate, and cauſe a Weignt equal to it to riſe, provided it comes nearer the Centre in its Riſe; and accordingly as it ſelf riſes, will be overbalanced by another Weight equal to it; and therefore they endeavour by various Contrivances to produce that Effect, as if the Conſequence of it would be a perpetual Motion.

But I ſhall ſhew, that they miſtake one particular Caſe of a general *Theorem*, or rather a *Corollary* of it, for the *Theorem* it ſelf. The *Theorem* is as follows:

Theor. If one Weight in its Deſcent, does by Means of any Contrivance, cauſe another Weight to aſcend with a leſs *Momentum* or Quantity of Motion than it ſelf, it will preponderate and raiſe the other Weight.

Cor. 1. Therefore if the Weights be equal, the deſcending Weight muſt have more Velocity than the aſcending Weight, becauſe the *Momentum* is made up of the Weight multiplied into the Quantity of Matter.

Cor. 2. Therefore if a Leaver or Balance, have equal Weights faſten'd or hanging at its Ends, and the *Brachia* be ever ſo little unequal, that Weight will preponderate, which is fartheſt from the Centre.

SCHOLIUM.

This Second *Corollary* cauſes the Miſtake; becauſe thoſe, who think the Velocity of the Weight is the Line it deſcribes, expect that that Weight ſhall be overpois'd, which deſcribes the ſhorteſt Line, and therefore contrive *Machines*, to cauſe the aſcending Weight to deſcribe a ſhorter Line than the deſcending Weight.

As for Example, in the Circle A D B a (*Fig.* 5.) the Weights A and B being ſuppoſed equal, they imagine, that if (by any Contrivance whatever) whilſt the Weight A deſcribes the *Arc* A a, the Weight B is carried in any *Arc*, as B b, ſo as to come nearer the Centre in its riſing, than if it went up the *Arc* B D; the ſaid Weight ſhall be overpois'd, and conſequently, by a Number of ſuch Weights, a perpetual Motion will be produced.

This is attempted by ſeveral Contrivances, which all depend upon this falſe Principle; but I ſhall only mention one, which is repreſented by *Fig.* 6. where a Wheel having two parallel Circumferences, has the Space between them divided into Cells, which being carv'd, will, (when the Wheel goes round) cauſe Weights plac'd looſe in the ſaid *Cells*, to deſcend on the Side A A A, at the outer Circumference of the Wheel; and on the Side D to aſcend in the Line B b b b, which comes nearer the Centre, aud touches the inner Circumference of the Wheel. In a *Machine* of this Kind, the Weights will indeed move in ſuch a Manner, if the Wheel be turn'd round, but will never be the Canſe of the Wheels going round. Such a *Machine* is mentioned by the Marquis of *Worceſter*, in his Century of Inventions in the following Words, N°. 56.

" To provide and make that all the Weights of the
" deſcending Side of a Wheel, ſhall be perpetually
" farther from the Centre, than thoſe of the mount-
" ing Side, and yet equal in Number, and heſt to
" the one Side as the other. A moſt incredible thing,
" if not ſeen; but tried before the late King (of bleſ-
" ſed Memory) in the *Tower* by my Directions, two
" extraordinary Ambaſſadors accompanying his Ma-
" jeſty,

“ jeſty, and the Duke of *Richmond*, and Duke of *Hamilton*, with moſt of the Court attending him. The
“ Wheel was fourteen Foot over, and had fourty Weights
“ of fifty Pounds a Piece. Sir *William Balfore*, then
“ Lieutenant of the *Tower*, can juſtify it, with
“ ſeveral others. They all ſaw, that no ſooner theſe
“ great Weights paſſed the Diameter Line of the lower
“ Side, but they hung a Foot farther from the Centre;
“ nor no ſooner paſſed the Diameter Line of the upper
“ Side, but they hung a Foot nearer Be pleaſed to
“ judge the Conſequence.

Now the Conſequence of this, and ſuch like *Machines*, is nothing leſs, than a perpetual Motion; and the Fallacy is this. The Velocity of any Weight is not the Line, which it deſcribes in General, but the Height that it riſes up to, or falls from, with reſpect to its Diſtance from the Centre of the Earth. So that when the Weight (*Fig.* 5.) deſcribes the *Arc* A a, its Velocity is the Line A C, which ſhews the perpendicular Deſcent (or meaſures how much it is come nearer to the Centre of the Earth) and likewiſe the Line B C denotes the Velocity of the Weight B, or the Height that it riſes to, when it aſcends in any of the *Arcs* B b, inſtead of the *Arc* B D: So that in this Caſe, whether the Weight B in its Aſcent be brought nearer the Centre or not, it loſes no Velocity, which it ought to do, in order to be rais'd up by the Weight A. Nay, the Weight in riſing nearer the Centre of a Wheel, may not only not loſe of its Velocity, but be made to gain Velocity, in Proportion to the Velocity of its counterpoiſing Weights, that deſcend in the Circumference of the oppoſite Side of the Wheel; for if we conſider two *Radij* of the Wheel, one of which is

Horizontal, and the other (faſten'd to and moving with it) inclin'd under the Horizon in an Angle of 60 degr. (*Fig.* 7.) and by the Deſcent of the End B of the *Radius* B C, the *Radius* C D by its Motion cauſes the Weight at D, to riſe up the Line p P, which is in a Plane that ſtops the ſaid Weight from riſing in the Curve D A, that Weight will gain Velocity, and in the Beginning of its Riſe, it will have twice the Velocity of the Weight at B; and conſequently, inſtead of being raiſ'd, will overpoiſe, if it be equal to the laſt mentioned Weight. And this Velocity will be ſo much the greater, in Proportion as the Angle A C D is greater, or as the Plane P p (along which the Weight D muſt riſe) is nearer to the Centre. Indeed if the Weight at B, Fig. 5. could by any Means be lifted up to *β*, and move in the *Arc β* b, the End would be anſwer'd ; becauſe then the Velocity would be diminiſhed, and become *β* C.

Experiment (*Fig.* 7.)

Take the Leaver B C D, whoſe *Brachia* are equal in Length, bent in an Angle of 120 degr. at C, and moveable about that Point as its Centre: In this Caſe, a Weight of two Pounds hanging at the End B of the horizontal Part of the Leaver, will keep in *Æquilibrio* a Weight of Four Pounds hanging at the End D. But if a Weight of one Pound be laid upon the End D of the Leaver, ſo that in the Motion of D along the *Arc* p A, this Weight is made to riſe up againſt the Plane P p (which divides in half the Line A C equal to C B) the ſaid Weight will keep in *Æquilibrio* two Pounds at B, as having twice the Velocity of it, when the Leaver begins to move. This will be evident, if you let the Weight 4 hang at D, whilſt the Weight 1 lies above it: For if then you move the Leaver, the Weight 1 will riſe four times as faſt as the Weight 4.

XI. *A Method for rowing Men of War in a Calm. Communicated by Monſieur* Du Quet.

TO perfect the Art of Navigation, Two Things ſeem principally wanting; *viz.* An eaſy Method for finding the Longitude at Sea; and a Way to give a Veſſel its Courſe, when there's no Wind ſtirring.

I flatter my ſelf to have found the laſt; and hope to make it appear, by Reaſon and Experiment, That a Man of War may make a League an Hour in a Calm, by Means of revolving Oars, which are eaſily apply'd to the Sides of the Ship, without occaſioning any Incumbrance: As I ſhall make appear by the following Account, after having deliver'd my Notion of the Motion of Bodies in Fluids.

A Body ſwims upon Water, when it weighs leſs than the Volume of Water, whoſe Place it takes up; and it ſinks more or leſs in the Water, only in proportion as its Volume is more or leſs increas'd.

A Body lying in ſtill Water, is as it were *in Æquilibrio*; the leaſt Effort gives it Motion, and makes it loſe that *Æquilibrium.* If the Effort be continued, tho' ever ſo little, the Motion it communicates will be very ry ſenſible. How great ſoever the Weight of the Body be, when once it is in Motion, it will always continue ſo, if nothing hinders.

Upon theſe Principles, I conſider the Motion a Veſſel receives by means of Oars, and the Application of Hands that ſet it a-going. The *Impetus* of the Hand, apply'd at one End of the Oar, and the Reſiſtance the Water makes againſt the other End, are both impreſs'd upon the Point, where the Oar reſts upon the Veſſel. This Point is like the *Fulcrum* of the common Leaver, which always bears the Sum of the Weights at both Ends, beſides the Weight of the Leaver it ſelf: So that the greater the Effort is at one of the Oars, and the Reſiſtance at the other, ſo much the greater is the Impreſſion, which the Point or *Fulcrum* receives, in order to its being put in Motion. A Galley, with two Oars only, would go as faſt as it does with the uſual Number; provided the ſame Number of Hands were apply'd with equal Vigour to the two Oars, and the Oars were ſtrong and broad enough to make the neceſſary Reſiſtance: Becauſe then the *Fulcrum* of the two Oars would receive as much Impreſſion, as all the *Fulcra* of the common Oars taken together.

This Conſideration put me at firſt upon contriving a Way, how to apply a greater Number of Hands to the common inclined Oars; but, after ſeveral Tryals, I threw them aſide, and made uſe of perpendicular Oars; becauſe the firſt do only skim the Water, and (when the Sea is rough, and the Waves run high) they don't take Water oftentimes, and ſo become uſeleſs. For, in this Caſe, the Rowers are tript up, for want of meeting a Reſiſtance.

This Inconvenience is avoided by the revolving Oars; becauſe they take the Water perpendicularly, and enter far enough not to miſs it: And if the Water ſhould happen to evade the Stroke, the Rowers would not be ſo incommoded; becauſe they would be ſupported at every Vibration, which is only of three Foot. Beſides, in the Uſe of inclined Oars, more than half the Time is loſt, in raiſing and recovering the Oar, before they give the Stroke; which makes the Veſſel move by fits and jerks, ſo that the People aboard feel (as it were) every Stroke of the Oars when they play; whereas, the revolving Oars always move equally, and ſucceed one another without Loſs of Time; which makes the Veſſel move uniformly, without affecting thoſe who are aboard.

It is to be obſerv'd too, that a Gally built on purpoſe for the Uſe of inclined Oars, would not be ſo proper as another Veſſel for perpendicular Oars; becauſe the Gally has a conſiderable Length and but little Height above the Water.

Having propoſed this Invention to the Court of *France*, I was ſent to *Havre de Grace*, to make a Tryal, which had the Approbation of the Intendant. He made his Report, That the Officers at firſt objected to the Invention; but as for his own part, the more particularly he conſider'd it, the more he was convinc'd of its Uſefulneſs. I was afterwards ſent to *Marſeilles*, where I made ſeveral Tryals on board a Gally; the Swiftneſs of which was compar'd with that of another Gally, equipp'd as uſual. M. *de Chazelles*, a Member of the *Royal Academy of Sciences*, and Engineer of the King's Gallies, had Orders to make his

Obſer-

Obſervations, and ſend 'em to Court: A Copy of which he gave me, ſign'd with his own Hand, and is as follows.

The Report of Monſieur *de Chazelles.*

An Experiment of the Swiftneſs of a Gally, with perpendicular revolving Oars, invented by Monſieur Du Quet; *compar'd with that of a common Gally. Made at* Marſeilles, &c.

At 10 h. 3 min. in the Morning, the *Superbe* Gally quitted her Station over-againſt the *Auguſtins*, in order to fall down to the *Chain.*

At 10 h. 11 m. ſhe came to the *Chain.*

At 10 h. 6 m. the *Machine* Gally quitted her Station, at the innermoſt Part of the Port. She had three *Machines* on each ſide.

10. 13. She came to the *Chain.*

10. 19. The two Gallies abreaſt. Both row with their whole Crews.

10. 25. The *Superbe* paſſes; and then rows only with the hinder part of her Crew.

10. 27. The *Machine* Gally paſſes.

10. 28. Both row with their whole Crews.

10. 30. The *Superbe* Gally paſſes; and then rows only with the fore part of her Crew.

10. 32. The *Machine* Gally paſſes; upon which the *Superbe* Gally claps on more Oars, till ſuch Time as ſhe has acquir'd the ſame Velocity with the *Machine* Gally: And it appear'd, that with ſeven or eight Oars leſs than her Complement

on each ſide, ſhe kept up with the *Machine* Gally; making about 200 Rowers, which was the Number of the *Machine* Gally's Crew. There was a little Wind a-head, which retarded the *Superbe* ſomething more than it did the *Machine* Gally; becauſe the *Superbe* had her Maſts and Yard-Arms ſtanding, and the other not.

10. 43. Came to the Moorings of the *Iſles*. The Sign given for turning.

10. 47. The *Superbe* was come about. It appear'd, that the *Machine* Gally was conſiderably quicker in turning than the *Superbe*.

10. 30. They came again into Port.

By this it appears, that the *Machine* Gally has a conſiderable Advantage over the common one, in quitting her Station, and acquiring her firſt Motion: For, in ſeven Minutes, ſhe ran the whole Length of the Port; having quitted her Station by means of her Oars, without towing her ſelf by her Moorings; which is what another Gally would not have effected, but very ſlowly. And the *Superbe* Gally, after ſhe had mov'd from her Station, was eight Minutes in going a leſs Diſtance than the Length of the Port.

But if we conſider the Experiment made without the Harbour, it ſeems to prove the common Gally to have the Advantage over the *Machine* Gally, tho' the Number of Hands be equal. For, with eight Oars leſs than her Complement on each ſide, ſhe kept up with the *Machine* Gally, notwithſtanding the greater Reſiſtance of the Wind againſt her Maſts. However, if we conſider, that the Crew of the *Superbe* was a great deal better than that of the *Machine* Gally;

that the *Superbe* is acknowledg'd to be one of the beſt Sailors the King has; whereas that which had the *Machines*, is an old decay'd Gally, and reckon'd a very bad Sailor; beſides that the Crew of the *Superbe* are much better acquainted with the common Oar, than the others are with the new way of rowing; and that in the common Gally there's no Improvement to be made, either with reſpect to the Proportion of the Oars, their Length, the Breadth of the Pallets, the Height of the Point of Reſt, *&c.* or with reſpect to the Conſtruction of the Veſſel; whereas in the *Machine*, there are ſeveral Things to be improv'd and alter'd in the Oars, the Hand-ſpikes, and in diſpoſing the Men to the beſt Advantage. Theſe Things, I ſay, conſider'd, it ſeems reaſonable to believe, that a Veſſel with the *Machines* might go faſter than one with the common Oars; becauſe the Loſs of Time is avoided, which happens in the ordinary way of rowing.

This Experiment, however defective it be, for the Reaſons above, will prove, That the Velocity is greater in this way of rowing than in the other, when the Circumſtances on both ſides are equal. For, by my Journal, I find, that the *Patronne*, in Company with fourteen other Gallies, left the Port of *Marſeilles* at 50 min. paſt three; and rowing all in a Calm, came to the *Iſles* at 4 h. 23 min.; which made 33 m. in going from the *Chain* to the *Iſles*. But the *Machine* Gally made the ſame way, with 200 Men, in 30 min. having left the *Chain* at 10 h. 13 m. and arriv'd at the *Iſles* at 10 h. 43 min. altho' there was ſome Wind a-head.

Sign'd *CHAZELLES.*

This Experiment ſhews, that the *Impetus* does not depend upon the Number of Oars, but the Number of Men.

A Veſſel charg'd with revolving Oars, will go as faſt in a Calm with 100 Men, as it would do if towed by a Gally of 200 Men; becauſe there will be one Gally leſs to draw along.

Another Memoire of Monſieur Chazelles, *concerning the Uſefulneſs of perpendicular revolving Oars, invented by Monſieur* Du Quet.

The Experiment made of the New *Machine*, altho' defective by reaſon of the Difference there was with reſpect both to the Crew and the Veſſels, does yet leave room to expect a conſiderable Advantage from this Invention, in giving the Ship way: For tho' the common Gally ſhould keep up with the *Machine* Gally at their firſt ſetting out, with equal Number of Hands; 'tis evident, the *Machine* Gally will get the better at long Run, when the others Crew are ſo fatigu'd, as to be oblig'd to row by turns. For here the Men will hold out a longer Time, their Action not being ſo great, nor ſo violent. Beſides, having only 200 Men employ'd, and being equally mann'd with the other Gally, freſh Hands may be ſupply'd, and ſo they will continue to go at the ſame Rate: For in caſe of need, the Marines may be employ'd in this Service; which they will perform with as little Reluctance, or Trouble, as they work at the Capſtane.

The Reaſon of this Increaſe of Velocity appears plain, if we conſider the Difference between the common

mon way of rowing, and that by perpendicular Oars: The laſt is done by an uninterrupted Application of Force, in the ſame Direction; the other acts by Jerks. And, of the three Parts of Action that are employ'd, in order to give the Strokes; one in raiſing the Oar out of the Water, the ſecond in advancing the Hands forwards, and the third in preſſing againſt the Water; only the laſt turns to Account: And that ſtill loſes ſomething of its Efficacy; for the Crew, by their falling back all together, make the Veſſel plunge, and render its Motion oblique, which contributes very much to its Decay.

Theſe are not the only Defects of the common Oars; for, in order to augment their Force, the Number is to be increas'd, and conſequently, the Veſſel muſt have a greater Length; by which means, it is render'd weaker and leſs able to reſiſt the force of the Sea. Beſides, the Veſſel muſt be low-built, and uncover'd, (and ſo more expos'd to the beating in of the Waves) by reaſon they are obliged to proportion the Length of the Oar to the Strength and Size of the Men. And tho' the Crew ſhould be under ſome Covert, as they are in a Galeaſs; an Opening muſt be left for the Oars to play, by which the Waves may beat in.

Both theſe Inconveniencies are avoided, by the perpendicular Oars; becauſe the Addition of Force may be obtain'd, by only applying more Hands to the *Machine*; ſo that with two or three *Machines* on a ſide, there will be more or leſs Force, in proportion to the Number of Men employ'd, and the Length of the Veſſel may be leſſen'd at Diſcretion. And to guard againſt the Sea, another Deck may be made, ſhut cloſe on all ſides, even where the *Axis* of the *Machine* paſſes through.

The chief Objections againſt this Invention, ſeem to me ſufficiently obviated by Mr. *Du Quet's Memoir:* But tho' the whole of what is objected ſhould indeed prove, That a Veſſel made for ſailing, as the common Gally, would be ſo incumber'd with the *Machines*, as to make the Uſe of Sails impracticable; yet if it ſtill holds true, that ſhe will move faſter; as appears, both by Reaſon and Fact; it muſt be allowed, that a Veſſel might be ſo commodiouſly conſtructed, to carry theſe *Machines*, as to go as faſt as a Gally in a Calm, and better endure the Weather when under Sail.

Such a Veſſel would have ſeveral Advantages above a Gally, both in Sailing, and in Fight; not to mention the Conveniencies of lodging the Crew. She may put off to Sea any where, and thereby avoid the Dangers attending the Coaſt-Winds, which Gallies find to be a-head as ſoon as they have doubled certain Capes; and ſo they find themſelves between two Winds, which there would be no Danger of, farther out at Sea. With reſpect to Fight, ſhe may mount Cannon fore and aft, and on each ſide; and even Mortar-pieces. In Time of Battel, ſhe would be of wonderful Uſe; for ſhe would take and maintain her Poſt without Aſſiſtance, either at the Head, or the Rear of the Enemy's Line, and there make uſe of her Bombs: Beſides the Advantages of towing off other Veſſels, from their Danger in a Calm, and of boarding, or making off from the Enemy. And this holds in Ships of any Rate; provided the Length of the Oars, the

Breadth of the Pallets, and the Strength of the Handſpikes be proportionable. And the moving Force will always be in proportion to the Strength and Number of the Men employ'd, and not to the Number of *Machines*, as in the common Oars, which too are impracticable in Ships above the fourth Rate, by Reaſon of their great Length, which will be diſproportionate to the ordinary Bulk of a Man.

By this means the Crew will be free from the Fatigue of towing, and the Veſſel will move incomparably faſter than if it was towed; becauſe the Chaloups which tow, are ſubject to the Inconveniencies of the common Way of towing, by loſing two thirds of the Time; and beſides, they can't act all together: And the Veſſel that is towed, pulling them back after the Oar has made its Stroke, they have ſo much of the Space to regain by the next Stroke. Beſides, the Cable by which they tow, ſinking into the Water by its own Gravity, the Reſiſtance the Water makes to its Return, is to be overbalanced; all which Circumſtances together conſiderably diminiſh the towing Force.

Beſides, this Invention is not ſuch as is deſtructive to Mankind, and becomes uſeleſs to the Nation that firſt puts it in Practice, when generally known; on the contrary, it may be greatly advantagious to the Inventors at the Beginning, and every where ſerviceable on many Occaſions, when it is put in Practice by thoſe who uſe the Sea.

Signed *CHAZELLES.*

Mr. *de Chazelles* might have added, that the Chaloups that tow, are in cloſe Fight liable to be ſunk by the Enemy's Cannon, and are expoſed to the Waves by their having ſo little Height above Water.

It may be proper to explain the Advantages there are in boarding, or getting clear of the Enemy,by means of the new Oars.

The chief Advantage, and which includes all the reſt, is, that let a Veſſel crowd as much Sail as poſſible, the perpendicular Oars are always capable of increaſing her Swiftneſs, becauſe the Rowers have only a Motion of three Foot to make one Way, and as much the contrary Way, in order to make the Oars deſcribe four and fifty Foot Space in the Water, and that Motion of ſix Foot might be perform'd in two Seconds of Time, if the Oars met with no Reſiſtance ; conſequently the Veſſel muſt run four and fifty foot in two Seconds, that is, about ſix Leagues an Hour, before thoſe revolving Oars be unſerviceable, for then the Veſſel would go as faſt as the Oars could poſſibly move with a Diameter of eighteen Foot ; and if it was neceſſary to make them move faſter, it is only lengthning out their Diameter, and they would move ſo much the faſter, without obliging the Rowers to increaſe their own Motion.

Mr. *Arnoult* was order'd to examine the new Oars ; and he made his Report to the Court, that the Officers of the Gallies found, that they interfered with the Uſe of the Sails in a Gally, but might be of Uſe in other Veſſels and Bomb-ketches ; in Conſequence of which, I was ſent to *Toulon* to make the Experiment on Board a Bomb-ketch.

At the Time when the Experiment was made, Mr. *de Vauvre*, and the Officers of the *Marine* were at Sea, and only ſome Officers of the *Port* were preſent, who ſent a Verbal Proceſs to the Court, without acquainting me with it, or offering any Objection, although I had very much preſs'd them to it, in order to obviate the Prejudices might be conceiv'd againſt this Novelty.

At my Return to *Paris*, Mr. *de Salabery*, ſurpriz'd at my knowing nothing of that Account, gave it me to anſwer, which I did Paragraph by Paragraph: The whole was given to a general Officer then at Court to examine, and make a Report of it, the Reſult of which was, That this Invention ought to be put in Practice.

XII. *Part of a Letter from the Reverend Mr.* Rowlands, *to the Reverend Mr.* Derham, *Prebendary of* Windſor, *and F. R. S. Concerning the ſtocking of the River* Mene *with Oyſters.*

THE River of *Mene*, that divides *Angleſey* from *Carnarvonſhire*, near which I live, has at preſent the Bottom of its Channel for ſome Miles in length, all bedded with good Oyſters, in ſuch Plenty, that in the Seaſon, ſeveral Boats are daily empoly'd to dredge them up, and have done ſo theſe eight or nine Years laſt paſt to their great Profit; but what I recommend as obſervable, is, that about twenty four Years ago, we have good Aſſurance, that there were none to be found on that Bottom: but that a Gentleman about that Time, cauſed three or four hundred large Oyſters to be dropp'd into the Channel, juſt under his Land; from the Spat or Seed of which, it is moſt probable, the Flux and Reflux of Tides diſperſing it, all the Bottom at length, where ſmall Stones and a large Cultch received the Sperm, became cover'd with Oyſters. And what favours this Conjecture, that they are a Brood of Oyſters begun at that Time, is, that at the firſt finding, they appeared young and ſmall, but have ſince yearly increaſed in Bulk and Plenty, though prodigious Quantities have been taken up of them.

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F I N I S.

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PHILOSOPHICAL TRANSACTIONS.

Giving Some

ACCOUNT

OF THE

Preſent Undertakings, Studies, and *Labours*

OF THE

INGENIOUS,

IN MANY

Conſiderable Parts of the WORLD.

VOL. XXXII. For the Years 1722, 1723.

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TO

Sir *HANS SLOANE*, Bar^{t}.

PRESIDENT of the

College of *PHYSICIANS*,

And VICE-PRESIDENT of the

ROYAL SOCIETY,

This Thirty Second Volume of

Philoſophical Tranſactions

is Humbly Dedicated

BY

His Moſt Obliged, and

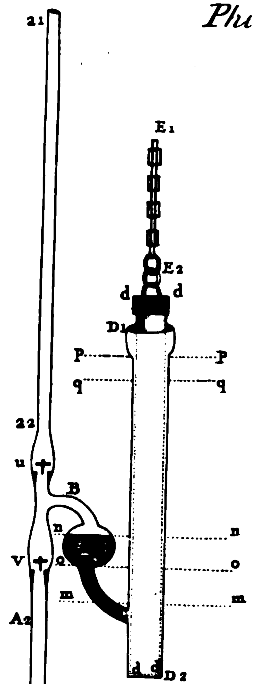
Moſt Humble Servant,

JAMES JURIN, *R. S. Secr.*

61-38

Philoſoph: Tranſact: N.o 370

Plate I.



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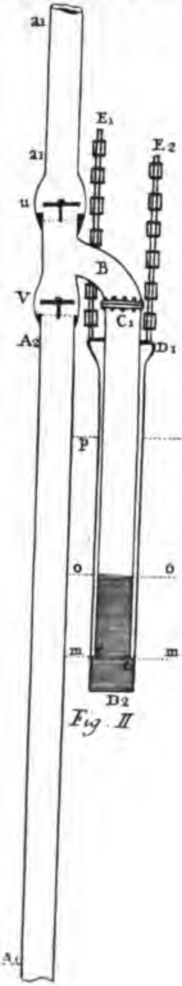

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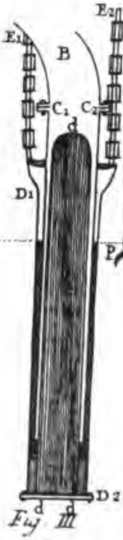

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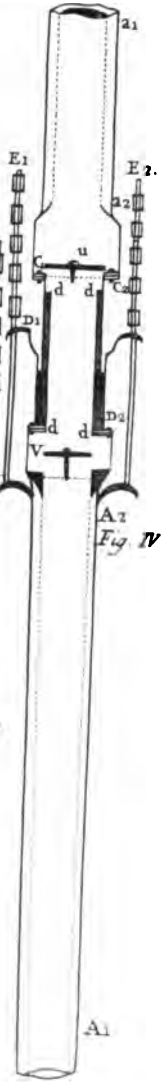

Fig. IV

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Beginning the Thirty Second Volume.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *January*, *February*, and *March*, 1722.

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I. *The Longitude of* Buenos Aires, *determin'd from an Obſervation made there by* Pere Feuillèe. By Edm. Halley LL. D. *Aſtronomer Royal*, and F. R. S.

I HAVE as Occaſion offered, made it my Buſineſs to collect ſuch Celeſtial Obſervations as might be of Uſe to determine the Longitudes of Places on the Sea coaſt of the World; in order to get as near as poſſible the Out-line, or true Figure of the Earth, without which our Geography of the Inlands muſt neceſſarily be very inſufficient. The Memoirs of the Royal Academy of *Paris*, afford a good Number of Obſervations of this Kind, and among the reſt, there is one made at *Buenos Aires* on the River of *Plate*, in the South *America*, by *Pere Feuillèe* in his Voyage to *Peru*: who, in the Memoirs for the Year 1711. is ſaid to have obſerved at that Place on the 19th of *Auguſt*, 1708. the Immerſion of the Star in the Southern Foot of *Virgo* (marked by *Bayer* with λ) behind the obſcure Limb of the Moon. Being deſirous to ſee what Longitude might be deduced from this Obſervation, I ſoon found that there was a Fault in the Day, and likewiſe in the Star; for that λ of *Virgo* was then nearly in 3 Degrees of *Scorpio*, and the Moon would not be there till the next Day, *Monday* the 20th of *Auguſt*; and the Latitude of λ being about half a Degree North, the Moon at that Longitude would be about 3 Degrees more Southerly than the Star, and conſequently far from Eclipſing it; for that at that time the deſcending Node was in the very Beginning of *Libra*. Hence I concluded it muſt be ſome other Star, that *Pere Feuillée* obſerved Eclipſed by the Moon: The Day was certainly the 20th and not the 19th of *Auguſt*, as was evident by the Place of the Moon; but as to the Star, it was neither in the *Tychonick* Catalogue, nor yet in that more copious *Britiſh* Catalogue of Mr. *Flamſteed*; but turning over that of *Hevelius*, I found a Star whoſe Situation agreed well with the Obſervation, and was undoubtedly the Star that was ſeen to immerge behind the Moon: The Place Mr. *Hevelius* gives it, allowing the Preceſſion of the Equinox, was then ♏ 1° 56′¼ with South Latitude 2° 51′¼. It remained then for me to be aſſured of the Place of this Star, and accordingly on the 21ſt and 24th of *December* laſt, I got ſuch Obſervations by help of the circumjacent Stars, that I was aſſured the Place of the Star, (which is a fair Star, of the 5th Magnitude) was at that time, ♏ 1° 58′ 40″ with South Latitude 2° 54′⅓, being above 2′ in Longitude, and 3′ in Latitude, more than *Hevelius* gives it. The Hour of this Occultation is ſet down preciſely 7h 5′ 38″ at *Buenos Aires*, the Latitude of the Place being 34° 35′ South. Whence the Altitude of the Moon there was then 42° 48′, and the Parallactick Angle 76° 38′, and the Parallax in Longitude 40′ 11″ to the *Weſt* and in

Latitude 9′ 33″ to the *North*. So the Moon's obſerved Place corrected by Parallax was ♏ 2° 28′ 4″ with South Latitude 2° 52′$\frac{1}{2}$. To this Place, by the *Calculus* of thoſe Numbers I have fitted to our Preſident's Theory of the Moon (but which would be improper and too long to be here recited) the Moon will be found to have arrived *Auguſt* the $\frac{9}{20}$ at 10^h 57′ 36″ apparent Time at *London*. But at *Buenos Aires* it was then computed but 7^h 5′ 38″, whence the difference of Longitude reſulting from this Obſervation is 3^h 52′ or 58 Degrees, by how much *Buenos Aires* is more Weſterly than *London*. I have twice repeated the Calculation to be ſure to avoid error, and by comparing my Chart of the Variation with the Longitude thus found, it appears that in this Caſe a Ship at Sea uſing thoſe Tables and that Chart, would by an Obſervation of this Occultation have fallen with greater exactneſs on the Coaſt of *America*, than by any Reckoning can be pretended to be done.

II. *A*

II. *A Deſcription of an Engine to raiſe Water by the Help of Quickſilver, invented by the late Mr.* Joſhua Haskins, *and improv'd by* J. T. Deſaguliers, LL. D. R. S S.

MR. *Haskins* finding that all Hydraulic Engines. working with Pumps, loſe a great deal of Water, (always giving leſs than the Number of Strokes ought to give according to the Contents of the Barrels:) and that when the Piſtons are new leather'd to prevent that Loſs, the Friction is much increas'd. and the Engines are ſubject to Jerks, which in great Works do often diſorder an Engine for a great while, by breaking ſome of the Parts; contriv'd a new way of raiſing Water without any Friction of Solids; making uſe of Quickſilver inſtead of Leather, to keep the Air or Water from ſlipping by the ſides of the Piſtons in the Barrels where they work; hoping thereby to prevent all the aboveſaid Inconveniences, and alſo to have Water Engines leſs liable to be out of Order than any yet made.

The firſt Experiment he made with an Engine that he ſet up at my Houſe about two Years ago, which I repeated before the Royal Society in a Model; and tho', by the ill Contrivance of the Parts, it did not raiſe near the Quantity of Water that the Invention is capable of; yet I ſhall deſcribe the Machine here, becauſe it will ſerve for the better Underſtanding of our preſent Engine.

Fig. I.

dddd repreſents a *Lignum Vitæ* Plug or Piſton (which Mr. *Haskins* call'd a Plunger) about 6 Foot long made heavy enough with Lead at top to ſink into *Mercury*, which is beforehand pour'd into the Barrel D 1 D 2 up to *m m*. The Chain E 1 E 2, joyn'd to the Piſton and the Power that moves it, being let down till the Piſton comes to D 2, the *Mercury* riſes to the ſame Height in the Barrel, and in the Receiver R, (which it fills) namely to *n n*, as appears in the Figure. Then drawing up the Piſton till its Bottom is come to *m m*, the *Mercury* coming out of the Receiver down to *o o* makes a *Vacuum*, and the Weight of the Atmoſphere cauſes the Water to riſe up thro' the Sucking Pipe A 1 A 2, and Valve V into the Receiver where the *Mercury* was before. Upon letting down the Piſton again, the *Mercury* riſes into the Receiver, and drives up the Water thro' the Elbow B, the forcing Valve *u*, and ſo up the forcing Pipe *a* 2 *a* 1: But when once the forcing Pipe (which here was 46 Foot high) is full, before any *Mercury* can enter into the Receiver, and force any Water out at the top of the Pipe *a* 1, the *Mercury* between the Piſton and Barrel muſt riſe up to *q q* near 3 ⅓ Feet above the Bottom of the Receiver, and as it continues to riſe up to *p p*, the Water is thrown out with a Velocity proportionable to the Height that the *Mercury* is raiſ'd above the 14th part of the Height of the Water. Now tho' the Friction of Solids is here avoided, it is plain that the *Mercury* muſt move from *m m* to *q q* without raiſing any Water, and that it can only force in going from *q q* to *p p*, and only ſuck in falling from *o o* to *m m*: And unleſs the Piſton is ſtopp'd a little while when at loweſt, the Water won't have time to run out: So likewiſe the Piſton muſt be ſtopp'd when at higheſt, that the Receiver may have time to fill.

Mr. *Haskins* likewiſe propoſed another Way, repreſented in *Fig.* 2; where the ſame Letters repreſent the ſame Parts, only here the Barrel is moveable by the two Chains E 1 E 2, and inſtead of a ſolid Piſton, the hollow Cylinder C 1 *cc* is fix'd, and the *Mercury* moving up and down in the lower Part of it, ſucks and forces the Water thro' the Elbow. The Figure repreſents the Engine ſucking, by means of the *Mercury* hanging from *o o* to *m m*. In order to force, before any Water can be driven out, the *Mercury* in the inner Cylinder muſt deſcend from *o o* to *m m*, and riſe up to *pp* between that Cylinder and the Barrel; ſo that here alſo a great deal of time is loſt; beſides the great Quantity of *Mercury* us'd, which is very expenſive; becauſe as much *Mercury* is mov'd every Stroke as the Water rais'd.

Theſe Difficulties very much puzzled Mr. *Haskins*, and quite diſcourag'd ſome other Perſons that had got the Secret of the Invention, and were ſetting up againſt him. But when I had conſider'd the Matter a little, tho' I had not time to contrive a Machine for it, I told him, That a little *Mercury* might be made to raiſe a great Quantity of Water, and there ſhould not be ſuch a loſs of time as in his Engines; but that I would have him find it out before I aſſiſted him farther. In a little time he found out the Contrivance repreſented in *Fig.* 4; and afterwards that of *Fig.* 3; which laſt was what I had thought of: And both theſe were alſo found out by the late Mr. *William Ureem*, who was an excellent Mechanick.

FIG. III.

Here the Barrel is mov'd as in *Fig.* 2, but the Plug *ddd* taking up a great deal of Space, there is Occaſion for no more *Mercury* than what will make a concave Cylinder or Shell up to *pp* between the Barrel D 1 D 2, and the hanging Cylinder C 1 C 2 *cc*, when the Stroke is made for forcing; and a concave Cylinder between the Plug and C 1 C 2 *cc*, when the Suction is made. I gave Mr. *Haskins* the Proportions for an Engine this way, of which he made a Draught, and ſhew'd it to the Right Honourable the Lord Chancellour about ſix Months ago. This I mention here, that no body may endeavour to get a Patent for this Invention, to the Prejudice of Mr. *Haskins*'s Aſſignees; who, ſince his Death, have deſir'd me to aſſiſt them in perfecting the Engine.

FIG. IV.

Here the Barrel with a third Cylinder *dddd* inſtead of the Plug of *Fig.* 3, is lifted up and down every Stroke, and the Water paſſes thro' *dddd*, the *Mercury* making a Shell ſometimes between the middle and inner Cylinder, as in the Suction; and ſometimes between the Barrel and the middle Cylinder, as in the forcing Stroke.

Mr. *Haskins* had contriv'd ſuch a Machine as is repreſented by this 4th Figure, and beſpoke the ſeveral Parts before he dy'd; and therefore when I was deſir'd by his Aſſignees to direct the ſetting up the Machine, I was oblig'd to make uſe of the Pieces already made, in order to ſave the Expence of a new Engine: And now the whole put together with ſome

Alterations, make the Engine repreſented by *Fig.* 5, as it is ſet up at my Houſe in *Weſtminſter*, and by the Force of one Man, raiſes a Hogſhead of Water in little more than a Minute and a half to the Height of 27 Feet. All the Fault of the Machine of *Fig.* 5 is, that the Pendulum Handle *Ff* is too long, and the Bottom of the middle Cylinder C ought to be juſt in the middle of the Height to which the Water is to be raiſ'd, ſuppoſing three Copper Cylinders to be as they are here: If likewiſe the Barrel D 1 D 2 work'd under the forcing Pipe, the Lift would be eaſier. Therefore I deſcribe the Machine with the ſmall Alteration repreſented in *Fig.* 6.

The ſucking and forcing Pipe and Valves are mark'd with the ſame Letters as in the other Figures; and the Chains E 1 E 2 muſt be ſuppos'd to hang from ſuch Pullies, and to be mov'd by ſuch a Pendulum as is in *Fig.* 5. The Barrel D 1 D 2 (call'd otherwiſe the outer Cylinder, and repreſented by the ſame Letters in *Fig.* 7,) has within it another Cylinder (call'd the inner Cylinder or Plug, as *dddd Fig.* 7,) between which two Cylinders a certain Quantity of *Mercury* is pour'd in, and the hanging Cylinder C coming down into the *Mercury*, a Stroke of 13 Inches may be made by the Motion of the Barrel, which, in going down ſucks by making a *Vacuum* in C, and in going up forces the Water out of the top of the forcing Pipe, performing the Office of a common Piſton; only that inſtead of Leather to make it tight to the Cylinder C, there is always a thin Shell of Quickſilver either between the middle Cylinder C and the inner one, (*dddd Fig.* 7.) as happens when the Suction is made, or between the middle and outer Cylinder, as happens in lifting up the Barrel to Force. In the Suction, the *Mercury* is higher in the inner Shell than in the outer Shell, by an Height equal to a little more than $\frac{1}{14}$ Part of the Height of the Barrel above the Water to be rais'd: And in forcing, it is higher in the outer Shell than in the inner by a little more than $\frac{1}{14}$ of the Height of the Pillar of Water to be forc'd. And therefore if the Water is not required to be rais'd above 64 Feet, the Barrel ſhould move ſo as to make the Middle of its Stroke at the Height of 30 Feet, or at the middle of the way from the Water to be rais'd, to the Delivery at top.

The 7th Figure drawn by a larger Scale, repreſents the three Cylinders, which are here made of Copper in their juſt Proportions: And for the ſake of thoſe that would conſider this matter fully, I have here given their Lengths, Diameters within and without, and Thickneſs.

	Outer Cylinder or Barrel, D1 D2.	Middle or hanging Cylinder, in which the Stroke is made C1 C2 *cc*.	Inner Cylinder or plug cloſed at top by a Cap, and moving up and down with the Barrel to which it is joyn'd at bottom. *dddd*.
	Inches	*Inches*	*Inches*
Length	30	29,0	31,2
Diameter within.	6,74	6,35	6,03
Thickneſs	0,10	0,08	0,13
Diameter without	6,94	6,51	6,29

Here B B repreſents part of the Elbow of *Fig.* 5. or of the forcing Pipe of *Fig.* 6. But as the Spaces between the Cylinders are ſo ſmall, as not to be viſible even in a large Draught made by a Scale; I have here given three more Draughts of the three Cy-

Cylinders, where the Height is agreeable to the Scale of the 7th Figure, but the Diameters of the middle and inner Cylinders are made leſs than they are in the Engine, to make the Space between (where the *Mercury* riſes and falls) viſible; and the Cylinders themſelves are repreſented by ſingle Lines.

The Quantity of *Mercury* us'd in this Engine is 36 ½ Pounds, which being pour'd in between the outer and inner Cylinder, riſes up to the Height of 16. Inches.

When the Barrel is pull'd up (as in *Fig.* 9,) ſo as to have the middle Cylinder within an Inch of the Bottom of the Barrel; the *Mercury* on both ſides the middle Cylinder will riſe up to the Height of 23,1 Inches, that is about two Inches below the Cup D1, to the Line *q q*.

When the Barrel is going down to fill the ſucking Pipe and middle Cylinder C, the *Mercury* in the inner Shell will be 25 Inches high, and only 13 in the outer Shell, *Fig.* 9, where the ſhaded part repreſents the ☿.

At the End of the ſucking Stroke the *Mercury* is up to the top of the inner Cylinder, and ſcarce an Inch in the outer Shell. *Fig.* 8.

In raiſing the Piſton from forcing to Sucking, the firſt 1 ¼ Inch drives the *Mercury* out of the inner Shell, and raiſes it in the outer Shell 13,28 Inches.

The Depth of an Inch of Water in the middle Cylinder above the inner one or Plug is equal to a Space in the outer Shell of 13,28 Inches, and ½ of an Inch is equal to the ſame Height in the inner Shell.

Therefore when the *Mercury* is equally high in both Shells, a Motion of ¼ of an Inch of the Barrel will charge for Suction. That is, upon letting down the Barrel only ¼ of an Inch, the Preſſure of the Atmoſphere in the outer Shell will raiſe the *Mercury* in the inner one 13,28 Inches, at the ſame time, that it puſhes up the Water from the Well 13 Foot and a half high into the ſucking Pipe. And when all the Pipes are full, if the *Mercury* be equally high in both Shells, upon raiſing the Barrel one Inch, the *Mercury* will riſe 13,28 Inches in the outer Shell; which I call charging for forcing; becauſe in continuing to raiſe the Barrel, the forcing Valve immediately riſes, and the Water comes out at top during the reſt of the Stroke, which is 12 Inches, and delivers 1,6 Gallon of Water, Wine Meaſure.

Fig. 10. repreſents the forcing Stroke half way up; with the ☿ 17 Inches in the outer Shell 4 Inches in the inner, and the whole ſpace at bottom under the middle Cylinder 7 Inches.

From this it appears, that in the whole Stroke of 13 Inches in Length, there is only $\frac{1}{4}$ of an Inch loſt to charge for Suction, and in the next ſtroke, which is likewiſe of 13 Inches there is only one Inch loſt to charge for forcing; ſo that in a Motion of 26 Inches, there is but 1 $\frac{1}{4}$ Inch, or about $\frac{1}{20}$ part ineffectual. But this is owing to the too large ſpace of the outer ſhell, which contains 4 times more than the inner one, becauſe the Cylinders were only hammer'd, and not turn'd; for if the outer ſpace had been no bigger than the inner, then $\frac{1}{4}$ of an Inch of the ſtroke would have charg'd for forcing; ſo that only $\frac{1}{2}$ an Inch in 26, or $\frac{1}{52}$ part of the whole ſtroke would have been ineffectual; and in that Caſe, $\frac{2}{5}$ of the Quantity of *Mercury*, or a little more than 12 Pounds, would have been ſufficient.

There may ſtill leſs *Mercury* be us'd, if the middle Cylinder be made of Plate Iron turn'd on the outſide, and bor'd within, the outer Cylinder bor'd, and the inner one turn'd; ſo that if the Work be well perform'd, eight or ten Pounds of *Mercury* will be ſufficient in this Engine, tho' the Bore of the middle Cylinder, or Diameter of the Pillar of Water which is rais'd, be of 6,35 Inches. If the Bore of the ſaid Cylinder was but 3 Inches, leſs than 3 Pounds of Mercury would ſuffice, and leſs than ſix if there were two Barrels, in order to keep a conſtant ſtream thro' a Pipe of almoſt the ſame Diameter. This will very much leſſen the Expence of Mercury, which would otherwiſe be an Objection againſt this Engine; and by making the inner and outer Cylinder of hard Wood, as *Box*, or *Lignum Vitæ*, the Coſt of the Engine may ſtill be reduc'd. But if the Engine be very large, Caſt Iron bor'd will be proper for the outer Cylinder, and Caſt Iron turn'd on the outſide for the inner Cylinder or Plug, and hammer'd Iron bor'd and turn'd for the middle Cylinder.

There is an Objection, which ſeems at firſt to take off the intended Advantage of this Engine, which is this, *viz.* That inſtead of the Friction of the Leather of a Piſton, when we lift up our Barrel to force, the Reſiſtance, that the *Mercury* finds to riſe in the outer Shell, is at leaſt as great as the Friction that we avoid. Now that Reſiſtance is never greater than the Weight of a concave Cylinder of *Mercury*, whoſe Height is the greateſt to which the *Mercury* riſes in the ſaid Shell, and the Baſe is the Area of the Shell it ſelf. This Weight in our Engine is equal to 57,5 Pounds, and therefore one would think it greater than the Reſiſtance made by the Friction of a Piſton. But if it be conſider'd, that in the Deſcent of the Barrel for ſucking, the *Mercury* ſhifts immediately into the inner Shell, riſing to the ſame Height, and ſtill keeping the ſame Baſe; the aforeſaid Weight of 57,5 Pounds helps down the Barrel, and facilitates the overcoming of the Force of the Atmoſphere, conſequently the

Weight of the *Mercury* being balanc'd, is no Hindrance, whether you work with a ſingle or a double Barrel.

There remains only then the Hindrance by loſs of time in the Beginning of any Stroke: But I have ſhew'd that to be but $\frac{1}{32}$ part of the ſtroke. I have found that the beſt Engines now in uſe generally loſe near $\frac{1}{5}$ of the Water that they ought to give, according to their Number of Strokes. And Mr. *Henry Beighton*, an ingenious Member of this Society, having a great many times meaſur'd the Water that is rais'd by Engines in Mines, found that ſome Engines loſt $\frac{1}{4}$, and none ever loſt leſs than $\frac{1}{5}$ of what they ought to give according to the Number of the Strokes in their Pumps, whatever auxiliary Powers they were mov'd with.

There is indeed another Objection, but ſcarce worth notice; which is, that ſome Particles of *Mercury* will mix with the Water that is rais'd, and make it unwholeſome; but no body that conſiders Specifick Gravity, will imagine any ſuch thing. However, to ſatisfie thoſe that might ſtill apprehend it, it is to be obſerv'd, that none of the Water that is rais'd comes near the Mercury: For in the Cylinder C, and part of the Elbow B, (*Fig.* 5.) there is always above the Mercury a certain Quantity of Water that riſes and falls with the Barrel, and never goes into the forcing Pipe. The ſame happens alſo in the Machine of *Fig.* 6. for the Water having once run into the Cylinder C, all that is rais'd afterwards, comes thro' the forcing Valve without coming down to the Mercury.

Provided Care be taken to make the Barrel with its Plug tight, I don't ſee that this Machine will want Repair in a long time, except ſome of the auxiliary Powers be out of order, which do not relate to this Invention. The Numbers given will ſerve to examine the Truth of what I have aſſerted concerning the Motion of the Mercury: And from them one may make Tables to ſerve to proportion theſe Engines for raiſing any Quantity of Water to any Height, according to the Power one has to apply.

III. *Part of a Letter from Mr.* Peter Derante, *Chirurgeon in* Waterford, *to Mr.* John Douglas, *Chirurgeon, F. R. S. concerning the coming off of the* Scapula *and Head of the* Os Humeri, *upon a Mortification.*

ON the 5th of *November*, 1713, One *John Fletcher*, on board the *Neptune* of *Leverpool*, had the Misfortune to break the *Radius* and *Ulna* (of his left Arm) and their Ends burſt thro' the Skin. He was immediately dreſs'd by the Surgeon of the Ship with the common Aſtringents and Bandage; about five or ſix Days afterwards I was ſent for to go down to *Paſſage* to ſee this Man. When I came there, I found his Surgeon along with him, who told me, That the Man would do very well, becauſe he was in no pain at all; however, I deſir'd him to take off the Dreſſings, and let me look on't. Which being done, I found it was black and inſenſible from his Fingers to his Shoulder, and therefore adviſ'd the Extirpation of it immediately, but his Surgeon oppos'd it; however, I ſcarified it in ſeveral Places, and very deep, and then apply'd a warm Dreſſing. Next Day the Ship put to Sea, and the poor

Creature was ſent to *Waterford*, and committed to my Care. As ſoon as I could get my *Apparatus* ready, I cut off his Arm (in preſence of Dr. *Renet*, and Mr. *Carr* an Apothecary) as high as poſſible; then I cauterized the Stump, which was perfectly mortified as high as the *Acromion*. Next Day I perceiv'd the Mortification ſpreading toward the lower Angle of the *Scapula*, then I rubb'd the Edges of the Mortification with armed Probes dipt in a Solution of *Argentum Vivum* in *Aq. Fort.* which compleatly anſwer'd my Intention; for from that time the Mortification ſpread no further; next Dreſſing I ſcarified and cauterized all the mortified part, and dreſs'd it *S. A.* I continued this Method for ſeventeen or eighteen Days, and then the Sloughs began to ſeparate and caſt off daily; ſome time afterwards the *Scapula* began to part from the *Os Humeri* and *Clavicula*, and at length came out whole; I was then put to it what to do about the ſtump of the *Os Humeri*, which ſtill adhered to the *Pectoralis* and *Latiſſimus Dorſi*; but in a little time it alſo ſeparated and came away, without any Hemorrhage ſucceeding, which I expected. I was afterwards obliged to cut off part of the *Clavicula*, before I could cicatrize the Wound, which was ſoon afterwards accompliſhed. I have the *Scapula* and Head of the *Os Humeri* ſtill in my Cuſtody, and have ſhewn them to many Surgeons and Phyſicians. All *Waterford* is Witneſs of this Fact, and they called him ever afterwards *Three Quarters of a Man*.

I am, &c.

Waterford, *Jan.* 172$\frac{1}{2}$.

Pet. Derante.

IV. *Hydatidum ingenti copiâ in Abdomine repertorum Hiſtoria. Communicavit cum Societate Regiâ* Johannes Thorpe, *M. D. S. R. S.*

RHedarius Excell. S. D. Baronis de Penterrider, ætatis 58 annorum, Temperamenti ſic dict: Cholerico Phlegmatici, Staturæ quadratæ, non obeſus ſed robuſtus, bene appetens, officio ſuo bene fungens. nec vino, nec liquoribus ſpirituoſis immoderatè utens, bono ſatis Regimine vivens præterito Auguſti Menſe auxilium meum petiit, pro Abdomine fortiter tumente & tenſo, Aſcitico minimè abſimili, urinâ attamen bonæ notæ & in quantitate ſufficiente optimè prodeunte, pedum nullo œdemate comparente, Reſpiratione liberrimè procedente ventre autem in dies creſcente (qui Menſe Junio tumere incepit) ſanguine in vaſis hæmorrhoidalibus ſtagnante, nodos ibidem magnos elevante & interdum dolorificè furente, Appetitu ac Viribus nunquam proſtratis.

Fateor Ænigma mihi fuiſſe an Tumor ſit ſiccus humiduſne? Ad ſatisfaciendum verò Indicationibus, in ſpecie hæmorrhoidibus ſuppreſſis, medicamenta chalybeata cum Purgantibus mixta exhibui, per quæ fluxus hæmorrhoidum mox excitatus, dolores ſedati, & magna ſordium quantitas cum aliquali levamine per tubulum inteſtinalem fuit eliminata. Poſtea ad exemplum Etmulleri in hydrope ſicco, Nitr. præparat. quotidie ad unc. ſſ. item Mercurium vitæ & omnis generis Diuretica mitiora, fortiora, &c. omnia incaſſum dedi.

Abhinc Patiens ſub ſanitatis recuperandæ ſpe, pro more Vulgi auxilium Empiricorum imploravit, qui ſtatim Aureos ipſi montes promiſerunt, multisque ſaginatum Arcanis miſerum in malis deſeruerunt.

Abdomine igitur mole ſuâ maximè urgente, ad Paracenteſin ſine morâ progrediendum erat; quam die 14 Januar. Chirurgus Expertiſs. D. *Anel.* unanimi noſtro conſenſu Patienti adminiſtravit; contenta verò conſiſtentiam gelatinoſam nacta per anguſtius Cannulæ immiſſæ ſpatium exire non poterant. Inciſio ergo cum Lancetta facienda erat; quâ factâ, materia gelatinoſa coloris griſei & lutei, albumine ovorum ſpiſſior, cum magna quantitate globulorum alborum ſphæricorum, triangularium, vermicularium, gemellorum, teſticuli- & ovarii-formium, &c. magnitudinem nucis avellanæ adæquantium primo die ad menſuras ſex evacuata erat, ægro quantum ad vires optimè ſe habente. Poſtero die, cum facile judicare licebat, factam inciſionem non ſatis eſſe amplam pro exitu majoribus hujus naturæ globulis concedendo, Lancettâ igitur vulnus ampliabatur, per quod globuli ovum Columbinum magnitudine ſuperantes magno numero cum ſex iterum gelatinæ menſuris, & magno fruſto maſſæ cujuſdam albæ portionem omenti referentis, de novo excreti erant. Cui Maſſæ plures dictorum globulorum filamentis fibroſis adhæreſcebant.

In examinatione materiarum, gelatina ſuper carbonum ignem indureſcit & albeſcit, pelliculâ deſuper ſe formante.

Spiritu Vitrioli, Spir. Sal. Ammon. vol. Ol. Tart. per deliqu. Spir. Vin. rectif. Aceto affuſis, manifeſti nihil videtur, quàm levis induratio materiæ ab uno quoque iſtorum liquorum, etſi naturâ inter ſe invicem contrariorum.

Globuli coloris ſunt albiſſimi, ſenſui tactûs reſiſtentes; rupti liquorem album chylo ſimilem fundunt, membranâ ſeu pelliculâ manifeſtâ cincti.

Maſſa iſta albicans quæ portionem omenti refert, etſi craſſitie pollicem adæquet, ſubſtantiam purè membranaceam habet

Numerus globulorum majorum & minorum ad ſeptem vel octo millia ſe extendit.

Hodie qui eſt incluſivè decimus ſextus ab operatione dies, æger quàm optimè ſe habet, venter eſt mollis, planus, vulnus pulcherrimum, urina, pulſus, reſpiratio bona, conſequenter nulla febris, ſomnus tranquillus. Evacuatio materiæ gelatinoſæ durantibus 13 diebus fuit copioſa, inodora, nunc purè ſeroſa & parca, ſed fortem odorem ſpirans, immiſſumque Catheterem argenteum nigricans, quod corporibus heterogeneis corruptis magis quàm viſceribus gangrænâ prehenſis attribuimus, quoniam febris abeſt. Hodie poſt injectionem Decocti ex Enula Campan. Agrimon. &Mell. cum Tinctur. Aloes. Myrrh. & Balſam. du Commandeur, rupti pellicula globuli fuit excreta, quem in ſtatu ſuo naturali ovum gallinaceum mole adæquâſſe, fortaſſis ſuperâſſe, certum eſt.

Appetitus & vires nunc ſunt quæ nobis faceſſunt. Medicamenta porrigo Traumatica, Alexiteria & Confortantia invicem combinata juxta victum Analepticum. Φαινόμενον notatu dignum examini verſatiorum in arte amicè diſcutiendum traditur, & ut animi ſui ſenſa nobiſcum communicare haud graventur, obnixè rogatur. Dabam Pariſiis d. IV. Calend. Januar. M DCC XXII.

Barthol. Anhorn, ab Hartuiſs.

V. *An Account of ſome Experiments made with the Bile of Perſons dead of the Plague at* Marſeilles, *with what appear'd upon the Diſſection of the Bodies; as likewiſe ſome Experiments made with the Bile of Perſons dead of other Diſeaſes. By the Learned and Curious Dr.* Deidier, *Profeſſor of Phyſick in the Faculty of* Montpellier. *Communicated by* John Woodward, M D. Prof. Med. Greſh. *and* R. S. S.

Experiments made upon the Bile of Perſons dead of the Plague.

Experiment I.

THE Human Bile, taken from the Gall-Bladder of the Bodies of thoſe that dy'd of the Plague at *Marſeilles*, has been always found to be of a black and greeniſh Colour. It has become conſtantly of a laſting Graſs Green upon the mixing of Spirit of Vitriol with it; and always very yellow when we mixt Oil of Tartar *per deliquium*, or the Alcalous fixt Salt of the ſame diſſolv'd in a ſufficient Quantity of Water. Theſe two Colours, green, and yellow, have continued for whole Months. The ſaid Bile has become of a black Colour, like Ink, but ſoon fading, by the Affuſion of Spirit of Nitr

Experiment II.

The Bile, taken from the Gall-Bladder of the Bodies of thoſe that dy'd of the Plague, having been pour'd into a Wound made on purpoſe in different Dogs, has render'd them thereupon melancholy, drowzy, and without caring to eat. All theſe Animals dy'd in three or four Days with the eſſential Marks of the true Plague, declared by Buboes, Carbuncles, and gangrenous Inflammations in the *Viſcera*, in the ſame manner as in the human Carcaſſes from whence the Bile was taken.

Experiment III.

A Drachm of the ſaid peſtiferous Bile having been mix'd with two Ounces of Fountain Water made lukewarm, and injected into the Jugular Vein of ſeveral Dogs, has render'd them preſently drowzy, and has kill'd them in four Hours, with gangrenous Inflammations, the Heart ſtuffed full of black thick Blood, the Liver ſwell'd, and the Gall-Bladder full of green Bile.

Experiment IV.

The ſame Quantity of Bile, injected by the Crural Vein, has produc'd in the Dogs a Heavineſs in about an Hour. They have had ſuch a ſtrong loathing of their Food, that they would not eat or drink the leaſt matter after the Injection was made. They piſs'd frequently, eſpecially if they were ſtirr'd. On the third Day there appear'd conſiderable Tumors under the *Axilla*, and on their Thighs, about 3 Inches from the Wound. The Wound turn'd to a Gangrene, and the Creature uſually dy'd on the fourth Day, with all the Marks of the Plague.

Experiment V.

A Dog, of the Hoſpital at the *Mail*, in *Marſeilles*, who follow'd the Surgeons when they went to dreſs the Sick, uſed greedily to ſwallow the corrupted Glands, and the Dreſſings charged with Pus that they took off the Plague-Sores; he lick'd up the Blood that he found ſpilt on the Ground in the Infirmary; and this he did for about three Months, and was always well, gay, brisk, full of Play, and familiar with all Comers. We injected into the right crural Vein of this Dog about a Drachm of the peſtiferous Bile, mix'd with two Ounces of warm Water. He dy'd the fourth Day, as the others did, with a Bubo on the wounded Thigh; on which likewiſe there were two Carbuncles, and the Wound gangren'd. What we took particular Notice of in this Dog was, that after the Injection, both when he was living, and after he was open'd, when dead, he had a very ſtinking Smell, which we did not obſerve in any of the others. He had moreover a conſiderable Hæmorrhage from the Wound, the Night before he dy'd, having ſtruggled hard to eſcape out of his Confinement.

Experiment VI.

The 2d of *May* having injected about a Drachm of humane Bile, taken from Perſons dead of the Plague, mix'd with two Ounces of warm Water, into the Crural Vein of a Dog, the Creature was preſently drowzy, refus'd his Food, and dyed about the third or fourth Day after the Injection, with all the inward and outward Marks of the Plague that the others had.

Experiment VII.

The ſixth of the ſame Month we collected the Bile of this Dog dead of the Plague, and injected it by the Crural Vein into the Blood of another Dog, who, preſently after the Injection, had Convulſive Motions all over him, which were follow'd by a Lethargick Heavineſs. On the ſecond Day after the Injection, there appear'd a Carbuncle upon the great Pectoral Muſcle on the right ſide The third Day there aroſe a conſiderable Bubo on the Thigh; and the Dog dy'd the ſame Day. Upon opening the Body, we found the fore part of the Breaſt all gangren'd under the Teguments; and, in the inward parts, the *Viſcera* were full of black clotted Blood; as in all the reſt. The outward Surface of the Lungs was all Purple; The Heart was ſwelled as big again as uſual, with the four Cavities full of black clotted Blood. The Creature had liv'd three Days from the Injection without eating or drinking.

The 10th of *May* we injected the Bile of this ſecond Dog into the Crural Vein of a third Dog, who was thereupon ſeiz'd with violent Convulſions, and different convulſive Motions, for about half a quarter of an Hour. When he was recover'd from theſe, he appear'd dull, and ſleepy; he vomited with violent Strainings. The Vomiting was followed with a Hick-up. He eat a little boiled Meat, having faſted a good while before the Injection was made; but he vomited it up two Hours after he had taken it. He died the third Day with the ſame Symptoms of the Plague that the other Dogs had.

The State of the Bodies of Perſons dead of the Plague, from which we took the Bile for the Experiments beforemention'd.

The firſt Body.

ONE *Beleſleur*, a Soldier, of 25 Years of Age; of a ſtrong robuſt Conſtitution, having a flattiſh Bubo growing on the hollow of the right Groin, dy'd delirous. We found in his Carcaſs the Heart of an extraordinary Bigneſs, ſtuffed with black clotted Blood. His Lungs covered with a livid Purple, and ſticking a little to the *Pleura*. The Liver was double the natural Bigneſs, and ſtuffed with a thick Blood. The Gall Bladder was full of a black and greeniſh Bile. The *Dura* and *Pia Mater* by their Blackneſs ſeem'd to have been ſeized with a gangrenous Inflammation. The inward Subſtance of the Brain was ſprinkled over with an infinite number of ſmall livid Spots.

The ſecond Body.

Mary Piſanne, aged 30 Years, of a ſanguine Habit, had a *Bubo* under her right Arm-pit, with a *Delirium* that was followed by a mortal Sleepineſs. In the opening of her Body, we remark'd, that the Lungs were in their natural State. The Heart was of a prodigious bigneſs, full of black coagulated Blood, with the left Auricle livid and gangrenous. The Liver, which was much enlarged, was all cover'd with Purple;

ple; and the Gall Bladder fill'd with a black and greeniſh Bile. The Head was not open'd.

The third Body.

Peter Moulard, of a tender, feeble Conſtitution, about 14 Years of Age, had a *Bubo* under the hollow of the right Groin very deep, and that never came well out. He became delirous with Convulſions, in which this Child dy'd. We found in his Body his Heart of double the bigneſs that it ought naturally to be, containing a black thick Blood: His Lungs were beſprinkled with livid Spots. The Gall Bladder full of a black and greeniſh Bile. The Head was not open'd.

The fourth Body.

Jean Raynaud, Cook, aged about 25 Years, of a melancholy Temperament, had the whole Habit of his Body cover'd with a purple livid Colour, and a *Bubo* under his left *Axilla*; he dy'd in a delirous Phrenzy. We found in his Carcaſs two Abſceſſes, one between the Teguments and the left great pectoral Muſcle, and the other in the Breaſt between the *Sternum* and the *Mediaſtinum*. His Heart was very large, fill'd with black thick Blood. The right Auricle was of the Breadth of three Inches; the left was in its natural State. His Lungs were cover'd with little livid Spots, remaining ſoft and pliant, without any hardneſs in the Subſtance of them. The Liver was larger and harder than ordinary, and was alſo full of livid purple Spots; and the like were found in the Subſtance of his Brain, of which all the Veſſels were fill'd with black thick Blood.

The

The fifth Body.

Jaques Audibert, aged about 35, of a melancholy Complexion, four Months after he had been cured of the Plague, the Mark of which was a *Bubo* in the fold of the right Groin, which came well to a Suppuration, was attack'd afreſh with three Carbuncles; one in the middle of the Arm, and the other two in the fore Arm. He had but little Fever, with ſome ſmall Sickneſs at Stomach; but a *Delirium* coming of a ſudden, carried him off. Upon opening his Body, we found his Heart of a prodigious Bigneſs. The right Auricle being of the breadth of five Inches; and the left of three. We found a little Impoſtume upon the Body of the *Aorta*. The Lungs were cover'd with livid Spots, and the Liver appear'd gangrenous. The Gall Bladder was of a very black Colour. The *Duodenum* and the *Rectum* were inflam'd. We did not open the Head.

The ſixth Body.

Venture Cajole, about 40 Years of Age, of a melancholy Temperament, dy'd without any outward Eruption on the third Day, of a violent Fever, with a Sleepineſs. We found in the Body the *Mediaſtinum* torn towards the uppet part. The *Pericardium* was of a livid Colour. The Heart larger than in its natural State, by the ſwelling of its Ventricles full of black thick Blood, as in the other Carcaſſes. The Liver alſo was very large, and of a livid Colour, with a Carbuncular Puſtule on the ſide of the Gall Bladder; this was fill'd with very black Bile.

The ſeventh Body.

Marguerite Bachaire, aged 18 Years, of a lively vigorous Conſtitution, having two Carbuncular Puſtules on the middle and inſide of the Thigh, with a ſharp Pain in the Head, dy'd delirous. We found in her Body the Coverings of the Brain of a blackiſh red; The Cortical part of a livid Colour, and the Medullary ſprinkled with a few black Spots; The Heart of a prodigious Size, full of a thick black Blood: The Liver alſo very large, and the Gall Bladder very full of a black and green Bile. There were ſeveral livid Spots upon the Surface of the Inteſtines.

The eighth Body.

Louiſe Belingere, 20 Years of Age, having a *Bubo* in each fold of the Groin, died very ſuddenly without any dangerous Symptoms. We found in her Carcaſs the Heart all cover'd with a livid Purple, much larger than natural, fill'd with thick black Blood, having a *Polypus* in each Ventricle. The Lungs were in their natural State. The Liver of a prodigious Greatneſs; and the Gall Bladder full of Bile, of a deep green Colour.

The ninth Body.

One nam'd *Rampeau*, a Peaſant, aged about 20 Years, of a ſanguine robuſt Conſtitution, having had a Carbuncular Parotide for the Space of eight Days, accompany'd with a burning Fever, was carried to the Hoſpital the 2d of *May*, where he dy'd the 5th. We found the outward part of the left ſide of the Lungs cover'd with a livid Purple. The Heart was of double th natural Bigneſs, having ſcarce any Blood in the Ventricles, whoſe Cavities were fill'd each with a large *Polypus*, that on the right ſide having dilated the Auricle to the breadth of four Inches. The Liver alſo was larger than ordinary, and the Gall Bladder was full of a black and green Bile.

The Experiments, and the opening of the Bodies mentioned above, were made in the Hoſpital at the *Mall*, and in the Apothecary's Shop of the Reverend the Reformed Fathers of *Marſeilles*, during the Months of *February*, *March*, *April*, *May*, in Company of Meſſ. *Roberts* and *Ribeaud*, Doctors of Phyſick.

The following Experiments were made at Montpelier, *in the Hoſpital of St.* Eloy, *during the Months of* September, October, *and* November, *in company with Monſ.* Fizes, *Dr. of Phyſick, and Profeſſor of the Mathematicks, and Meſſieurs* Duli *and* Morel, *young Surgeons of the ſaid Hoſpital.*

A Soldier, between the Age of 20 and 25, of a lively brisk Temper, being ſick in the Hoſpital of St. *Eloy* of an ordinary malignant Fever, died about the 15th Day by a Defluxion upon his Breaſt. His Lungs were found very much blown up, filling all the Cavity of the Breaſt, and adhering to the *Pleura*. Having remark'd that the Bile in the Gall-Bladder was of a bright Graſs green Colour, we gather'd it for the following Experiments.

This Bile, being mingled with four Ounces of warm Water, was part injected into the Jugular Vein of a Dog, and a Compreſs ſoak'd in the reſt of the Liquor was applied to the reſt of the Wound. The Creature thereupon appear'd heavy and ſleepy, and would neither eat nor drink; the third Day he eat and drank willingly. The Compreſs falling off the fourth Day, the Wound was diminiſhed one half, and was healed by little and little, and the Dog grew perfectly well.

The eighth Experiment.

A Peaſant of about 50 or 60 Years old, of a melancholy Temperament, had been near a Month in the Hoſpital with an ordinary malignant Fever, having alternatively a *Delirium* and a Sleepineſs. After his Death, the Bile was found extreme thick, and black as Ink, and a great deal of it. We put about a Drachm in a Wound made for that purpoſe on the outer part of the right Thigh of a Dog, thruſting in Pledgets dipt in the ſaid Bile diluted, into the Wound. There did not appear any Change in the Dog. We made him ſwallow ſome of the ſame Bile, without loſing his Appetite; and ſeeing he was like to do well, we left the Wound to it ſelf, which heal'd in fifteen Days only by the Dog's licking it.

The ninth Experiment.

Being willing to inject ſome of the ſame Bile into the Crural Vein of another Dog, and the Syringe being ſtopt by the too great Thickneſs of the ſaid Bile, the Injection could not be made; wherefore we took a Pledget ſoak'd in as much of this Bile diluted, as it could take up, and apply'd it to a freſh Wound made in the inſide of the right Thigh. The Pledget was faſten'd within the Skin by ſome Needles. This Application produced no conſiderable Alteration in the Dog; he neither appear'd ſleepy nor ſtomachleſs; but lick'd his Sore readily enough; and after the Pledget was fallen off, the Wound heal'd as in the foregoing Experiment.

The tenth Experiment.

About a Drachm of the ſame black Bile drawn from the ſame Carcaſs, and mixed with warm Water, was injected into the Jugular Vein of another Dog. The Creature was not incommoded thereby, but was as brisk as before the Injection, only he appear'd very thirſty and he drank with Greedineſs. The next Morning being willing to view the Wound, we found it black and dry, and the Dog becoming ſurly, bit one of the Aſſiſtants. The two Ligatures made for the Injection were taken away without our ſeeing any Blood to run out. We apply'd a Doſſil, charged with the ordinary Digeſtive, and kept on by a Bandage; and about four Hours after the Dreſſing, we found the Dog dead, having lived 23 Hours after the Injection. Having opened him, we found that his Heart beat ſtill with Violence, and the beating ceaſing, there was no Blood either in the Ventricles or the Auricles. This Liquor crowded together in the great Veſſels, appear'd of a lively red, and very fluid, without any of thoſe Concretions that we conſtantly obſerved in all the Bodies that dy'd of the Plague. Here appeared neither internal nor external Marks of the Plague.

The eleventh Experiment.

An Inhabitant of *Montpellier*, aged about 30 or 35, very fat and robuſt, of a ſanguine Complexion, having had a Fall upon the Pavement, had received a ſimple Wound on the upper part of his Forehead on the right ſide. This being neglected, brought on an *Eryſipelas* all over his Face, which was accompany'd with a Swelling of the left *Parotide*. This appear'd and diſappear'd thrice from Morning till Night. The *Eryſipelas* came ſuddenly on; he grew delirous, and died after fifteen or twenty Days Illneſs, reckoning from the Fall. In the opening of the Body, we found a quantity of Water between the Skull and the *Dura Mater*. The Brain, which was firmer than ordinary, was a little red, and part of the *Pia Mater* covering the hinder part of that *Viſcus*, appeared inflam'd. There was about half a Septier of Water of a yellowiſh Colour, ſhed in the Cavity of the Breaſt. The great right Lobe of the Lungs was a little hard on the upper part. The Heart had a Polypoſe Concretion in each Ventricle: We found likewiſe two Pints of limpid Water got forth into the lower Belly. All the Fat of his Carcaſs was yellow. The Liver appeared to us a little ſwell'd; and the Gall Bladder almoſt empty, not having above two Drachms of yellow Bile in it. That Bile of this Carcaſs, mixed with two Ounces of warm Water, was injected into the Crural Vein of a Dog. The Creature eat and drank heartily after the Injection, and did not appear at all incommoded. The Wound bleeding much, we were obliged to fill it with reſtringent Powders, kept in by a Pledget and a convenient Bandage. Twenty four Hours after, the Dreſſing was taken away, the

Wound appeared black and dry. The Dog licking it, it ſuppurated the next Day, and afterwards became red and well colour'd; and the Wound was leſſen'd one half in eight Days, during which time, the Dog appeared in perfect Health.

The twelfth Experiment.

Eight Days after the foregoing Experiment, the Dog, that was the Subject of it, was kill'd by about half a Drachm of Powder of *Hungarian Vitriol*, diſſolved in a ſpoonful of warm Water, which we injected into the Jugular Vein. The Creature dy'd on the ſpot, with univerſal Convulſions. His Heart was found full of grumous Blood reduced to a kind of thick Pap, but without any Clods. The Bile of this Dog was yellow and in little quantity. Not being able to inject it into the Crural Vein of another Dog, becauſe the Vein was too ſmall, we contented our ſelves to dip two Compreſſes in this Bile, which we apply'd and kept under his Skin, by two Wounds made on purpoſe in this ſecond Dog: There arriv'd no notable Change. We obſerved in theſe two Dogs no Marks either internal, or external, of the Plague.

Done at Montpelier *the 4th of* December, 1721.

VI. *The way of proceeding in the* Small Pox inoculated *in* New England. *Communicated by* Henry Newman, *Eſq*; *of the* Middle Temple.

1. WE make uſually a Couple of *Inciſions* in the *Arms* where we make our *Iſſues*, but ſomewhat larger than for them, ſome times in one *Arm*, and one *Leg*.

2. Into theſe we put bits of *Lint*, (the patient at the ſame time turning his Face another way, and guarding his Noſtrils) which have been dipt in ſome of the *Variolous Matter* taken in a Vial, from the Puſtules of one that has the *Small Pox* of the more laudable Sort, now turning upon him, and ſo we cover them with a Plaiſter of *Diachylon*.

3. Yet we find the *Variolous Matter* fetched from thoſe, that have the *inoculated Small Pox*, altogether as agreeable and effectual as any other. And ſo we do what is taken from them that have the *Confluent Sort*.

4. Within Four and Twenty Hours, we throw away the *Lint*, and the *Sores* are dreſſed once or twice every Four and Twenty Hours, with warmed *Cabbage Leaves*.

5. The Patient continues to do all Things, *as at other times*, only he expoſes not himſelf unto the Injuries of the *Weather*, if that be at all Tempeſtuous.

6. About the Seventh Day the Patient feels the uſual Symptoms of the *Small Pox* coming upon him; and he is now managed as in an ordinary *Putrid Fever*. If he cannot hold up, he goes to *Bed*; If his

Head ach too much, we put the common *Poultice* to his *Feet*, if he be very Sick at the *Stomach*, we give him a *gentle Vomit*, yea, we commonly do theſe Things *almoſt of Courſe*, whether we find the Patient want them or no. And we reckon the *ſooner* we do theſe Things, the *better*. If the *Fever* be too high, in ſome Conſtitutions, we *Bleed* a little: And finally, to haſten the Eruption, we put on a Couple of *Bliſters*.

7. On or about the Third Day from the Decumbiture the *Eruption* begins. The Number of the *Puſtules* is not alike in all, in ſome they are a *very few*, in others they amount to an *Hundred*, yea, in many they amount unto *ſeveral Hundreds*; frequently unto more than what the Accounts from the *Levant* ſay is uſual there.

8. The *Eruption* being made, *all Illneſs* vaniſhes; except perhaps a little of the *Vapours* in thoſe that are troubled with them; there is nothing more to do, but to *keep Warm*, drink proper Tea's, eat Gruel, Milk Pottage, Panada, Bread, Butter, and almoſt any thing equally Simple and Innocent.

9. Ordinarily the Patient *ſits* up every Day, and entertains his Friends, yea, ventures upon a *Glaſs of Wine* with them. If he be too Intent upon hard *Reading* and *Study*. we take him off.

10. Sometimes. tho' the Patient be on other Accounts eaſy enough, yet he *can't Sleep* for divers Nights together. In this Caſe we do not give him *Anodynes* or *Opiates*, becauſe we find, That they who have taken theſe Things in the *Small Pox* are generally peſtered with miſerable *Biles* after their being recovered. So we *let them alone*; their *Sleep* will come of it ſelf, as their *Strength* is coming on.

11. On the Seventh Day the *Puſtules* uſually come to their Maturity; *and ſoon after* this they go away,

as thoſe of the *Small Pox* in the *Diſtinct Sort* uſe to do.

12. The Patient gets abroad quickly, and is moſt ſenſibly *Stronger*, and in *better* Health than he was before. The Tranſplantation has been given to *Women in Child-bed*, Eight or Nine Days after their Delivery; and they have got earlier out of their Child-bed, and in better Circumſtances, than ever in their Lives. Thoſe that have had ugly *Ulcers* long running upon them, have had them healed on, and by this *Tranſplantation*. Some very feeble, crazy, *Conſumptive* People, have upon this *Tranſplantation*, grown hearty and got rid of their former Maladies.

13. The *Sores* of the *Inciſion* do ſeem to dry a little in Three or Four Days of the Feveriſh Preparation for *Eruption*. After this there is a *plentiful Diſcharge* at them. The diſcharge may continue a little while after the Patient is quite well on other Accounts; But the *Sores* will ſoon enough dry up of themſelves; but the *later*, the *better*, as we think. If they happen to be *inflamed*, or otherwiſe Troubleſome, we preſently help them in the ways we do any *Ordinary Sores*.

VII. *A Letter from Dr.* Nettleton, *Phyſician at* Halifax *in* Yorkſhire, *to Dr.* Whitaker, *concerning the Inoculation of the* Small Pox.

SIR,

HAVING too often found with no ſmall Grief and Trouble, how little the Aſſiſtance of Art cou'd avail in many Caſes of the *Small Pox*, I was induced to try the Method of Inſition or Inoculation, which came ſo well recommended by ſeveral Phyſicians from *Turkey*, and which had alſo been lately practiſed in *London*. This I thought was ſufficient to juſtify the Attempt, and what ſucceſs it has met with, I have here ſent, according to your deſire, a faithful Account of, and of every Thing that has been done or obſerved here, relating to that Affair, which might be worth your Notice.

It was in *December* laſt, That I firſt began to put this Method in Practice, and finding it to ſucceed beyond my Expectation in the firſt inſtance, I was encouraged to repeat it upon ſome others, and afterwards ſeveral, ſeeing with how much eaſe theſe got thro' the Diſtemper, were deſirous to have the ſame done to themſelves or their Children; ſo that there are now upwards of Forty here, who have received the *Small Pox* by Inſition; who are all got well thro' the Diſtemper, and are at this time thro' Gods Bleſſing in very good Health. Only one that was inoculated, did Die; of which I ſhall give you a particular Relation hereafter, and refer it to your Impartial Judgment, whether that ought to caſt any Blemiſh upon the Operation or no.

What was done by way of Preparation was chiefly purging with Rhubarb for Children, and ſometimes Vomiting or Bleeding for grown Perſons; and many have had no Preparation at all. But I always found, as far as I was able to judge, That thoſe, whoſe Bodies were well prepared by ſuch proper Methods as their different Ages or Conſtitutions did ſeem to require, had more favourable Symptoms than others in like Circumſtances, where that was omitted.

The Method, which I always took in the Operation, was to make two Inciſions, one in the Arm and another in the oppoſite Leg. It is not material as to raiſing the Diſtemper, whether the Inciſions be large or ſmall; but I commonly found, that, when they were made pretty large, the quantity of Matter diſcharged afterwards at thoſe Places was greater; and the more plentiful that Diſcharge, the more eaſy the reſt of the Symptoms generally are, and they are alſo by this means the beſt ſecured fromany inconvenience, which might follow, after the *Small Pox* are gone off.

At firſt I collected ſome of the Matter from the Puſtules of one, who had the *Small Pox* of the natural Sort, into a ſhell or Vial, and infuſed two or three drops of it into the Wound; but finding it to be very troubleſome and difficult to get any quantity of the Matter, and obſerving alſo, that the leaſt imaginable will be ſufficient for the Purpoſe, I commonly take ſmall Pledgets of Cotton, and ripping the Puſtules, when they are ripe, with the point of a Lancet, roll the Pledgets over them, till they have imbibed ſome of the Moiſture. I put one of theſe upon each Wound, and cover it with any common Plaiſſter till the next Day, when I commonly take away both the Cotton and the Plaiſter, leaving the Wound to it ſelf, only covering it with a ſlight linnen Roller, to defend it from the Air. I have ſometimes rubb'd the Pledget only once over the Wound, without binding it on, which I found to anſwer the End as well; and from ſome other Obſervations I have made, I have been ſurprized to ſee the *Small Pox* produced this way, when I was very well aſſured, the quantity of Matter received into the Veſſels, cou'd not amount to the hundredth part of a Grain.

The Perſons inoculated have not been confin'd to any Regimen, but only to be kept moderately warm; and thoſe, who were grown up, to live very temperate and regular, to keep their Minds eaſy and compoſed, and to uſe proper means to drive away all Fear and Concern. Some have been obliged from the time of the Inſition to abſtain from Fleſh and all ſtrong Liquors; but I found afterwards, that the Eruption did not proceed ſo well, when they were obliged to live too low. Perhaps in warmer Climates, where they are not ſo much accuſtomed to live upon Fleſh, ſuch Abſtinence may be neceſſary; but here I find it beſt to let them eat and drink as uſual, tho' ſomething more ſparingly, till the Fever begins to riſe; and then, but not before, we enjoin ſuch a Regimen as is uſual in like Caſes.

The firſt Thing that occurr'd after the Inſition, was the Inflammation of the Wounds, which commonly happen'd about the fourth Day, when they began to appear very red round about, and to grow a little ſore and painful; in about two Days more they began to digeſt and run. In ſome they begin to run ſooner, and the quantity diſcharged is much greater than in others. I generally found, that in thoſe who diſcharged moſt this way, the Fever was more ſlight, and the *Small Pox* fewer, tho' I have known ſome do very well when theſe places have only appear'd very red, but have ſcarce run any thing at all, as it uſually happens, when the Inciſion is made ſo ſuperficial as not to cut thro' the Skin.

About the ſeventh Day the Symptoms of the Fever begin to come on, which are the very ſame, that we always obſerve in the *Small Pox* of the diſtinct Kind, in the natural way. A quick Pulſe, great heat and thirſt, pain in the Head and Back and about the region of the Stomach, vomiting, doſedneſs, ſtartings, and ſometimes Convulſions. All were not ſeized with all theſe Symptoms, nor in the ſame Degree or Continuance; ſome began on the ſeventh Day, and continued ill without any Remiſſion till after the eleventh; many not till the eighth or ninth Day; and the Fever in theſe was more moderate with great Intermiſſions; and ſome have ſcarce had any illneſs at all. During all this time the Places of Inciſion continued to be very ſore and ſwell very much, ſo as to appear very large and deep, and to diſcharge a great deal of Matter.

On the tenth Day the *Small Pox* moſt commonly did appear, ſometimes on the ninth, and ſometimes not till the eleventh: but I never found that any difference of Age, Conſtitution, or any other Cauſe ever made them vary above one Day from the tenth. The Number was very different, in ſome not above Ten or Twenty, moſt frequently from Fifty to two Hundred, and ſome have had more than could well be numbered; but never of the confluent Sort. Their Appearance was the ſame with thoſe of the diſtinct Kind, they commonly came out very round and florid, and many times roſe as large as any I have obſerved of the natural Sort, going off with a yellow Cruſt or Scab as uſual; tho' it ſometimes happens, eſpecially when the Sores diſcharge a very great quantity of Matter, that they are both few in Number, and do not riſe to any Bulk; but having made their Appearance for Four or Five Days they waſt inſenſibly away.

After the *Small Pox* come out, the Feveriſh Symptoms gradually abate, and when the Eruption is compleated, they uſually ceaſe, without any ſecond Fever, or any farther trouble in any reſpect.

While the Puſtules were riſing, and for ſome time after they were gone, the Sores continued to ſwell and to run very much, the longer they did ſo the better; but they never fail'd to heal up of themſelves after a certain Time.

I very rarely ſaw Occaſion for any Medicines in the courſe of the Diſtemper, only ſometimes when the Symptoms ran very high, I gave a gentle Anodyne, to be repeated as Occaſion ſhould require, and once or twice I thought it neceſſary to bliſter, and to uſe ſuch Medicines as are found to be moſt ſerviceable in the *Small Pox* of the natural ſort. After the Puſtules are gone away, they have always been purged twice or thrice, and ſometimes let Blood, which is all that has been uſually done. But tho' the Practice may ſeem to be very eaſy, yet it is an Affair of ſuch a Nature as to require the utmoſt Care, and I preſume it will never be undertaken without the Advice of Phyſicians to direct a proper Method of Preparation before the Inſition is made, as well as a juſt Regimen afterwards; to watch every Symptom, and lend Nature all proper aſſiſtance, when ever it ſhall be requiſite. Where this is done, with Gods Bleſſing, it will ſeldom fail of being attended with happy ſucceſs.

It has happen'd in one Inſtance or Two, That the Symptoms in the Diſtemper have been worſe than uſual; and ſome few, after the *Small Pox* were gone off, have been ſubject to other indiſpoſitions. Of all which I ſhall give you a particular Account.

* The firſt that was Inoculated, a Boy about a Year and half Old, began to be ill on the Eighth Day, and had a brisk Fever for two Days, then the Puſtules began to appear, which were but few in Number, and roſe very large. The Child was ſoon well, and continued ſo for about a Month, when being carryed out and kept a long Time in the Cold, he fell into a Feveriſh diſorder accompanied with a Cough, and

was ill for Four or Five Days; after that time it went off, and he has ever ſince been in very good Health.

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* *Thomas Thorp*'s Son of *Hallifax.*

* The Second was a Girl two Years of Age, in a Family where they had formerly Bury'd three Children ſucceſſively of the *Small Pox*, and this they fear'd might undergo the ſame Fate. The Fever came on about the ſeventh Day, and ſhe continued very ill till the tenth, on which Day about Noon ſhe had a ſtrong convulſive Fit. In the Evening the *Small Pox* appear'd, and tho' ſhe had more in Number than uſual, yet ſhe grew well as ſoon as they were fully come out, and has continued ſo ever ſince.

† The Eighth and Ninth were in a Family where they had four Children, none of whom had had the *Small Pox* I was call'd to the Eldeſt, who was ſeized in the natural way with the moſt malignant Sort I ever ſaw, attended with the worſt Symptoms that cou'd be, inſomuch that he died on the fourth Day, all full of purple and livid ſpots. The Parents were very deſirous, that any means might be uſed to preſerve the reſt; but here I was in great doubt and perplexity what part to act. I knew very well, That if I ſhou'd venture to make the Inſition, whatever ſhou'd happen wou'd be charged upon that, and it was not improbable, but ſome of them might have already taken the Infection, in which Caſe it was uncertain what the event might be. On the other Hand, if it was omitted, I did very much fear they might all Dye, ſuch inſtances having been known, and the Contagion which was got amongſt them, being of ſuch a de-

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* *Jer. Turner*'s Daughter.
† Mr. *John Symſon*'s Children.

ſtructive Nature. Wherefore I was willing to run the riſque of my Reputation, rather than that the Children ſhou'd all periſh. They were therefore all three inoculated the Day before the eldeſt died, after having told the Parents that I cou'd not anſwer for the Succeſs, in Caſe they had already catch'd the Infection, which would be known if any of them fell ill before the Seventh Day. According as we fear'd, one of them began on the ſecond Day, much after the ſame Manner with the Eldeſt, and the *Small Pox* appear'd on the third Day, or rather an univerſal redneſs all over the Skin, interſperſed with many purple ſpots. There were none of theſe ſpots near the Places of Inſition, which began to ſwell a little, as uſual about the fourth Day, and the *Small Pox* did riſe a little more about thoſe Places than elſewhere; but Nature was too far oppreſs'd with the violence of the Diſtemper, and tho' this Girl continued longer than her Brother, and was not delirous as he was, yet ſhe died on the ſeventh Day. I did not reckon this Child in the Number of thoſe who received the Diſtemper by Inoculation; for I thought there was ſufficient Reaſon to conclude, that ſhe had taken the Infection before; but of this I muſt leave you to judge as you pleaſe, I only give you a Relation of Facts. The other two continued well till the eighth Day, when they both fell ill together. The *Small Pox* came out on the tenth, of a very good Sort, tho' more in Number than ſome others had, and they both got very eaſily thro' the Diſtemper without any Indiſpoſition ſince. It was obſervable, that the Elder of theſe Children, about the time of the Eruption, had many Spots appear'd on him of a deep red Colour, very much like thoſe of the other two Children, which changed in them to Purples afterwards, which the Mother was very much concern'd at, fearing they wou'd prove the ſame in this; but when the *Small Pox* came out, theſe Spots grew gradually leſs, and at laſt quite diſappear'd The other Child had been very ſubject to Convulſions for a long Time, when very young, and it was afflicted with the ſame very much, from the Time that the Fever came on, till the *Small Pox* appear'd.

* The Twenty Seventh was a marry'd Gentlewoman aged about Twenty Six; who got very well thro' the Diſtemper; but about a Week after was ſeized with a very great Coldneſs and Shivering, which were followed by a burning Heat, with a great Pain and Diſorder in her Head, which continued for ſeveral Hours. She had ſome time ago an intermitting Fever, which I took this to be a Paroxyſm of, and expected its return ; bnt ſhe felt no more of it, and has ever ſince continued in good Health.

Of her two Sons, who were both inoculated at the ſame Time, the Younger got thro' the Diſtemper with a great deal of eaſe, the *Small Pox* being few, and the Symptoms very ſlight; but the Elder a Boy about five Years old, fared quite otherwiſe. The Symptoms before the Eruption were more than uſually ſevere, eſpecially the Vomiting; the Puſtules appear'd at the uſual Time, but more numerous than ordinary, and when the Eruption was finiſh'd, the Fever did not ceaſe, as it has done in every inſtance but this: on the fifth Day after he was ſeized, the ſwelling of his Face began, which was follow'd by a Pain and Swelling in his Throat, and a Salivation, which continuing till the eleventh Day, were ſucceeded by a

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* Mrs. *Breara* of *Eland.*

Swelling in his Hands and Feet, the uſual Symptoms of the diſtinct Sort when they are very full; and tho' there appear'd ſome little Signs of Malignity, yet with the Uſe of Bliſters and the milder Cordials and Alexipharmicks, the Puſtules roſe very large, and all Things went on very well, ſo that he got thro' the Diſtemper without any Danger, but with much more Pain and Trouble than any of the reſt have endured. After the *Small Pox* were gone off, we found a hard Swelling upon his Shoulder, which diſabled him for ſome time from moving his Arm; but by the Uſe of ſome common Applications, that is entirely gone. In this Family the only Child they had before theſe, died of the *Small Pox*, of a very malignant ſort, and this Boy was of an ill habit, and has had many dangerous illneſſes.

* The Twenty Third, was a Girl about Nine Months old; in this Child, after ſhe was well of the *Small Pox*, the Mother diſcover'd that one of her Thighs was a little ſwell'd, which was painful to her for ſome time, and made her unwilling to move that part; there were alſo ſome ſmall Tumours in the Groin; but theſe went off in a few Days, there only remaining a hardneſs about the Knee, which alſo diſappear'd in a ſhort Time without coming to Suppuration, and the Child is now in perfect Health.

† The Thirty Seventh was a Girl ſix Years of Age; ſhe got very eaſily thro' the Diſtemper; but before the *Small Pox* were gone, we found a ſmall Tumor upon the Muſcles of the Loins, which ripen'd very ſpeedily, and was open'd and heal'd up in a very ſhort time. Her Siſter, a young Woman about Eigh-

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* Mr. *Js. Hanſon*'s Daughter.
† Mr. *John Haigh*'s Daughters.

teen had alſo a Swelling of the ſame Kind in her Leg, but it lay ſomething deeper, and gave her a great deal of Pain for Three or Four Days; afterwards it diſcharged a great quantity of Matter, and was heal'd without any farther Trouble.

All the reſt, excepting theſe I have mentioned, got very well thro' the Diſtemper without any manner of Trouble, or Hazard, or any ill Conſequence afterward. Whether thoſe ſlight Indiſpoſitions, which ſome have been ſubject to afterward, were owing to the Inſition, I have not been able to judge; but I preſume what they have endured in the Courſe of the Diſtemper, and what has followed after, is not to be put in the Ballance with what is undergone in the common way, by thoſe who are thought to come off very well; and if this Method were more generally practiſed, 'tis probable ſome means wou'd be found out, to prevent even theſe ſubſequent diſorders, which are no more frequent, nor near ſo bad, as thoſe which follow the natural Sort.

In two Inſtances the Inoculation had no effect, the Reaſon of which, in one, * was becauſe the Child had the *Small Pox* before, as the Parents did believe; but the Diſtemper had been ſo favourable, as to leave it doubtful. † In the other, the Matter was taken, when the Puſtules were wither'd, and almoſt gone, and that little moiſture which they contain'd, I ſuppoſe, had loſt its Virtue; the Boy to whom it was made uſe of, was no way affected; the Places of Inciſion did not at all inflame, or ſwell, as uſual, nor did any Puſtules appear; but about a Month after, he was ſeized with

the

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* *Mich. Bland*'s Daughter.

† *William Clark*'s Son.

the Diſtemper in the ordinary way, and did very well

Some of thoſe who have been inoculated, that are grown up, have afterwards attended others in the *Small Pox*, and it has often happen'd, that in Families where ſome Children have been inoculated, others have been afterwards ſeized in the natural way and they have lain together in the ſame Bed all the Time; but we have not yet found, that ever any had the Diſtemper twice; neither is there any Reaſon to ſuppoſe it poſſible, there being no difference that can be obſerved, betwixt the Natural and Artificial Sort, (if we may be allow'd to call them ſo) but only that in the latter the Puſtules are commonly fewer in Number, and all the reſt of the Symptoms are in the ſame proportion more favourable. There is one Obſervation which I have made, tho' I wou'd not yet lay any great ſtreſs upon it, that in Families where any have been inoculated, thoſe who have been afterwards ſeized, never had an ill ſort of *Small Pox*, but always recovered very well.

Thus, Sir, I have given you a ſhort and plain Account of what has occurr'd to me concerning this Method of Inoculation: 'tis not any pains or care I have taken in writing this Letter, that can recommend it, for I have been ſolicitous about nothing but Truth, my deſign being only to give you a ſhort Hiſtory of the Diſtemper raiſed by Inoculation, ſo far as I have obſerved it. The Number might perhaps have been greater, if I wou'd have preſs'd it; but I only took ſuch as deſired it of themſelves, being cautious of perſwading any Body to it, becauſe I had but little Authority hereabouts to ſupport me; tho' I ought to acknowledge the kindneſs of many of my Friends, who being convinced, that this Method would be of ſome uſe, were very zealous to promote it; and in particular that very learned and worthy Gentleman, Dr. *Richardſon*, did upon all Occaſions vindicate this Practice. I know not whether I ſhall have an opportunity of doing much more this way at preſent, the *Small Pox* being in a great meaſure gone from this Town and Countrey; but I have already ſeen ſo much of it, that I ſhou'd never be afraid of its ſucceſs.

There is only one Thing more I am obliged to mention, which I wou'd rather have paſs'd over in ſilence, and that is the vigorous oppoſition it has met with from many honeſt well meaning Perſons, who cou'd not but fancy, that it is an unlawful and unwarrantable Practice. They have gain'd a great Majority on their ſide here, at well as in other Places, where it has been practiſed. I only wiſh, that, as they act upon a principle of Conſcience, they wou'd have been leſs buſy in raiſing, or ſpreading falſe and groundleſs Reports, whereby this Matter has been very much miſrepreſented, and many, entertaining a wrong Notion of it, have been deterr'd from making uſe of this Method for themſelves or their Children, who have ſince been unhappily taken off by the *Small Pox*. But when this Affair is ſet in a true light, and found to be always ſafe and effectual, I believe all the Objections raiſed againſt it will fall of courſe. It wou'd be of the greateſt moment that the World ſhou'd know what you think of it in Town, and how you have found it to ſucceed; 'tis commonly objected here, that it is not approved of in *London*; but if thoſe Gentlemen, who have juſtly gain'd the greateſt Honour and Reputation in our Profeſſion, ſhou'd by finding it ſucceſsful ſee cauſe to declare publickly in its favour, that wou'd be the greateſt means to forward it in the Country, and to remove thoſe unreaſonable prejudices, which do generally prevail againſt a Method, which I believe has no where been put in practice with any other Aim, than to do ſome ſervice to Mankind: for which Reaſon I doubt not but this ſhort Narrative will be acceptable unto you from, *&c.*

Thomas Nettleton.

Haliſax, Apr. 3.
1722.

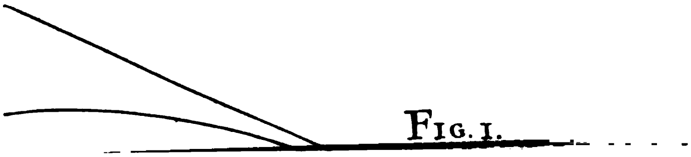

Fig. 1.

[illegible]

VIII. *A Letter from the ſame Learned and Ingenious Gentleman, concerning his farther Progreſs in inoculating the* Small Pox: *To Dr.* Jurin R. S. Secr.

IN Anſwer to what you require from me, as to what has been farther done, I have only to add that ſince I writ to Dr. *Whitaker*, I have made the Inſition upon about fifteen Perſons, who have all had the Diſtemper very favourably, and got thro' it with a great deal of eaſe. As nothing uncommon or extraordinary did happen in any of theſe Caſes, it will not be neceſſary to trouble you with a particular Account of any of them: They were moſt of them at ſome diſtance, the *Small Pox* being in a great meaſure gone from this Town and Neighbourhood.

I am very ſenſible of the Favour done me by the Royal Society, who were pleaſed to take notice of my Letter to Dr. *Whitaker*, which you had nothing to move you to beſides a generous Diſpoſition to encourage the ſmalleſt attempts towards any Thing, that may tend to publick Advantage. I muſt own that all the Information I had concerning this Affair, which I have happen'd to be engaged in, was entirely from the *Philoſophical Tranſactions*. 'Tis now about ſix Years ſince the Royal Society did communicate to the World ſome Letters from two very conſiderable Phyſicians reſiding in *Turkey*, whoſe good Senſe or Integrity we had no Reaſon to call in queſtion; theſe Gentlemen did ſolemnly aſſure us, that the Method of Inoculation had been for many Years practiſed in thoſe parts of the World, with almoſt conſtant ſucceſs. I had, as well as all others who have been engaged in the Practice, with ſufficient Sorrow and concern, been called to many in the *Small Pox*, whoſe Caſes were ſo deplorable, as to admit of no relief. And therefore I cou'd not but be very thoughtful about this Method, which promiſed to carry Perſons thro' that cruel Diſtemper, with ſo much eaſe and ſafety. I was ſo far from knowing that it was a Crime, that I always thought it the Duty of our Profeſſion, to do whatever we could to preſerve the Lives of thoſe, who commit themſelves to our Care; And I knew no Reaſon, why we ought not, with all humble thankfulneſs to Almighty God, to make uſe of any means, which his good providence ſhall bring to light conducing to that End. This Matter, tho' of ſo great Importance, lying dormant ſo long after it was known, is, I preſume a ſufficient proof, that none have been very forward to try Experiments. But when we had the Account in the Publick Papers, that it had by their Royal Highneſs's Command been done with ſucceſs at *London*, I cou'd not be ſatisfied without trying it here. I was ſoon convinced, that it would be of very great uſe; and the more experience I have had of it ſince, the more I am confirm'd in the ſame opinion I believe all others, who have ſeen any thing of this Practice, are in the ſame ſentiment, and there is no doubt, but in a few Years the World will acknowledge the ſervice, which the Royal Society have done to Mankind, in firſt revealing to this part of *Europe* a Thing ſo beneficial as it will certainly prove; for tho' ſome few unfortunate accidents may ſometimes happen, yet thoſe will be very rare in compariſon of the many ſad and diſaſtrous

Events, which this Diſtemper has been, and ever will be very fruitful of, while it is left to rage in its full force and violence.

Sir, I doubt not but when you have collected a ſufficient Number of Obſervations for it, you will be able to demonſtrate, That the Hazard in this Method is very inconſiderable, in proportion to that in the ordinary way by accidental Contagion, ſo ſmall, that it ought not to deter any Body from making uſe of it. In order to ſatisfy my ſelf, what Proportion the Number of thoſe that die of the *Small Pox*, might bear to the whole Number that is ſeized with the Diſtemper, in the natural way, I have made ſome enquiry hereabouts, and I ſhall take the freedom to tranſmit the Accounts to you, becauſe I believe you may depend upon their being taken with ſufficient care and impartiality. In *Hallifax*, ſince the beginning of laſt Winter, 276 have had the *Small Pox*, and out of that Number 43 have died. In *Rochdale*, a ſmall Neighbouring Market Town, 177 have had the Diſtemper, and 38 have died. In *Leeds* 792 have had the *Small Pox*, and 189 have died. It is to be noted, that in this Town, the *Small Pox* have been more favourable this Seaſon than uſual, and in *Leeds* they have been more than uſually mortal; but upon a *Medium* in theſe three Towns, there have died nearly 22 out of every hundred, which, is above a *fifth Part*, of all that have been infected in the natural way. I have in theſe Accounts confined my ſelf to the Limits of the Towns. The Numbers that have had the *Small Pox* in the Country round about, is vaſtly greater; but the Proportion of thoſe that die is much the ſame. I have made the enquiry in ſeveral Country Villages hereabouts, in ſome I found the Proportion to be greater, in others leſs, but in the main it is nearly the ſame. I am, *&c.*

Thomas Nettleton.

Halifax
June 16. 1722.

F I N I S.

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PHILOSOPHICAL TRANSACTIONS.

For the Months of *April* and *May*, 1722.

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I. *Obſervations of the Variation on board the* Royal African *Pacquet, in* 1721. *By Capt.* Cornwall.

N. B. The Meridional Diſtance is reckon'd from St. *Jago*.

Month and Year.	Latitudes.	Meridian Diſtance.	Longitud.	Variation.
Auguſt 24th 1721.	9° 8′ Sou.	9° 23′ W.	9° 25′ W	2° 13′ E
Ditto 26	11° 12′ S.	10° 46′ W	10° 50′ W	4° 30′ E
Ditto 27	11° 34′ S.	11° 28′ W	11° 41′ W	4° 29′ E
Ditto 28	12° 32′ S	11° 31′ W	11° 43′ W	4° 27′ E
Ditto 31	15° 46′ S	10° 53′ W	11° 6′ W	6° 10′ E
Septemb. 2*d.*	16° 26′ S	8° 25′ W	8° 30′ W	7° 16′ E
Ditto 5th	18° 45′ S	9° 31′ W	9° 39′ W	6° 17′ E
Ditto 6	19° 47′ S	9° 10′ W	10° 0′ W	8° 6′ E
Ditto 17	28° 43′ S	1° 7′ W	1° 9′ E	5° 53′ E
Ditto 22	31° 33′ S	3° 41′ E	3° 56′ E	4° 10′ E
Ditto 27	33° 30′ S	11° 29′ E	12° 57′ E	0° 11′ W
Ditto 30	32° 40′ S	19° 6′ E	12° 1′ E	3° 0′ W
October 1*ſt*,	32° 53′ S	21° 18′ E	24° 59′ E	5° 41′ W
Ditto 3	32° 30′ S	25° 33′ E	30° 0′ E	7° 47′ W
Ditto 5	32° 28′ S	30° 37′ E	35° 52′ E	8° 44′ W
Ditto 6	31° 22′ S	31° 40′ E	37° 7′ E	10° 57′ W
Ditto 7	31° 11′ S	32° 4′ E	37° 47′ E	11° 20′ W

Obſer-

Obſervations on the Coaſt of Africa.

Month and Year.	Latitudes.	Meridian Diſtance.	Longitud.	Variation.
October 13*th*, 1721.	26' 17' S.	35' 35' E	41° 41' E	14° 30' W
Ditto 19	19 41' S			12° 22' W
Ditto 21	17° 4' S			14° 29' W
Ditto 25	13° 56' S			14° 48' W
November 4*th*,	10' 57' S			13° 11' W
Ditto 7	8' 19' S			15° 14' W
Ditto 29	5° 0' S	in *Cabenda-Bay*		14° 33' W
From *Cabenda* to *London*, Merid. Diſtance from thence.				
December 9*th*,	3' 25' S	11° 30' W	11° 43' W	11° 32' W
Ditto 14	3° 30' S	21° 18' W	21° 24' W	
Ditto 20	0° 30' S	30° 41' W	30° 46' W	1° 5' W
January 1 172$\frac{1}{2}$.	10' 50' N	39' 8' W	39° 16' W	1° 1' E
Ditto 6	17° 15' N	43° 21' W	43° 29' W	1° 41' E

II. *A*

II. *A Letter to Dr.* Mead, *Coll. Med.* Lond. *& Soc. Reg. S. concerning an Experiment, whereby it has been attempted to ſhew the falſity of the common Opinion, in relation to the force of Bodies in Motion. By* Henry Pemberton, *M. D.* R. S. S.

S I R,

PEruſing the Learned *Polenus*'s Tract *de Caſtellis,* you were pleaſed to ſend me; I have found in it ſeveral curious Experiments, among which I reckon that of letting Globes of equal Magnitude, but of different weights, fall upon a yielding Subſtance, as Tallow, Wax, Clay or the like, from heights reciprocally proportional to the weights of the Globes. This Experiment engaged in particular my Attention, as it is brought with deſign to overturn one of the firſt Principles eſtabliſh'd in Natural Philoſophy. And the Knowledge I have of your great Eſteem for that part of Science, emboldens me to trouble you with my Thoughts upon this Experiment; for I cannot by any means admit of the Deduction that is drawn from thence, that becauſe the Globes make in this Experiment equal Impreſſions in the yielding Subſtance, therefore they ſtrike upon it with equal force: whereby it is attempted to prove the Aſſertion of Mr. *Leibnitz,* that the force of the ſame Body in deſcending is proportional to the height from whence it falls; or, in all Motion, proportional to the Square of the Velocity, and not proportional to the Velocity it ſelf, as is commonly thought. On the contrary, I think this very Experiment proves the great unreaſonableneſs of Mr. *Leibnitz*'s Notion.

I am ſurpriz'd, that ſo careful a Writer as *Polenus* appears to be, from the accuracy wherewith he delivers his Experiments, ſhould not rather ſuſpect his reaſoning in an intricate Caſe, than thus contradict a Principle in Philoſophy, that has been directly prov'd by a multitude of Experiments, in particular by thoſe Sir *Iſaac Newton* recommends for that purpoſe *(a)* ; and that is moreover abundantly eſtabliſh'd by its exact Agreement with all Obſervations; as being the Principle upon which all appearances, hitherto obſerved in the motion of Bodies, are accounted for by juſt and undeniable Arguments; and we ſhall find on Inquiry, that the preſent Caſe comes alſo under the ſame Rule.

As the uſe of Experiments in Natural Philoſophy is to diſcover the Cauſes of Things, by exhibiting more ſimple Effects of thoſe Cauſes, than occur in the ordinary courſe of Nature; ſo for this end it is neceſſary, that our Argumentation upon Experiments be perfectly juſt, otherwiſe they may lead us into Errors. The firſt thing neceſſary for making right Deductions from an Experiment, is to determine the proper uſe thereof; which I think in this before us is not rightly underſtood. Certainly this Experiment of *Polenus* is much more fit to inform us of the Law, by which theſe yielding Subſtances reſiſt the motion of Bodies ſtriking upon them, than to ſhew the forces, with which Bodies ſtrike ; for whatever thoſe forces be, the Effects muſt be very different, according to the Difference there may be in the Rule obſerved by ſuch reſiſtance.

(*a*) Princip. Philoſ. Nat. *p.* 19.

Now this Experiment ſhews, that if two Globes in Motion bear againſt equal Portions of the yielding Subſtance, the Oppoſition, that Subſtance makes to the Motion of the Globes, will be the ſame in both, however different the Velocities be, with which they move. This I ſhall demonſtrate as follows.

Let *A* and *B* be two Globes, equal in Magnitude, but of different Weights, which are equally immerſed into a yielding Subſtance. Suppoſe the Velocities, with which they move in their preſent Situation, to be reciprocally in the Subduplicate *ratio* of the Weights of the Globes; that is, let the *ratio* of the Weight of the Globe *A* to the Weight of the Globe *B*, be Duplicate of the *ratio* of the Velocity of the Globe *B*, to the Velocity of the Globe *A*. Since therefore the *ratio* of the quantity of Motion in the Globe *A*, or of the force with which it moves, to the quantity of Motion in the Globe *B*, or to the force with which that Globe moves, is compounded of the *ratio* of the Weight of the Globe *A*, to the Weight of the Globe *B*, and of the *ratio* of the Velocity of the Globe *A*, to the Velocity of the other Globe *B*, the force, with which the Globe *A* moves, is to the force, with which the Globe *B* moves, as the Velocity of this Globe *B*, to the Velocity of the other Globe *A*. But if the ſame Oppoſition be made to the Motion of the Globes, when they bear upon equal Portions of the yielding Subſtance, the Effect of that Oppoſition, while the Globes enter farther into the Subſtance by equal Spaces, will be proportional to the time, in which the Globes are moving thoſe Spaces, or in which the Oppoſition is made, if we conſider thoſe Spaces while naſcent or in their firſt Origine; the Effect therefore of this Oppoſition will be reciprocally proportional to the Velocity of each Globe: namely, the momentaneous loſs of force in the Globe

A will be to the momentaneous loſs of force in the Globe *B*, as the Velocity of the Globe *B*, to the Velocity of the Globe *A*; and the whole force of the Globe *A* has been found to bear the ſame *ratio* to the whole force of the Globe *B*; conſequently theſe Globes, while they penetrate equal Spaces into the Subſtance, loſe parts of their force, which bear the ſame proportion to the whole: and therefore, if their Velocities be at any time reciprocally in the Subduplicate *ratio* of their Weights, ſo that the forces or degrees of Motion, with which they move, be reciprocally proportional to their Velocities, the forces, with which they preſs into the yielding Subſtance, at equal Indentures made in the Subſtance, will continue in the ſame Proportion; and therefore upon the Theory of Reſiſtance here ſuppoſed, when the whole Force and Motion of both theſe Globes is entirely loſt, they will be plunged into the Subſtance at equal Depths.

Now whereas in the Experiment of *Polenus*, the Globes, falling from Heights reciprocally proportional to their Weights, ſtrike upon the yielding Subſtance with Velocities reciprocally in the Subduplicate proportion of their Weights, and the Effect is in all Caſes found to be, what is here deduced from the Theory of Reſiſtance, I have propoſed; it is a ſufficient Confirmation of the Truth of this Theory.

Only here, Sir, I ought to obſerve to you, That I have ſuppoſed the Globes to be ſtopt by the whole Reſiſtance of the Subſtance, they move againſt; although in ſtrictneſs they are ſtopt only by the exceſs of that Reſiſtance above the Action of Gravity upon them. But I have neglected the Conſideration of the Action of Gravity, that being but ſmall in Proportion to the Reſiſtance, as will appear from the Globes being much more ſpeedily ſtopt by this Reſiſtance, than they would be by the Action of Gravity, if its force were applied upwards; for by that force alone, the Globes would not be ſtopt, till they had meaſured Spaces equal to the heights above the reſiſting Subſtance, from whence they fell; which heights bear a great Proportion to the depths, the Globes in this Experiment are immerſed into the yielding Subſtance, as I have found upon trial.

Thus, if I miſtake not, may be removed the Difficulty attending this Experiment. But as Mr. *Leibnitz*'s Opinion is deduced from it by Means of this Axiom, that Effects are proportional to their Cauſes; ſo that here the Effects being thought the ſame, the Cauſes are concluded to be ſo likewiſe; it will not be amiſs to mention in this place an Experiment, where this Axiom may be more juſtly applied, than it can be in our preſent caſe, from which Experiment the received Opinion may be proved. This Experiment is mentioned by *Polenus* from *Merſennus*, though tried by him ſomewhat rudely; but has been often ſince made in the following manner. To one end of the Beam of a Ballance is hung a Weight, and from a proper height is let fall upon the other extremity of the Beam another Weight, which by ſtriking thereon ſhall raiſe the end, to which the Weight is ſuſpended, to ſuch a height, as is juſt ſufficient to ſet free a certain Spring. If then a different Weight be hung in the room of the former; the height, from whence the falling Weight muſt deſcend, in order to raiſe the Extremity of the Ballance, to which this other Weight is ſuſpended, to the ſame height as before, that is, high enough to ſet the forementioned Spring at liberty, is found to be ſuch, that the Velocity, with which the falling Weight ſtrikes upon the Ballance, in this latter Caſe, will be to its former Velocity, as the latter Weight to the former, (a) except only that, agreeably to what *Merſennus* himſelf remarked, when the Weight deſcends from great heights, an Altitude ſomewhat greater than this Rule implies is required to raiſe the other Weight as much as is deſired. But whether the bending of the Arm of the Ballance, when acted upon with a great force, or whether any increaſe of Friction in this Caſe, occaſion the Irregularity here mentioned, we need not ſtrictly enquire; for this Irregularity is yet leſs reconcileable with the new Opinion, than the regular Effects of the Experiment. Hence therefore we may ſee, that the very Method of reaſoning, which being applied erroneouſly, is ſuppoſed to prove Mr. *Leibnitz*'s Sentiment concerning the force of Bodies in Motion, will, when juſtly uſed, confirm the other Opinion in relation to that Matter.

But as I have aſſerted in the beginning of this Letter, that the very Experiment of *Polenus* is not only reconcileable to the common Doctrine of Motion, as I have now demonſtrated; but even that it does it ſelf make manifeſt the great unreaſonableneſs, if not the abſolute Abſurdity, of Mr. *Leibnitz*'s Opinion; it remains that I briefly make proof of this.

If two Globes *A* and *B*, of equal Magnitude but of different Weights, ſtriking on a yielding Subſtance with equal force, in every Caſe loſe all their Motion at equal Depths, it is neceſſary that at all times, during their Motion, they loſe equal Degrees of force, when they bear upon equal Portions of the Subſtance, in entring equal Spaces into the Subſtance. This will be eaſily ſeen from what has before been ſaid. Now whereas Mr. *Leibnitz* ſuppoſes the power of Gravity to give to the ſame falling Body Degrees of force proportional to the height from whence it falls; according to his Opinion, by the power of Gravity, equal Degrees of force are added in the deſcent of the ſame Body through equal Spaces; and in different Bodies deſcending through equal Spaces, the Degrees of force added will be as the quantity of Matter, or as the weight of each Body. Therefore while the Globes *A* and *B* penetrate equal naſcent Spaces into the yielding Subſtance, by the Action of Gravity, were not that Action overcome by the Reſiſtance of that Subſtance, additional Degrees of force would be communicated in ſuch proportion, that the force added to the Globe *A*, would be to the force added to the Globe *B*, as the weight of the Globe *A*, to the weight of the Globe *B*, or in the Duplicate *ratio* of the Velocity of the Globe *B*, to the Velocity of the Globe *A*. But ſince the Globes loſe the ſame Degrees of force in entring equal Naſcent Spaces into the yielding Subſtance, the Effect of the Oppoſition made by this Subſtance to the Motion of the Globes, during the time of their paſſing through ſuch Naſcent Spaces, will be both the taking from them that ſame Degree of force, and moreover the additional force, which would otherwiſe have been given them by their own Gravity. But farther, the Oppoſition made to the motion of the Globe *A*, to the Oppoſition made to the motion of the Globe *B*, will be in the *ratio* compounded of the *ratio* of the Effect of the Oppoſition, the Subſtance makes to the motion of the Globe *A*, to the Effect of the Oppoſition, the Subſtance makes to the motion of the Globe *B*, and of the *ratio* of the Time, in which the Oppoſition is made againſt the latter Globe, to the Time in which it is made againſt the former; which latter *ratio* is the ſame with the *ratio* of the Velocity of the Globe *A*, to the Velocity of the Globe *B*. But ſince it is ſhewn, that the Effect of the Oppoſition made by the yielding Subſtance to theſe Globes is two-fold, and that one part of the Effect of the Oppoſition made to the motion of the Globe *A*, is equal to one part of the Effect of the Oppoſition made to the motion of the Globe *B*; and that another part of the Effect of the Oppoſition made to the motion of the Globe *A*, to another part of the Effect of the Oppoſition made to the motion of the Globe *B*, is in the Duplicate *ratio* of the Velocity of the Globe *B*, to the Velocity of the Globe *A*. One part of the Oppoſition it ſelf made to the motion of the Globe *A*, will be to one part of the Oppoſition againſt the motion of the Globe *B*, as the Velocity of the Globe *A*. to the Velocity of the Globe *B*, and another part of the Oppoſition to the motion of the Globe *A*, to another part of the Oppoſition to the motion of the Globe *B*, will be as the Velocity of the Globe *B*, to the Velocity of the Globe *A*. So that when the Globes bear upon equal Portions of the yielding Subſtance, the Oppoſition to their Motion will be in part as the Velocity of the Globes, and in part reciprocally as their Velocity. Hence, becauſe the reſiſting Subſtance is of an uniform Texture, the Oppoſition to the Motion of either of the Globes in its preſent Situation, and when moving with its preſent Velocity, will be to the Oppoſition it would meet with in the ſame Situation, if it moved with any other Velocity, partly as the preſent Velocity to that other Velocity, and partly as that other Velocity to the preſent. But by that part of the Oppoſition made againſt the motion of the Globe, which is directly as the Velocity, the Globe can never be wholly ſtopt; for upon the ſtopping of the Globe, that part of the Oppoſition

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(a) *Vid.* 's Graveſande Phyſic. Elem. Tom. I. p. 59.

tion to its Motion will likewiſe totally ceaſe, and conſequently the Globe's Weight will carry it further down, unleſs the other part of the Oppoſition againſt its Motion prevent it. But I ſay again, neither can this latter part of the Oppoſition made to its Motion be ever great enough to ſtop the Globe; for the Degree of this Oppoſition being reciprocally as the Velocity of the Globe, when the motion of the Globe is wholly taken away, it will become infinitely greater, than at any time, while the Globe is in Motion; ſo that when the Globe ſhould be ſtopt by this part of the Oppoſition made to its Motion, the Oppoſition to the Globe's Motion will become infinitely great; inſomuch, that no Degree of force whatever could be able to impel the Globe further into the Subſtance, but this can never come to paſs. Beſides, it is not neceſſary to apply any ſuch refined Argument againſt this part of the Reſiſtance; it would be alone ſufficient to conſider, how unreaſonable a Suppoſition it is, that a Reſiſtance ſhould increaſe, when the Velocity of the reſiſted Body decreaſes.

Thus may this Experiment be made uſe of to invalidate that very Opinion it is brought to ſupport. But another uſe may likewiſe be made thereof: For it will ſerve to illuſtrate what the great Sir *Iſaac Newton* has more than once hinted, that the Reſiſtance of Fluids, which ariſes from the Tenacity of their Parts, decreaſes in a leſs Proportion than the Velocity of the reſiſted Body decreaſes (a); for as this Reſiſtance bears a great Analogy to the Reſiſtance of the yielding Subſtances we have been here treating of, ſo we have found, that the Reſiſtance of theſe Subſtances does not much depend upon the Velocity of the Body, againſt which the Reſiſtance is applied.

(a) *Vid.* Philoſ. Nat. Princip. Math. Prop. 52. lib. 2. in Schol. Opticks. Qu. 28. p. 339, 340.

And thus, Sir, we may ſee how all Experience conſpires in confirming and ſetting forth that ſtupendious force of reaſoning, which has enabled our great Philoſopher moſt ſurprizingly to ſearch out, and diſtinguiſh the Springs of Natural Operations; a Work infinitely more difficult to accompliſh than even the great Improvements he has made in pure Mathematicks, which were previouſly neceſſary in order to his ſucceeding in his Searches after the Knowledge of Nature; for in this laſt Purſuit he has given Proof, not only of a more unbounded Invention, than is required in the ſubtileſt Geometrical Speculations; but has alſo there diſcovered the greateſt Diſcernment, and moſt conſummate Judgment; ſince in his Philoſophical Writings, he has never been once impoſed on by an Hypotheſis, nor by any other of the various Fallacies, which my Lord *Bacon* in his *Novum Organon*, has reckon'd up as the Cauſes, that had hindred the Improvement of the true Philoſophy.

But here I ſhall put an end to this long Letter; for the Freedom of which, I know, I need make no Apology to you, Sir, of whoſe great Candour I have for ſome Years paſt been a conſtant Witneſs; and as I have frequently admired, how amidſt the vaſt Employments of your Profeſſion, you ſhould find time to purſue with ſo great Succeſs ſuch various ſorts of Learning; ſo I have as often been delighted to obſerve with what Benignity you receive thoſe, who have made any the leaſt part of uſeful Knowledge their Study.

I am, Sir, &c.

H. P.

POST-SCRIPT.

ABout a Week after I had ſent you the Letter, containing my Obſervations on *Polenus*'s Experiment, I had the good Fortune to hear an excellent and learned Friend of yours, to whom you had been pleaſed to ſhew my Letter, give a very curious and weighty Argument to confirm Sir *Iſaac Newton*'s Sentiment in relation to the Reſiſtance of Fluids, which I had deduced from the above mentioned Experiment; and as this very much pleaſed me, I ſhall here endeavour to ſend you an Account of it in the following manner:

Suppoſe pieces of fine Silk, or the like thin Subſtance, extended in Parallel Planes, and fixt at ſmall Diſtances from each other. Suppoſe then a Globe to ſtrike perpendicularly againſt the middle of the outermoſt of the Silks, and by breaking through them to loſe part of its Motion. If the pieces of Silk be of equal Strength, the ſame Degree of force will be required to break each of them; but the Time, in which each piece of Silk reſiſts, will be ſo much ſhorter as the Globe is ſwifter; and the loſs of Motion in the Globe conſequent upon its breaking through each Silk, and ſurmounting the Reſiſtance thereof, will be proportional to the Time, in which the Silk oppoſes itſelf to the Globe's Motion; inſomuch that the Globe by the Reſiſtance of any one piece of Silk will loſe ſo much leſs of its Motion as it is ſwifter. But on the other Hand, by how much ſwifter the Globe moves, ſo many more of the Silks it will break through in a given Space of Time; whence the number of the Silks, which oppoſe themſelves to the Motion of the Globe in a given Time, being reciprocally proportional to the Effect of each Silk upon the Globe, the Reſiſtance made to the Globe by theſe Silks, or the loſs of Motion, the Globe undergoes by them in a given Time, will be always the ſame.

Now if the Tenacity of the Parts of Fluids obſerves the ſame Rule as the Coheſion of the Parts of theſe Silks; namely, That a certain Degree of force is required to ſeparate and diſunite the adhering Particles, the Reſiſtance ariſing from the Tenacity of Fluids muſt obſerve the ſame Rule as the Reſiſtance of the Silks, and therefore in a given Time the loſs of Motion, a Body undergoes in a Fluid by the Tenacity of its Parts, will in all Degrees of Velocity be the ſame; or in fewer Words, that part of the Reſiſtance of Fluids, which ariſes from the Coheſion of their Parts, will be Uniform.

III. *An Account of the Falls of the River* Niagara, *taken at* Albany, Octob. 10. 1721. *from Monſieur* Boraſſaw, *a* French *Native of* Canada. *By the Honourable* Paul Dudley, *Eſq*; F. R. S.

THE Falls of *Niagara* are a mighty Ledge or Precipice of ſolid Rock, that lies acroſs the whole breadth of the River (a little before it empties it ſelf into, or forms the Lake *Ontario*) and very ſteep.

Monſieur *Boraſſaw* never meaſur'd the Falls himſelf, though he has been there at ſeven different Times: But what he ſays is, That,

This laſt Spring the Governour of *Canada*, Monſieur *Vaudreil*, ordered his own Son, with three other Officers, *viz.* Meſſieurs *Longue Iſle*, St. *Ville*, and *Laubineau* to Survey *Niagara*, and take the exact height of the Cataract, which they accordingly did with a Stone of half an hundred Weight, and a large Cod-line, and found it upon a Perpendicular no more than twenty ſix Fathom; his Words were *Vingt & Six Bras.*

This differs very much from the Account Father *Hennepin* has given the World of that Cataract, for he makes it an hundred Fathom; and our Modern Maps from him, as I ſuppoſe, mark it at ſix hundred Feet; but I believe *Hennepin* never meaſured it, and there is no gueſſing at ſuch Things.

When I objected *Hennepin*'s Account of thoſe Falls, to Monſieur *Boraſſaw*, he replied; That accordingly every Body had depended upon it as right until the late Survey. Upon further Diſcourſe he acknowledged, That below the Cataract for a great way, there were numbers of ſmall Ledges, or Stairs croſs the River, that lower'd it ſtill more and more, till you come to a Level; ſo that if all the Deſcents be put together, he does not know but the Difference of the Water above the Falls, and the Level below, may come up to Father *Hennepin*, but the ſtrict and proper Cataract upon a perpendicular, is no more than twenty ſix Fathom, or an hundred and fifty ſix Foot, which yet is a prodigious Thing, and what the World I ſuppoſe cannot parallel, conſidering the greatneſs of the River, for it is near a Quarter of an *Engliſh* Mile broad, and very deep Water.

Several other Things Monſieur *Boraſſaw* ſet me right in, as to the Falls of *Niagara*. Particularly it has been ſaid, That the Cataract makes ſuch a prodigious noiſe, that People can't hear one another ſpeak, at ſome Miles diſtance, whereas he affirms, you may converſe together cloſe by.

I have alſo heard it poſitively aſſerted, That the Shoot of the River, when it comes to the Precipice, was with ſuch a mighty force, that Men and Horſe might march under the Body of the River without being wet. This alſo he utterly denies, and ſays the Water falls in a manner right down.

What he obſerv'd farther to me was,

That the Miſt or Shower (his Word was *La Brume*) which the Falls make, is ſo extraordinary, as to be ſeen at five Leagues diſtance, and riſes as high as the common Clouds. In this Brume or Cloud, when the Sun ſhines, you have always a glorious Rainbow.

Of the River it ſelf, which is there called the River *Niagara*, he tells me it is much narrower at the Falls, than either above or below, and that from below there is no coming nearer the Falls by Water, than about ſix *Engliſh* Miles, the Torrent is ſo rapid, and withal ſuch terrible Whirl-pools.

He confirms Father *Hennepin*'s and Mr. *Kellug*'s Account of the large Trouts of thoſe Lakes, and ſolemnly affirmed there was one taken lately, that weighed eighty ſix Pounds; which I am the rather inclined to believe upon the general Rule that Fiſh are according to their Waters. To confirm which, a very worthy Miniſter, now alive in *New England*, affirmed to me; That while he was a Priſoner at *Mon-real*, in *Canada* River, he ſaw a Pike brought up one Day from the River to the Governour's Houſe, and carried upon a Pole between two Men, that meaſured Five Foot, and Ten Inches long, and proportionably large.

I my ſelf this laſt Summer, ſaw a Cataract, three Leagues above *Albany*, in the Province of *New York*, upon *Schenectada* River called the *Cohoes*, which they count much of there; and yet that is not above 40 or 50 Foot perpendicular. From theſe Falls alſo there riſes a miſty Cloud, which deſcends like ſmall Rain, that, when the Sun ſhines, gives a handſome ſmall Rainbow that moves as you move, according to the Angle of Viſion. The River at the *Cohoes* is to 40 or 50 Rods broad, but then it is very ſhallow Water, for I was told that in a dry Time, the whole River runs in a Channel of not more than fifteen Foot wide.

In my Journey to *Albany*, 20 Miles to the Eaſtward of *Hudſon*'s River, near the middle of a long riſing Hill, I met with a brisk noiſy Brook ſufficient to ſerve a Water-Mill, and having obſerved nothing of it at the beginning of the Hill, I turned about and followed the Courſe of the Brook, till at length I found it come to an End, being Abſorb'd, and ſinking into the Ground, either paſſing through Subterraneous Paſſages, or ſoaked up with the Sand; and tho' it be common in other Parts of the World for Brooks and even Rivers thus to be loſt; yet this is the firſt of the Sort, I have heard of, or met with in this Country.

P. Dudley.

IV. *A Letter from Mr.* Leeuwenhoeck, *F. R. S. concerning the Muſcular Fibres in ſeveral Animals, and the Magnetick Quality acquired by Iron, upon ſtanding for a long time in the ſame Poſture.*

Delft, *Apr.* 21ſt, 1722.

To the Illuſtrious Royal Society.

Honoured Sirs,

IN a Letter I receiv'd ſometime ago from one of your Secretaries, among many Expreſſions of your kind Acceptance of my Labours I am requeſted by that

Gentleman in the Name of the *Royal Society*, to endeavour by repeated Obſervations to confirm ſome of my late Diſcoveries, and to ſet them in ſo clear a Light, as to ſtop the Mouths of the moſt incredulous Gainſayers.

With regard to this, I beg leave to obſerve to you, that I examine a great many different Subjects, of which I commit no account to Paper, becauſe the Reſult is the ſame with what I have already deſcribed; and that whenever I make any Diſcovery, which I apprehend will not eaſily meet with Credit, I ſuffer the Object to lie before the Microſcope Day after Day, and ſometimes for whole Years together, till it is eaten up by Inſects. This I do with deſign to let it be ſeen by as many different Perſons as poſſible.

It is not above eight Days ago, that I was viewing a Portion of the Fleſh of a fat Ox, as likewiſe the Muſcular Fibres of a Cod-Fiſh, and of a Pearch, which Fibres being cut tranſverſly, I could ſee in them very diſtinctly the great number of ſmall Veſſels, that ran along the length of each Fibre. And I have ſeen the ſame this Month of *April*, in the Muſcular Fibres taken from the hinder Leg of a Mouſe, and cut thro' tranſverſly.

I have at this time ſtanding before a Microſcope, ſome of the Muſcular Fibres of a fat Ox, with thoſe of a Mouſe lying beſide them, in order to have as many Eye-witneſſes as poſſible, of their being of the ſame ſize in theſe two Animals, and I uſe the ſame Method in ſuch other of my Obſervations, as are likely to appear incredible to other Perſons.

In ſpeaking formerly of the ſmall *Fibrillæ*, that help to ſuſpend the Teſticles of a Ram, I forgot to mention, that each of theſe conſiſts of exceeding ſmall Veſſels, which run parallel to its length.

I have likewiſe at this time ſtanding before a Microſcope, a ſmall Portion of the Bone of an Ox, in which may evidently be ſeen the Veſſels, which proceed from the Bone, and compoſe what is called the *Perioſteum*, as likewiſe the Openings of theſe Veſſels; the reaſon of whoſe appearing ſo clearly is, as I imagine, that they are filled with the Medullary Oil.

I take this Opportunity of informing you, that the Iron-Croſs, which is ſuppoſed to have ſtood upon the Steeple of the New-Church here about two hundred Years, having been lately taken down to be repair'd, I was inform'd by a certain Foreign Gentleman, that a piece of Iron, that has ſtood for a long time in one Situation, would thereby acquire a Magnetick Quality. Upon which I deſir'd a Workman to procure me a piece of that Croſs, who accordingly brought me a bit of it, of about a Span long, and a quarter of an Inch thick, which I apply'd both to a working Needle, and the Needle of the Compaſs, but without any Effect upon one or the other.

Some time after, the ſame Workman brought me ſome other pieces, looking like ruſty Iron, which he had broken off from the bottom of the Croſs, where it had been faſten'd by four croſs pieces bound down with Iron, to an erect piece of Timber nine Inches ſquare, and cover'd with Lead in ſuch a manner, that no wet could get to it.

This ſeeming ruſty Iron would take up ſeveral Needles hanging by one another, and appear'd to have a ſtronger Magnetick Virtue than two Loadſtones, which I had then in the Houſe; and was ſo hard, that no File would touch it. I gave one of the greateſt pieces to a Knifegrinder, to grind it for me, who was a long time about it, and complain'd that it was harder than Steel.

The Report of this made ſo great a Noiſe in the Town, that in a little time my pieces of Iron were all begg'd away, except one little one, which I kept for my ſelf.

I am, &c.

Antony van Leuwenhoek.

V. *An Account of the manner of bending Planks in His Majeſty's Yards at* Deptford, *&c. by a Sand-heat, invented by Captain* Cumberland. *By* Robert Cay, *Eſq*;

THe place, where the Planks lie to be ſoftned in the Stove, is between two Brick-Walls; of ſuch a length, height and diſtance from each other, as ſuffice to admit the largeſt, or to hold a good number of the ſmaller Sort: the bottom is of thick Iron Plates, ſupported by ſtrong Bars; under the middle of which, are two Fire-places, whoſe Flews carry the Flame towards the Ends.

The Planks are laid in Sand; the loweſt about ſix or eight Inches above the Iron-Plates, they are well cover'd with the Sand, and Boards laid over all, to keep in the Heat. The Sand is moiſtned with warm Water, (for which purpoſe they have a Cauldron adjoyning to the Stove) and if the Timber be large, and intended to be very much bent, ſo that it muſt lie long in the Stove, they water the Sand again, once in 8 or 10 Hours. When 'tis judged to be ſoft enough, the Sand is remov'd ; and the Workmen carry away their reſpective Planks, to the ſeveral Places, where they are to be us'd ; and having firſt nail'd a thin Board upon the out-ſide, to preſerve the Plank from Bruiſes, they fix one part in its proper place, and bring to the others, by any power they can moſt conveniently apply. This Work ſeems to be perform'd with wonderful Eaſe ; notwithſtanding ſome we ſaw were ſo knotty, that the Builders aſſur'd us, they cou'd not have brought them to that Curvature by the former Methods. Thoſe we ſaw put in between others, very exactly fitted the Spaces they had been cut for; and the Workmen told us, they had made no Allowance either for the ſwelling, or ſhrinking of the Wood.

This Method excells that of burning the Planks over an open Fire in ſeveral reſpects : particularly, that no part of the Wood is deſtroy'd, but remains of the ſame Dimenſions ; at leaſt very nearly ; a Plank of the breadth of 16 Inches being ſaid not to alter above $\frac{1}{10}$ part of an Inch. The Edges of the Plank are preſerv'd ; and conſequently the Work muſt be much firmer, and the Calking laſt longer. The extraordinary ſoftneſs of the Wood, while 'tis warm, makes it eaſily bend to any Figure neceſſary in Ship-building, which it holds very well, if they have occaſion to take it off again after it is cold whereas the Plank bent by burning, would ſtart when looſened ; and could only be fixed to the Timbers by ſuch a force, as was able to overcome the Reſiſtance occaſion'd by the Spring of the Plank. It likewiſe adapts it ſelf very readily to the Surface of the Timbers, if they happen to be uneven.

They ſhew'd us the Gun Deck-Clamps in a Ship of the Second Rate; which are very large Planks, bent and twiſted in ſo peculiar a manner, that they never could by any other Method, bend them into that Form, but uſed to cut them into Shape. The whole Operation is perform'd with much leſs trouble to the Carpenters, as well as at leſs Expence; and they hope the Wood will be more durable; as 'tis evident, from the deep Tincture the Sand receives, that a conſiderable quantity of Sap comes out of the Oak, while its in the Stove: and a large Plank was obſerv'd by the Workmen or Officers of the Yard, to weigh ſome Pounds leſs, when it was taken out.

A Plank five Inches thick requires five or ſix Hours to make it fit for bending; and the Time requiſite for others, ſeems to be in a Duplicate Propottion to their thickneſs.

Explanation of the Figures.

Fig. 1. Repreſents a Plank, in the Buttocks of a Second Rate Ship, whoſe length from *A*, to *C*. is three Feet, and thickneſs (*AF*) 4¼ Inches, the end *C*, of this Plank was bent 12 or 13 Inches from the ſtreight Line *AB*

Fig. 2. and 3. are Sections of the Stove.

A A. the Fire-places.

B. *B*. the Aſh-holes.

C. C. the Flews under the Iron bottom.

D. *D*. the two Chimneys.

E. the place for the Planks and Sand.

F. *F*. the two Brick-walls.

G. G. two inclin'd planes, for the Men to ſtand on, *&c.* when they put in, or take out Planks, or water the Sand.

h. h. The bottom of the Stove, made of Iron.

i. i. The Grates to lay the Fewel on.

VI. *A Letter from the Reverend Mr.* James Field, *Rector of St.* Johns *in* Antegoa, *concerning two Caſes of Wounds in the Stomach, to Mr.* John Douglas, *Surgeon,* F. R. S.

SIR,

THE Accidents of which you deſire me to give you an account, were as follows.

A luſty young Negro man returning home about Noon, went into his Houſe, where ſeeing ſome ripe Plantains, he eat of them heartily; his Father in Law, about 60 Years of age, coming to the ſame Houſe ſoon after, enquired of the young Fellow, who had taken his Plantains. Who anſwered, That he being hungry, had eaten them. Ay, you Dog, ſays the old Fellow, have you ſerved me ſo, I kept them for my own Dinner; and without more ado, took his Knife, and gave the young Fellow a moſt deſperate Wound in the upper Region of the Belly, a vaſt Gaſh being made in the Stomach, in ſo much that the Plantain, which he had eaten, burſt through the Wound, which was made ſtreight up and down.

The old Man immediately fled for it, the young Fellow's Companions hearing what was done, purſued the old Man with Bills in their Hands: the old Man, ſeeing them follow him in this manner, and get ground of him, and ſuſpecting their Deſign was to kill him, pull'd out the ſame Knife, with which he had ſtabb'd the other, and gave himſelf as deſperate a Wound, as he had given him, and in the upper Region of the Belly, his Stomach being likewiſe ſeen, only with this Difference, that this laſt Wound was tranſverſe, or from left to right, the firſt directly up and down; the old Fellow was carried home and laid in the ſame Houſe, where the other wounded Fellow lay.

This Accident happen'd about Noon, and Mr. *Forreſt*, the Surgeon came not to dreſs them till between four and five; he ſtitch'd up the Stomachs of them both entirely, and their Bellies too, only leaving in each a ſmall hole for Suppuration; a Fever ſeized each of them, the old Man was in moſt danger, the Fever held them about a Fortnight, the Wounds were brought to a good Digeſtion, and in a Months time or thereabouts, the young Fellow went abroad, but the old Man lay ſomething longer. They were both perfectly cured, and have been very well ever ſince, tho' its above fifteen Years ſince this Accident happen'd.

Windſor,
Aug. 24. 1720.

I am, &c.

Jam. Field.

VI. *A Letter from Mr.* Atkinſon, *Surgeon in* White-Chappel: *Concerning an Impoſthumation in the Stomach. To the ſame.*

SIR,

THE Caſe you deſire a particular account of, was as follows. I had a Patient about ſeven Years ago, which had a large Tumour on the upper part of her Belly, it was hard and painful, but did not alter the natural colour of the Skin, and had been three Months in coming, I applied a warm Gum Plaiſter to it, which in about a Fortnights time brought it to a Suppuration. I then applied a Cauſtick about the bigneſs of a Shilling; when the Eſchar fell off, I ſaw a ſolid kind of Subſtance appear in the Orifice, I laid hold of it with my *Forceps*, and pulled it gently towards me. upon which there thruſt forcibly out a quantity of it, that near fill'd my Hand, ſo I dreſt it. Next dreſſing, the ſame Subſtance appear'd again, which on her ſtraining, forced out near twice as much as before. I was at a great loſs to determine what this ſtuff was, but at laſt concluded it was the *Omentum* it ſelf, in which Opinion I was confirm'd by ſome other Surgeons I ſhew'd it to. I was ſtill in doubt, whether the Stomach was concerned in this Caſe or not, till the next Removal of the Dreſſings, at which there ſpurted out above half a Pint of Ale in a full Stream, which was part of a Pint ſhe had drunk a little while before. I now concluded the Caſe mortal, however, I ordered her to keep her Bed, to lie conſtantly upon her Back, and feed on things of eaſie

Digeſtion. The greateſt part of what ſhe eat or drank, came through the Ulcer for eight or ten Days, ſo that I had no hopes of ever curing it, yet contrary to my Expectation in about ſix Weeks ſhe was perfectly cured, and is now living and in good Health.

VIII. *An Account of the quantity of Reſin in the* Cortex Eleutheriæ. *By Mr.* John Brown, *Chymiſt,* F. R. S.

DR. *Douglas* having lately deliver'd in a Paper to be read before this Society, giving an account (from the Hiſtory of the Royal Academy at *Paris*) of the *Cortex Eleutheriæ*; and among other things having ſaid of it, that Monſieur *Boulduc* had from one Ounce of the Bark, by means of Spirit of Wine, gotten 5 Drachms of Reſinous Extract, there remaining 3 Drachms of *Fæces*; and that Gentleman's account of ſome of the Properties of this Bark being founded on the quantity of Reſin ſuppos'd to be contain'd in it: I propos'd to Dr. *Douglas* and ſome other Gentlemen of the Society, (who agreed with me in believing, that ſcarcely any part of any Plant whatſoever would yield that quantity of Reſinous Extract) to try the Experiment, which was perform'd after the following manner.

I took two Ounces of pick'd Bark, and digeſted it in rectify'd Spirit of Wine, which was often decanted and freſh Spirit put on, until the Bark would yield no more Tincture. The impregnated Spirit being evaporated by a very gentle heat; there was left two Drachms of Reſinous Extract; the remains of the Bark dry'd, weigh'd one Ounce, two Drachms and a half; the loſs this way is three Drachms and an half.

I boil'd theſe remains in ſeveral Waters, until they would no longer tinge the Water, which being evaporated yielded one Drachm and an half of extract; the remains of this dry'd weigh'd one Ounce and half a Drachm; the loſs by this Method is half a Drachm.

I took two Ounces more of pick'd Bark, and boil'd it in ſeveral Waters, 'till the Bark gave no more Colour; and then upon an Evaporation of the Water, had two Drachms of Extract. The remains being dry'd weigh'd one Ounce ſix Drachms; here the loſs was not any thing, except ſo much as might anſwer in weight to the quantity of the *Menſtruum* left in the Extract, which Allowance muſt likewiſe be made in the other Extracts.

I digeſted the remains in rectify'd Spirits of Wine, untill they no longer ting'd the Spirit, and by a very gentle Evaporation, I found remaining one Drachm of Reſinous extract. What was left, when dry'd, weigh'd one Ounce, two Drachms, and an half; in this the loſs was two Drachms and an half.

The Difference in the quantity of Extract obtain'd by theſe two different Methods, is but half a Drachm, and the Medium between them, upon putting together the ſeveral Extracts made with Spirit of Wine and Water, is in the whole but three Drachms and a quarter. But the Extract made with Spirit of Wine alone, is no more than two Drachms from two Ounces of the *Cortex*, inſtead of ten Drachms, which it ought to have yielded according to Monſieur *Boulduc*.

IX. *An Account of the new Method of Cutting for the Stone. By* J. Douglas, *Surgeon,* F. R. S.

EVer ſince I was Anatomiſt and Surgeon enough, to reaſon upon the Cauſes of the Tedioufneſs and bad Succeſs of the common Methods of Cutting for the Stone, I concluded that they were principally owing to the natural Structure and Situation of the Parts concern'd.

Therefore I began to conſider, why that Operation might not be perform'd the high way, ſo frequently mention'd by Authors, but never approv'd of by any, except the moſt ſagacious of all Surgeons, *Fr. Roſſetus*, who has certainly been very little read, or elſe very ill underſtood, elſe this Operation had not been ſo long a Secret.

After making ſome Experiments on dead Bodies, I was convinced, that the Stone might be extracted that way with a great deal of leſs trouble than commonly, and I was perſwaded that the Wound would heal again, by the great number of authentick Inſtances we have of accidental Wounds in the ſame place being ſpeedily and firmly cured; and therefore I reſolv'd to make the Experiment on the firſt Patient I could meet with, which I could not procure till *December* 1719. and then I proceeded as follows.

The Patient was plac'd flat on his Back, on a Table, with a Pillow under his Head; then his Wriſts and Ancles were faſten'd together, with Straps Then

I ordered one Aſſiſtant to his Head, another to each of his Shoulders, two to the *Penis*, one of which was to manage the Ligature, and the other the Prepuce, and one to each Knee, to hold them as faſt and firm as poſſible.

The Patient and Aſſiſtants being thus plac'd, the Operation conſiſts of three Parts.

1*ſt*, In filling the Bladder, which is done thus:

Firſt, Paſs the *Catheter*, Fig. i. then draw out the Stillet, Fig. ii. then order the Ligature Aſſiſtant to caſt the Ligature, with is a Skein of Silk, round the *Penis*, above the Glans. Then fix the Key, Fig. iii. to the head of the *Catheter*, Fig. iv. to keep it ſteady, while you ſcrew on the Syringe, Fig. v. then ſcrew the ſecond part of the Sucking-Pipe, Fig. vi. to the firſt, Fig. vii Then order the *Penis* Ligature to be ſtraiten'd, and the Prepuce Aſſiſtant to gather the Prepuce up about the *Catheter*, and to hold it as cloſe as poſſible. Then order the Water, being a little warmer than Milk, to be clapp'd under the Sucking-Pipe, then draw up the Water into the Syringe, and thruſt it into the Bladder at leiſure, and repeat it till the Bladder is ſo full, that you can perceive its Tumour through the *Abdomen*, (at which time you will alſo obſerve the *Penis* above, and the Prepuce below the Ligature, very much ſwell'd, and the Patient in a great deal of Pain) which is a certain Sign that there is enough injected; then withdraw your Syringe and *Catheter* together, taking particular care that your *Penis* Aſſiſtants ſtraiten their gripe, leaſt the Water ſhould follow the *Catheter*, which would undo all.

2*dly*, In making the Inciſion, which is done thus:

Order the *Penis* Aſſiſtants to turn the *Penis* towards the *Anus*, that ſo their Hands may be the more out of the way, then take the firſt Knife, Fig. viii. and cut at leiſure, and with a ſteady Hand, from near the upper part of the Tumour of the Bladder, or lower, according to the computed bigneſs of the Stone, down to the *Os Pubis*, and exactly in the middle; when you are got a little more than half way through the abdominal Muſcles, take the ſecond Knife, Fig. ix. clap its back on the middle of the *Os Pubis*, then run its point down towards the *Sphincter*, until you get into the Cavity of the Bladder, which is diſcover'd by the iſſuing out of the Water, then run your Knife along very quickly towards the Fund of the Bladder, as far as is neceſſary.

3dly, In extracting the Stone, which is done thus: Before you withdraw your Knife, introduce the fore and middle Fingers of your left hand, between the Knife and the *Os Pubis*, into the Bladder, then withdraw your Knife, and thruſt the fore and middle Fingers of your right hand into the *Anus*, and raiſe the Stone up towards the Wound, and ſo you will eaſily catch hold of it, (though never ſo ſmall) with your Fingers which are in the Bladder, then draw it out with the ſmalleſt end foremoſt. Then introduce your Fingers again, to ſee if there are any more Stones, which are to be drawn out as before.

Then take a Needle and Thread, and make one Stitch through the Skin, in the middle of the Wound, and tie it pretty cloſe, then undo the Straps and carry the Patient to Bed.

The Patient being put to Bed, I laid a Pledget arm'd with Balſam over the Wound, and a bit of ſticking Plaiſter over that. Then I embrocated all the *Abdomen*, *Scrotum* and *Penis*, with warm *Ol. Chamæmel.*; then I applied over the dreſſing and all the *Abdomen* an *Emollient Pultice*, ſpread almoſt an Inch thick on ſoft Flannel; then I turn'd a ſwath a little broader than the Patient's Hand once round him, and pinn'd it on the Pultice Cloath, juſt tight enough to keep it on.

As ſoon as he was dreſs'd, I gave him an Opiate (for nothing is ſo proper as reſt) ſuch as this ℞ *Aq. Cinnam. Hord.* ℥ii. *Laud. Liq. Gutt.* xv. *Syr. de Mecon.* ℥ii. which may be encreaſed or diminiſhed as the Caſe requires.

Next Evening I took off the Pultice and dreſſing, and cut the Stitch; then fomented the Wound and all the *Abdomen* with Stupes wrung out of *Aq. Calc.* and freſh Urine, as warm as he could bear it; then dreſs'd the Wound as before. Then I rubb'd all the *Scrotum, Penis,* and Groins with *Unguentum Album,* to prevent their being ſcalded by the Urine, which flows from the Wound.

The Wound muſt be dreſs'd twice a Day at leaſt, till you have a plentiful Digeſtion.

After every dreſſing, the Ointment and Oil was uſed, as before directed.

There is little Variation in the dreſſing of the Wound, except what is common in others.

The Urine flows always through the Wound, until the Wound of the Bladder is cured, which is ſooner or later according to the Conſtitution of the Patient.

When the Urine begins to come the right way, it pains and ſcalds them much after the ſame manner, as when they had the Stone, (which is cauſed by the Contraction of the *Urethra,* that has been ſo long uſeleſs,) but it never laſts above a Day or two, and then they make Water with the ſame Eaſe and Freedom, as any other Perſon.

They ought not to be forced to go to Stool under ſix or ſeven Days, unleſs there is ſome particular reaſon for it, becauſe ſtraining to go to Stool injures the Wound.

They ought never to be taken up, except to get their Beds made, until the Urine comes all the right way, becauſe it makes them ſick and faint; and conſequently hinders the Cure of the Wound.

Cold is to be avoided as the Peſt, becauſe it puts them to a great deal of pain either to ſtifle it, or to Cough out.

N. B. If a flexible *Catheter* could be paſs'd, and kept in the Paſſage without pain, it would very much haſten the Cure of the Wound.

I made this Operation the firſt time on the 23d of *December* 1719. upon a Boy between 16 and 17 Years of age, and in five Weeks time he was perfectly cured.

The Stone was of the Figure and Bigneſs of *Fig*. x.

I made the ſecond Operation on the 12th of *May* 1720. on a Boy of Eight Years of age, and in ſix Weeks time he was perfectly cur'd.

The Stone was of the Figure and Bigneſs of *Fig*. xi.

The third Patient was but three Years of age, and was Cut in *Auguſt* 1720. but died of Convulſions about 15 Hours after the Operation.

Fig. xii. Shews the Form and Bigneſs of this Stone.

The fourth Operation was made on the 23d of *March* 1720-21. upon a Boy about 14 Years of age, and in four Weeks afterwards he was perfectly cured.

This Stone is repreſented in *Fig*. xiii.

In this Patient I made a ſmall Wound in the *Peritonæum*, through which I ſaw the Guts preſent, but I puſh'd them back with my Finger, and then ſtitch'd the Skin, and we had no farther Inconveniency by it. Theſe three Patients have been already ſhew'd before this Society.

This Operation may be perform'd with equal Succeſs on Females, when the Stone is large, but if it is but ſmall, the common way of extracting them is very good.

From all which I think I may ſafely conclude in the Words of the inimitable *Roſſetus*.

Poſt hac nemini dubium eſſe debet, novam hanc noſtram Cyſtotomiam, vetere illâ tot doctiſſimorum Chirurgorum Cyſtotomiâ (tam periculosâ, ut eam aggredi vel ipſe Hippocrates Chirurgôn Chirurgotatos metuerit) & leniorem & tutiorem haberi.

F I N I S.

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Philoſoph: Tranſact: N° 372.

Fig 1

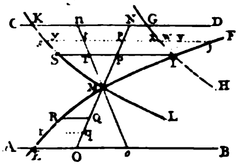
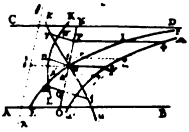


Fig 2



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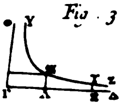


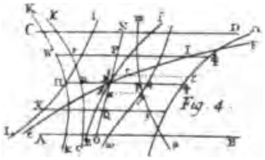
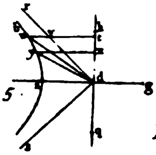


Fig. 4.

Fig. 5.



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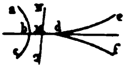

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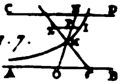


Fig. 8

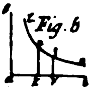


Fig 9

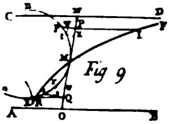
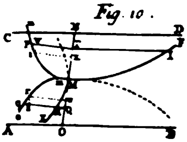


Fig. 10.



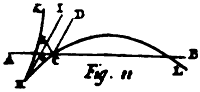
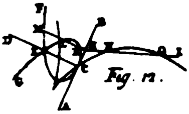


Fig. 11



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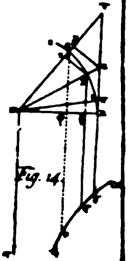


Fig. 14.

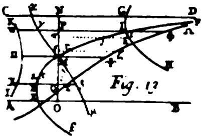


Fig. 13

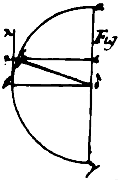


Fig 15

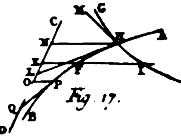


Fig. 17.

Fig. 16.

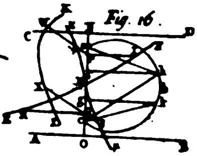


Fig 20

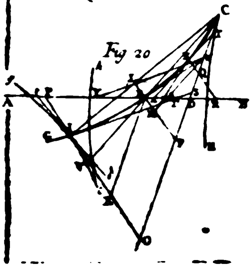


Fig 18

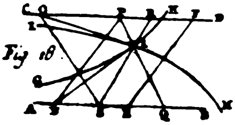
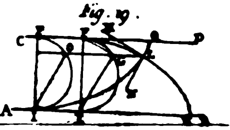


Fig. 19.



Numb. 372.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *June*, *July*, and *Auguſt*. 1722.

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Soc. Socius; Aſtronomiæ & Experimentalis Philoſoph. poſt *Coteſium* Profeſſor. *Cantabrigiæ* 1722. in 4to. Proſtant apud Bibliopolas Londinenſes.

ERRATA.

PAG. 111. *lin.* 13, *pro* = *leg.* proportionalem.

Pag. 114. *lin.* 15, *dele* '.

Pag. 123. *lin.* 13, *leg.* $\sqrt{aa+RR}+R$.

Pag, 127. *lin.* 3, *dele brevem lineam, quæ ſuper* + *b ducitur.*

Pag. 129. *lin.* 15, *leg.* q φ υ.

*Pag.*130. *lin. ult. poſt* invenietur *adde* ſi modo *m* ſit numerus affirmativus; vel etiam ſi *n* ſit unitas, *m* numerus affirmativus, & 2 *p* numerus impar negativus numero *m* major.

Pag. 132. *lin.* 12, *poſt* requiritur *adde* cujus ſegmenti centrum æqualiter diſtet a lineis AB, CD.

Pag. 138. *lin.* 19, *leg.* IΓS.

I. *An Account of a* Parhelion *ſeen in* Ireland. *In two Letters from* Arthur Dobbs *Eſq*; *of* Caſtle Dobbs *in the County of* Antrim, *to his Brother Mr.* Richard Dobbs *of* Trinity-College *in* Dublin; *and by this laſt communicated to the Royal Society.*

I Saw a pretty diſtinguiſhable *Parhelion* here on *Thurſday* Evening, of which I ſend you the Account below, as I minuted it down after I came Home, having been in the Fields when I ſaw it.

March 22d 172½, about half an Hour after 5 in the Afternoon nearly, I ſaw a diſtinguiſhable *Parhelion*, the Sun near *Weſt*, about an Hour high, the Wind and Carry of the Clouds, about N. and by E. the Sky in ſeveral Places obſcured with light Clouds, and the Sun entring into one ſomewhat more watery, yet ſo as to diſtinguiſh its Disk, At firſt appeared below the Sun, breaking out of the Cloud, ſuch Rays as are uſually ſeen in an Evening, in a Sky interſperſed with Clouds. In a little Time appear'd at the ſame Height with the Sun, as near as I could gueſs, having no Inſtrument, a luminous Spot, being about four Times the largeneſs of the Sun's Disk, and about 30 D. diſtant from the Sun to the Southward, which was covered with the lively Shades of red and yellow on the Side next the Sun, and encreaſed in Splendor (ſo as ſcarce to be born by the naked Eye) till it exceeded the Brightneſs of the Sun, which was then under a thin Cloud, ſo as eaſily to perceive his Disk. After this had appeared about 3 or 4 Minutes, I finding it to be a real *Parhelion*, began to look about for the *Halo* they generally appear in; and as I obſerved ſome Rays reſembling a Glory to point upwards from the Sun, I ſaw in thoſe Rays at the ſame Diſtance (being, as near as I could gueſs, about 30 D. perpendicularly above the Sun) the Colours of the *Halo* appearing as in the luminous Spot; but inſtead of finding it, as I expected, in a Circle ſurrounding the ſun, it was inverted, yet not circular, but making an obtuſe Angle, the point towards the Sun. I then looked to the Northward of the Sun, and as the Cloud, which was thicker on that ſide, moved Southwardly, a luminous Spot began to appear at the ſame Diſtance from the Sun as the other, and in the ſame Parallel of Altitude, which had the ſame Colours towards the Sun, and increaſed in Brightneſs, but did not come up to the Brightneſs of the other Spot, yet was as luminous as the Sun then appear'd: this Spot was very little bigger than the Sun's Disk. As the Cloud mov'd on, till it came to about 60 D. to the Southward of the Sun, and 30 D. from the Spot, at an equal Height there appeared another Spot tinged with the Colours of the Rainbow. The whole Appearance laſted a Quarter of an Hour. The Reaſon of my not ſeeing the *Halo's*, which generally appear with them, was, that there was a good deal of clear Sky above the Sun, and the Cloud was too thick below it.

Caſtle Dobbs,
March the 24th 172½

Arthur Dobbs.

I Have ſent you, as you deſired, the Scheme of the *Parhelion*, with all the Notes that I cou'd take in the ſhort Time it continued, having been only out occaſionally, without any Inſtrument to compute the Proportions, or take Angles; ſo that my Obſervation cannot be exact, but only approach the Truth. However I can recollect nothing material, that I have omitted.

A. The Place of the Sun, being nearly Weſt about 12 or 13 D. above the Horizon, being about an Hour before Sun-ſet.

B. The luminous Spot, being about 30 D. to the Southward of the Sun, as near as I cou'd compute, having no Inſtrument to take the Angle, and in the ſame Parallel of Altitude; the Spot was not ſo well defin'd as in the Scheme, being more imperceptibly ſhaded off in the Cloud, the two ſemicircular Lines next the Sun were thoſe tinged with the Colours; the neareſt the Sun being of a deep Scarlet, the inner one a deep yellow, both the Colours being ſoftned as they fell off from the Sun, all the reſt of the Spot being an intenſe Light, ſo as the naked Eye cou'd ſcarce bear it.

C. The other Spot to the Northward, which appeared ſometime after that marked B, being not quite ſo large, nor the Colours ſo intenſe, but the ſame way diſpoſed, thoſe next the Sun being red, the next yellow, and the reſt white.

D. A Spot in the Cloud, as it moved Southwardly, till it came to about 60 D. Diſtance from the Sun which had the Colours as in the other Spots, that next the Sun being red, the next yellow, but much fainter than in the *Parhelia*.

E. The Appearance of two Segments of Circles, at about the ſame Diſtance from the Sun, as the *Parhelia*, being perpendicularly above it, the Colours being fainter than in the *Parhelia*, but the ſame Way diſpos'd, the lower Lines next the Sun expreſſing the red, and the upper the yellow.

The Colours at D, and E, as they were not ſo intenſe, neither were they quite ſo broad as thoſe at B and C; the two Colours being added together were about $\frac{1}{4}$ of the Disk at B, and the Colours in the ſame Proportion at C; the Diameter of the *Parhelion* at B, being about double the apparent Diameter of the Sun, as near as I could compute, as in the Scheme is expreſs'd.

The Centres of the Segments of the *Halo*'s mark'd E, if not in the *Parhelia*, were very little below 'em.

Below the *Sun* and *Parhelia* the Cloud was too thick to diſcover any thing thro' it; and above them, till near the Segments mark'd E, the Sky was ſerene and nothing obſcured; but at E, where the Rays, which pointed upwards from the Sun, terminated, it appear'd hazy, and ſo thick as to reflect the Colours.

Caſtle Dobbs,
April 2. 1722.

Arthur Dobbs.

II. *A Letter to the* Royal Society *concerning the Particles of Fat. By Mr.* Leeuwenhoek, *F. R. S. Tranſlated from the* Dutch *by* John Chamberlayne, *Eſq;*

Delft, *May 1ſt, 1722.*

I Take the Liberty to lay before you the following Obſervations, which I made about two Years paſt.

I have formerly ſaid, that the Matter which we call Meal, or Flower, in Wheat, Rye, Barley, Oats, and in all ſorts of Beans, is ſhut up as it were in little Cells or Chambers, and that thoſe little Cells are ſeparated from each other by thin Membranes, which are thinneſt in Wheat. And foraſmuch as in the Enquiry into what is called the *Perioſteum* of an Ox or Sheep, I have often broke in Pieces the fat Particles thereof, and as often viewed thro' a Microſcope the ſaid broken Particles, ſo have I likewiſe placed a few of the fat Globules upon a clean Glaſs Plate, and afterward held the ſaid Glaſs Plate over a Coal Fire, or the Flame of a Candle, ſo long, till they were all melted and reduced into a liquid Matter; ſo that not only the Fat, which was ſhut up in the Skin of the fat Globules, but likewiſe the Skin itſelf was reduced to a fluid Matter; and thereupon I immediately brought it before my Sight; and viewing it with Attention, perceived, when the melted Fat was cold, that there were different Matters inclos'd in the ſaid fat Globules; for as much as there appear'd an inconceivable great Number of exceeding ſmall coagulated Particles, and the reſt of the

Parts, of which the Fat was compoſed, lay in one ſmooth and even Subſtance, as I have formerly ſaid; and I have been conſidering whether there might not be incloſed in ſuch a Globule of Fat, ſo many little Cells and Partitions as we ſee in a little Grain or Seed, but if it be ſo, it will remain concealed from our Eyes.

But having now again carefully contemplated theſe coagulated Globules of Fat, many of which go to the making of one little Bubble, I did often fancy, that I ſaw, that each of the ſaid ſmall Particles was provided with ſuch a tranſpareat Dent, as I have before ſaid, that the Meal Globules of Wheat, &c. are furniſhed with: From which Obſervation I ſaid to myſelf, ſince we find, that the great Creator of the Univerſe has made all his Creatures almoſt after the ſame manner; and that there grows no Plant whatever, let the Seed be never ſo ſmall, but there are inclos'd in it the ſame ſort of Particles as are found in bigger Seeds; and that tho' they differ in Magnitude, yet are they analogous one to the other: this being ſo, we may eaſily imagine, that a fat Globule has a Bark or Skin as well as any Seed, as we have often ſeen, and that they are furniſhed within with Particles analogous to thoſe of the Seeds, which we call Wheat, Oats, and the like; and altho' we render the Fat fluid by the help of Fire, yet will the ſmall Particles which are incloſed in the fat Globules, again reſume their former Figure, as I have formerly ſaid.

Yea I have fancied to myſelf, tho' it did not appear to my Sight, that each fat Particle is furniſhed with little Cells within, like the Seeds or Fruits of Plants.

Since I have ſet this down upon my Paper, I was informed that my Butcher had killed a Sheep of an uncommon Bigneſs, and that it weighed 140 Pounds, without the Fat that they took out of it, after it was kill'd, which weighed 51 Pounds, ſo that the whole Sheep weighed above 190 Pounds.

I cauſed a Piece of the Fat, that grew about the Kidneys of the ſaid Sheep, to be brought to me, imagining that its fat Particles would be of a coarſer Grain than thoſe of ordinary Sheep; for I have obſerved ſeveral times, that the bigger an Ox was, the larger were the fat Particles thereof; and ſince not one Man in a thouſand has any Knowledge of the Contexture of theſe fat Particles, for we find that there are not any two of one and the ſame Figure, they being compreſſed by other Particles with which they are ſurrounded, as I have frequently taken notice, I have cauſed ſome few of theſe fat Particles to be drawn here, as may be ſeen in *Fig.* 1. between A, B, C, D.

Now when we meet with one of theſe little Bundles of fat Particles, as has frequently occurr'd to us, in which the fat Particles were four times this Thickneſs; I imagine, that ſuch fat Particles cannot be produced out of one ſingle adipoſe Veſſel, but that out of ſuch a Veſſel ſeveral ſmall Sprigs iſſue forth, and out of each of thoſe ſmall Sprigs proceed others ſtill ſmaller, and that out of theſe Particles one larger fat Particle is formed like a Bunch of Grapes.

Now I cut off with a Razor the Fat in ſeveral Places of a greater Piece, as thin as I could, laying the thin Pieces upon ſeveral Glaſs Plates, and put them upon a Coal-Fire, ſo as to cauſe them to melt; and being melted, immediately viewed them with a Magnifying-Glaſs, when I obſerved the Skins, or membranous Coats, of the fat Globules lying among the melted Particles, and in the ſaid melted Particles there was nothing to be perceived but a limpid Matter ſurrounded with ſmall Air-Bubbles; but when the Fat was congealed, we could obſerve but very little of the Mem-

branes,

branes, becauſe they were covered with the Particles of Fat, with which theſe Membranes or Skins had before been filled.

I cauſed a few of theſe Skins of the fat Globules to be drawn, as may be ſeen here at *Fig*. 2. between E, F, G, H. During the ſaid Obſervation, I fix'd my Eye with attention upon the fat Particles of the Sheep which had been melted, and were again coagulated; and I could not but judge, that theſe fat Particles, which were exceeding ſmall, were analagous to that internal Matter, wherewith ſome of the ſmalleſt little Seeds are furniſhed, and in a great many of theſe exceeding ſmall Particles, I could in clear Weather diſcover ſome Tranſparency.

Moreover, I cut as thin Slices as it was poſſible of the Fat, yea ſo thin, that five or ſix of them did not weigh a Grain, and put them into a little Water, in order to try whether I could make any farther Diſcoveries thereby, with reſpect to the ſmall Particles of Fat; but it was in vain: only I ſaw floating upon the Water very ſmall Particles of Fat, which were coagulated in a ſpherical Figure, and the very biggeſt of thoſe fat Particles was no bigger than a Grain of Sand. I placed theſe Particles upon a Glaſs Plate; and viewing them with a Microſcope, I obſerved the Figure, which I mention'd above, as plain as before; and other fat Particles ſeemed to be of a different Figure. I put one of theſe into the hands of my Painter, or Deſigner, bidding him to draw what he obſerved, it being the Figure of one of the ſaid fat Particles, which was coagulated on the Water, as it is repreſented here by *Fig*. 3. between I, K, L, M, which was not very conformable with the other melted fat Particles; for in the doing it, all the Particles did not melt, for the fat Particles are not all extracted by the Water, and coagulate upon the Water in ſmaller and greater globular Particles; and when we take out of it the Remainder of the thin Slices of Fat, which float upon the Water, and view them with a Microſcope, we find that many of the fat Particles appear intire to the Eye; and whereas they were before very ſmooth and even in their Sides, they were now changed into rough and uneven Particles; ſo that one ſhould be apt to think, that there were two different ſorts of Particles in the Fat, and that one ſort melted more eaſily than the other.

Now in order to get theſe melted Particles of Fat out of the Water, without altering them, I made uſe of a round Glaſs, and with it skimm'd the Superficies of the Water; by which means ſome of the coagulated Particles ſtuck to the Glaſs. Moreover, I did again melt ſome of the fat Particles, which had been coagulated upon the Water, over a Coal Fire, as they lay in the Water; and when they were again coagulated, viewing them with a Microſcope, I found the ſmall fat Particles to be yet ſmaller than thoſe that were melted out of the Water.

In this laſt Obſervation I obſerved, with aſtoniſhment, the inconceivable Number of Veins and Membranes, which were diffus'd through the Fat, and the Multitude of ſeparated fat Particles, that were involved in their ſeveral Membranes.

After this, there was laid before me the Hind-Quarter of a ſucking Lamb, over which was ſpread what we call the Net, or Caul; and having cut off ſome Pieces of the ſaid Net, or Caul, upon which there was little or no Fat, with a Pair of Sciſſars, and placing them before a Microſcope, I obſerv'd again, that the fat Particles, where there were very few of them included between the Membranes, were of a more globular Figure than in other Parts, where a good many lay together, and that in other Places they were preſſed or bruiſed, which I fancy was occaſion'd by the Butcher's ſqueezing the Caul in that place with his Fingers; and in another Place the fat Particles had been ſo torn in pieces, that I could ſee nothing remaining but the Skins of the fat Globules.

Moreover, I ſaw that the fat Particles had ſuch a Pinch, or Dent, in them, as I have ſhewn, that there were in the Globules of Flower of Wheat; from which Spectacle, I am confirmed more than before in my Opinion, that the fat Globules might be ſeparated intirely, or in part, from the Skin with which they are ſurrounded, by opening the Dents, without breaking the Skin.

Then I took off the thin Membranes, which encompaſs'd the fat Particles, and viewing them with a Microſcope, obſerved, that the fat Particles had imprinted a roundiſh Figure on the Membranes, inclining to a hexangular Shape, that it was a Pleaſure to look on 'em; but in other Parts they were of an oval Figure.

Moreover, I took a flat Fiſh, which we call *Plaiſe*, and took off the Fat which adher'd to the Veſſels, or Bones, and viewed it with a Microſcope, and obſerved, that the Fat Particles were of ſeveral Sizes; and ſome were ſo ſmall, that I judged that fifty of the leaſt were no bigger than one great fat Globule; and moreover, I ſaw that many of the fat Globules had ſuch a Pinch in them, as we find in the Meal, or Flower, of thoſe little white Beans, which we call *French* or *Kidney-Beans*.

Afterwards my Servant brought to me the Fat of a *Perch*, which was nine or ten Inches long, and taking a little of it, I viewed it with a Microſcope, but could not diſcover any ſmall Particles in it, nor any internal Dent, as I had obſerved in the Fat of a ſmall *Plaiſe*.

After that the Fat of the *Perch* had lain an Hour or two upon the Glaſs, I viewed it again, and obſerved that the Particles were leſſened, or become ſmaller, and that the Skin of the fat Particles, which as yet was beſet with ſome fat Particles, was, as it were, ſhrunk, or wrinkled, and the Fat that was burſt out, lay about the fat Particles, and was ſo fluid and tranſparent, that we could not diſcover any Parts in it.

From this Obſervation I began to think, whether each of theſe fat Particles was not provided with an Orifice, or Hole, out of which the Fat might be protruded at all times, as often as the Parts of the Fiſh ſtood in need of Nouriſhment, without an entire laying open the Skin of the fat Globules; for we conſtantly find, that when the Eggs of the Perch, which we call the Roe, increaſe in Bigneſs, its Fat decreaſes; yea in ſuch a manner, that when the ſaid Eggs, or Roe, are arrived at their utmoſt Bigneſs, there is ſeldom or never any Fat to be ſeen upon the Inteſtines of the Fiſh.

Thus far my Notes go about Fat; and if my Health will allow me, I ſhall ſhortly tranſmit to you ſome more of the Obſervations which I have ſet down in writing.

III. *Part of a Letter from* Thomas Forſter, *Eſq*; F.R.S. *to Mr.* Machin, *Prof. Aſtron. Greſham, Secr. R. S. concerning a new Iſland lately raiſed out of the Sea near* Tercera.

London, *May* 12. 1722.

I Here ſend you a Draught of the ſeveral Bearings, and the Captain's Account of an Iſland lately broke out of the Sea, bearing S. E. from *Tercera*, Diſtance 17 Leagues.

John Robinſon, being Maſter of a ſmall Pink-Snow, called the *Richard and Elizabeth* from *Piſcataqua* in *New-England*, arrived at *Tercera*, *Decemb.* 10. 1720. near which Iſland he ſaw a Fire break out of the Sea. The Governor hired him to go with the ſaid Veſſel to view it, and ſent on Board ſixteen Sailors, and two Prieſts. On *Sunday*, the 18th of *Decemb.* we got under Sail at 12 o'Clock at Night, and ſtood from *Angras*, S. E. The next Day at two o'Clock in the Afternoon, we made an Iſland all Fire and Smoak: we continued our Courſe till the Aſhes fell on our Deck like Hail or Snow all Night. We bore from it, the Fire and Smoak roared like Thunder, or great Guns. At Break of Day we ſtood towards it again: at 12 o'Clock we had a good Obſervation, two Leagues South from it. We ſail'd round it, and ſo near, that the Fire and Matter it threw out, had like to have done us Damage: In which Conſternation we all betook our ſelves to Prayers, being in danger of driving aſhoar: then a ſmall Gale, at S. E. ſprung up, and carried us clear to our great Joy. The Breeze was accompanied with a ſmall Shower of Rain, which caus'd a great Duſt to fall on our Deck; with the ſaid Breeze we ſtood away for *Tercera*. The Governor inform'd us that the Fire broke out the 20th of *Nov.* 1720. in the Night, and that the prodigious Noiſe it made, caus'd an Earthquake, which ſhattered down many Houſes in the Town of *Angra*, and Places adjacent, to the great Terror of the Inhabitants. We took ſeveral Draughts of the Iſland at ſeveral Bearings in our ſailing round, of which the incloſed is a Copy. Prodigious Quantities of Pumice-Stones and half-broil'd Fiſh were found floating on the Sea, for many Leagues round the Iſland, and abundance of Sea-Birds hovering about it. So far the Captain.

An Acquaintance of mine informed me, that in his Paſſage from *Cadiz* to *London* (the latter end of *April* was 12 Months) he obſerved the Sea from Cape *Finiſter*, almoſt to the Chops of the Channel, to be covered with Pumice-Stones, ſome of which he gave me.

IV. *A Letter from Mr.* Ralph Thoresby, *F. R. S. to Sir* Hans Sloane, *Bart. Preſident of the College of Phyſicians, and Vice-Preſ. R. S. concerning the Effects of a violent Shower of Rain in* Yorkſhire.

THE Effects of a violent Shower of Rain at *Riponden*, near *Halifax*, were ſo ſurprizing, that I wrote to a Gentleman in thoſe Parts for an Account that might be depended upon; and particularly deſired to know, whether there was not an Eruption of

Waters out of the Hills, as the late ingenious Mr. *Townley* of *Townley* wrote me there was out of *Pendle-Hill*, in that at *Star-bottom* mentioned in the *Philoſ. Tranſ.* N° 245. but all the Account I can learn of this is, that what they call the Daſhing of two great watery Clouds upon the Hills, occaſioned the Inundation; what ever was the more immediate Cauſe, the Effects were diſmal, and ſo ſudden, that tho' it was upon the Day-time, the poor Creatures could not ſave their Lives. This Calamity happen'd the 18th of *May*, 1722. betwixt the Hours of 3 and 5, when by the modeſteſt Account the Beck was rais'd two Yards at leaſt in perpendicular Height above what was ever known before; which may be eaſily conceived by the Situation of the Place implied in the Termination *den*, which ſignifies a deep Valley between pretty ſteep Hills on each Side. *Fontes in convallibus*, is in the *Saxon* Verſion rendered ƿıllaꞅ on ꝺenum *Pſ.* ciij. 2. and *Valley of Tears*, (as this now) *Pſ.* lxxxiij. 6. ꝺene ꞇeoꞃa. Several Houſes, four Mills, (ſome ſay ſix) nine Stone-Bridges, and ten or eleven of Wood, are taken down, and the Wheels, Dams, and Sluices, (*Eboracentibus* Goits, from the verbal Noun ᵹeoꞇan *fundere*) of moſt of the Mills that are left ſtanding, broken and damaged; and a great deal of Cloth gone. Fifteen Perſons were drown'd, of whom *Jonas Longbothom* and his Servant are not yet found. Seven out of eight in one Houſe were either ſlain by the Fall of it, or drowned. A young Man eſcaped by help of a piece of Timber, was turned over and over again, in paſſing two or three Dams, but at laſt taken out alive, tho' diſtracted for the preſent; but it's hoped he may recover.

The Rapidneſs of the Torrent was ſo violent, that it took down the North-ſide of *Ripponden* Chapel, and carried off moſt of the Seats. A Man of *Deusbury* told me, that he ſaw four of them that were driven to that Town; and the Rector of *Caſtleford*, who viſited me the Day after, inform'd me, that many Goods were carried down ſo far, tho' above 20 Miles off. It tore up the Dead out of their Graves; at firſt I was willing to believe it only of an old Woman that had been buried that Afternoon, and ſo the Earth not fully ſettled again; but am ſince informed by a ſure hand, that two corrupted Corpſes were driven upon one Gentleman's Land, and as many upon another's. It ſwept away all the Corn-Land, as deep as the Plow had gone. Some Perſons ſav'd themſelves by forcing a Way out of the Roofs of their Houſes, and ſitting upon the Ridges till the Floods abated.

I was that Day ſeized by a ſmart Thunder-Shower, upon the Moor, as I was coming home.

Leeds, June 5.
1722.

V. *Part of a Letter from Monſ.* Couzier, *Phyſician in the Infirmaries at* Alais, *to Dr.* Deidier, *Profeſſor of Chymiſtry in the Univerſity of* Montpelier, *concerning a new Experiment made with the Blood of a Perſon dead of the Plague. Communicated to the Royal Society by Dr.* Woodward, F. R. S. *Prof. Med. Greſh.*

ON the Firſt of this Month I took an Opportunity of beginning the Experiments you had recommended to me, and of which I had promiſed to give you an Account.

I took a Quantity of Blood from the Body of a Perſon dead of the Plague, and mix'd it with warm Water, which Mixture I attempted to inject into the Crural Vein of a Dog, but the End of the Syringe being too large to enter the Vein, the Experiment did not ſucceed.

This made me reſolve to try the other Method you had propoſed to me, which was to lay ſome of the ſame infected Blood upon the Wound. This I accordingly did, and cover'd it with a Dreſſing, which the Dog got off in the Night. I found the next Morning that the Dog had lick'd his Wound, and that he refuſed his Food. Towards Night he began to bemoan himſelf, and gave Signs of an approaching Death. The next Morning I found him dead, the Wound being conſiderably ſwell'd and gangren'd, and the Edges round the Swelling were likewiſe gangrened.

Upon opening the Body we found the Liver ſomething larger than uſual, with Spots of a livid Purple, as in the Bodies of Perſons dead of the Plague. In the Stomach was found a Quantity of black coagulated Blood, of the Size of a Hen's Egg. This in all Likelihood was what he had ſwallow'd upon licking the Wound. The Heart was very large, with a black grumous Blood in the Ventricles, and the Auricles were turn'd blackiſh and gangrenous.

Alais, April 3.
1722.

VI. *Extract of a Letter from Dr.* Deidier, *concerning an Experiment made with the Bile of Perſons dead of the Plague. Communicated by Dr.* Woodward.

WE cauſed two Dogs to ſwallow a pretty large Quantity of the Bile taken from the Bodies of Perſons dead of the Plague.

Upon this the Dogs appeared heavy and melancholy, refuſed their Food, and made Water very often, eſpecially when they were any ways diſturb'd. Their Urine was thick and very fetid, and their groſs Excrements were tinged with the black and greeniſh Bile, which they had ſwallow'd. But in a few Days thoſe Accidents went off, and the Dogs recovered their perfect Health, though they were all the Time confin'd in a Cellar belonging to the Diſpenſary, in company with the other Dogs that we tried our Experiments upon.

Montpelier, April 28.
1722.

VII. *Solutio Problematis de curvis inveniendis, quæ quadam ratione in ſitu inverſo diſpoſitæ ſe interſecare poſſunt in angulo dato.*

JAM primum ad manus pervenerunt *Acta Eruditorum* ad hunc menſem *Auguſti*, ubi invenio a me peti, ut ea aperiam, quæ notis fictis celata in *Supplementorum ad Acta Eruditorum*, Tom. 8. Sect. 1. edita ſunt. Explicentur autem ea tabulâ ſequenti.

	z	y	x	v	u	t	s	r	qu	p	o	n	m	l	j	i	h	g	f	e	d	c	b	æ	a
b	æ	4	o	æ	i z	u	1	s	j	c	8 t	y	9	2	6	e p	3	m	f	7 l	v	w	5	r	h d
g	e	o	u	w	4 d	3	i	c	6	2	s z	7	5	y	æ	m &	1	r	j	9 p	a	h	v	8	f l
n	i	j	æ	5	o l	1	r	s	e	v	6 d	f	a	h	4	y z	w	u	7	9 t	3	8	c	m	2 p
q	o	8	3	u	v p	6	æ	1	c	w	h l	i	5	m	2	j d	f	e	9	a z	4	y	7	s	r t
x	u	y	4	1	c t	f	w	j	2	o	8 p	s	7	r	3	5 l	m	i	h	e d	9	æ	a	6	v z

In hac tabulâ ſex ſunt notarum ordines: ſupremus literas continet, quibus verba celata ſcribi debent, reliqui literas numeroſque continent, quæ verarum literarum loco uſurpantur. Literas autem b, g, n, q, x, quæ e lævâ horum ordinum inferiorum collocantur, propter earum uſum indices appellare licet. Scripturæ pars celata ab horum indicum duobus incipit, quorum poſterior oſtendit notas illum ſequentes in eo ordine, cui præfigitur, quæri oportere, ut veræ literæ cognoſcantur, quæ ſupra has notas in ordine literarum primo ſemper habentur: Ex hoc autem ordine notæ fictæ deſumendæ ſunt, donec alteri alicui indicium occurritur, quod cum ſit, hoc novo indice utendum eſt ut priori. Et hac regulâ tota ſcriptura explicabitur, niſi quod quandocunque plures indices ſunt contigui, omnes præter ultimum negligi debent: item verborum aperte ſcriptorum interpoſitio hunc indicum uſum non turbat. Hoc modo ſcriptura occulta, ſi pauci errores typographici emendentur, verba ſequentia complecti invenietur.

Curvarum, quæ problemati conveniunt, quæcunque ſumatur ordinata, illius fluxio ſecunda ab ejuſdem fluxione primâ diviſa (ut ſermone arithmeticorum utar) eandem dat quotientem, ſed contrario ſigno, ac fluxio ſecunda a fluxione primâ diviſa ordinatæ ex alterâ principii abſciſſæ parte jacentis, & ad eandem ab eo principio diſtantiam. Hujuſmodi autem curvæ inveniri poſſunt tribus regulis.*

Prima regula curvam, qualem problema requirit, ope ſpatii hyperbolici à curvâ quacunque deducit, quæ habeat ad æquales diſtantias a principio ſuæ abſciſſæ ordinatas æquales, & ab eâdem parte abſciſſæ poſitas. Eſt enim ordinata curvæ quæſitæ, ut area alius curvæ ordinatam habentis æqualem ſegmento aſymptoti hyperbolæ terminato a ſpatio hyperbolico æquali areæ curvæ primo aſſumptæ.

Regula autem ſecunda pendet a primâ, & curvam problemati ſatisfacientem ſine ope ſpatii hyperbolici ex curvis derivat, quæ habeant ad æqualia intervalla a principio ſuæ abſciſſæ ordinatas æquales, ſed a contrariis partibus abſciſſæ poſitas.

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* Scilicet, ſi abſciſſa a ſuo principio in oppoſitas partes æqualibus momentis fluit.

Et hæc ſecunda regula theorema ſequens præbet, nimirum, ſi aliqua curva ſumatur, qua problema ſolvi poſſit per regulam primam, & ſi hujus ordinatæ inſiſtunt abſciſſæ ad perpendiculum, invenietur curva problemati ſatisfaciens, ſi ad eandem abſciſſam conſtruatur alia linea curva, eâ lege, ut illius ordinata ex alterâ parte abſciſſæ ubique æqualis ſit aggregato aſſumptæ lineæ curvæ & ejuſdem ordinatæ; exceſſui autem hujus curvæ præ ordinatâ ſuâ æqualis ſit unaquæque curvæ conſtruendæ ordinata, quæ ex alterâ parte abſciſſæ jacet; omnes enim curvæ hac ratione conſtructæ problemati conveniunt.

Hoc autem theorema demonſtratur propoſitione ſequenti, quod in omni triangulo rectangulo quadratum ab alterutro latere angulo recto adjacenti æquale eſt rectangulo ſub ſummâ alterius lateris angulo recto adjacentis lateriſque angulo ei ſubtendentis, & ſub differentiâ eorundem laterum.

Denique tertia regula derivatur a ſecundâ, ope propoſitionis nonæ. libri de quadraturâ curvarum Neutoni.

SCHOLIUM.

Exemplum generale, quod exhibui, curva logarithmica, & cyclois plurimis modis inveſtigari poſſunt his regulis.

Unus caſus curvæ logarithmicæ commode invenitur per regulam primam, aſſumptâ lineâ rectâ loco curvæ in illâ regulâ memoratæ.

Alter hujus lineæ caſus deducitur ex regulâ ſecundâ ope ſpeciei quinquageſimæ nonæ linearum tertii ordinis, quæ omnium curvarum in illâ regulâ utilium eſt fere ſimpliciſſima præter parabolam cubicam & hyperbolam conicam.

Cyclois optime invenitur theoremate, quod a regulâ ſecundâ deduci diximus.

Exemplum iſtud generale facile invenitur regulâ tertiâ, aliis vero regulis non ſine ambagibus.

Regulis ſecundâ & tertiâ commodiſſime inveniuntur curvæ geometrice rationales; quæ deducuntur etiam a theoremate in regulam ſecundam pendente; quandocunque enim curva aſſumpta tam longitudinem quam ordinatam rationalem habet, cujuſmodi ſimpliciſſima eſt parabola ſemicubica, curvæ quoque inveniendæ ordinata rationalis erit.

Denique his regulis, vel etiam conditione in principio poſitâ facile eſt invenire, an curva aliqua propoſita problemati ſatisfaciet, & quibus poſitionibus id fiet: unde intelligi poteſt, an eadem curva diverſis modis problemati conveniat.

Horum brevem explicationem jam apponam, deſcribendo, ex amici chartâ, problematis ſequentis ſolutionem.

PROBLEMA.

Datis duabus lineis rectis AB, CD (in *Fig.* 1.) parallelis, ad abſciſſam AB curva EF deſcribenda eſt, quæ talis ſit, ut in ſitu inverſo ad abſciſſam CD deſcripta ſeipſam ſemper interſecet in angulo quolibet dato.

Ad abſciſſam CD deſcribantur curvæ GH, KL ſimiles & æquales curvæ EF, quarum altera huic curvæ EF occurrat in puncto quolibet I, altera vero per punctum M tranſeat, ut partes EM, KM curvarum EF, KL ſimiles ſint & æquales; & per punctum M, quod partes curvæ EF dirimit, quæ ſe mutuo interſecare debent, ducantur lineæ NMO, nMo, quæ cum rectis AB, CD angulos ſub NOB & ſub CNO, item angulos ſub noA, & ſub onD conſtituant ei æquales, in quo curva ſeipſam ſecare ponitur. Ducatur IPTS lineis AB, CD parallela; item huic proxima & parallela j x p t s; deinde ducatur I v lineæ N O parallela, & denique I w, S y parallelæ lineæ n o, ut angulus ſub I w j æqualis ſit angulo ſub I v x. Jam anguli ſub I x w & ſub I j v ſimul ſumpti æquales erunt angulo ſub x I M, ideoque & angulo ſub I w v æquales; unde angulus ſub x I w æqualis erit ei ſub I j v; & eodem modo angulus ſub j I v ei ſub I x w æqualis invenietur; adeo ut triangula I j v, x I w ſunt ſimilia, & j v : I v : : I w : w x. Porro pro abſciſſis æqualibus M P, M T ſcribatur z, pro ordinatâ P I, y, & $-v$ pro ordinatâ T S, pertinente ad curvæ X L arcum K M, qui arcui E M curvæ E F reſpondet. Creſcentibus autem abſciſſis M P, M T, & ſimul increſcentibus ordinatis P I, T S, earum fluxiones primæ eadem habebunt ſigna cum ſuis ordinatis, ſed utræque fluxiones ſecundæ idem habebunt ſignum; nam fluxio ſecunda unius ordinatæ idem habebit ſignum cum ſuâ ordinatâ, ſed alterius ordinatæ fluxio ſecunda ſignum habebit a ſigno ſuæ ordinatæ diverſum; propterea quod curvarum K M, M F alterius concavitas verſus convexitatem alterius convertitur, ut manifeſtum eſt. His autem cognitis invenietur j v : I v (= P p) : : $\dot{y} : \dot{z}$, I w (= T t) : w x (= s y) : : $\dot{z} : -\dot{v}$, & $\dot{y} : \dot{z} :: \dot{z} : -\dot{v}$, item $-\dot{y}\dot{v} = \dot{z}^2$, & denique poſitâ $\dot{z}$ invariabili $-\dot{y}\ddot{v} - \ddot{y}\dot{v} = 0$, vel $\dot{y}\ddot{v} + \ddot{y}\dot{v} = 0$, ideoque $\frac{\ddot{y}}{\dot{y}} = -\frac{\ddot{v}}{\dot{v}}$, quando y & $\ddot{y}$ ad curvam E F, ſed $\dot{v}$ & $\ddot{v}$ ad curvam K M pertinent. Idem vero locum quoque habet, quando omnes hæ fluxiones ad curvam E F referuntur, ſi abſciſſa in oppoſitas partes a ſuo principio fluere ſtatuitur; nam ſumptâ M Q = M P, & M q = M p, ductiſque Q R, q r ad A B, C D parallelis, puncta R, r in curvâ E F punctis S, s in curvâ K M reſpondent. Ponendo igitur abſciſſam in contrarias partes a ſuo principio æqualibus momentis fluere, *Curvarum, quæ problemati conveniunt, quæcunque ſumatur ordinata, illius fluxio ſecunda a fluxione primâ diviſa eandem dat quotientem*, &c. ut ſupra. Hæc autem curvarum quæſitarum conditio eſt, unde deducuntur regulæ ſequentes ad problematis ſolutionem.

Regula Prima.

CUM requiritur, ut MQ exiſtente = MP ſit $\frac{\ddot{y}}{\dot{y}} = -\frac{\ddot{v}}{\dot{v}}$, quandò abſciſſa in oppoſitas partes a puncto M æquabiliter fluit, ita ut ejus fluxioni in partibus abſciſſæ, quæ a contrariis lateribus puncti M jacent, ſigna diverſa tribuenda ſint, ponere licet $\frac{\ddot{y}}{\dot{y}} = \dot{z}$ ductæ in quantitatem quamcunque, quæ eadem maneat, & ſub eodem ſigno, pro eâdem magnitudine z, ſive illa affirmativa ſive negativa ſit. Deſcribatur igitur (in *Fig.* 2.) ad abſciſſam NO curva quælibet KL, cujus ordinatæ angulum quemcunque datum cum abſciſsâ conſtituant, & quæ habeat eas ordinatas æquales, &c. ab eodem latere abſciſſæ NO poſitas, quæ æqualiter diſtant a puncto M, ut ordinatæ PW, QX; deinde fiat $\frac{\ddot{y}}{\dot{y}}$ ordinatæ PW × $\dot{z}$ proportionalis, & $\frac{\ddot{v}}{\dot{v}}$ ordinatæ QX × $\dot{z}$. Jam (in *Fig.* 3.) exponatur hyperbola YZ ad aſymptotos ΓΔ, ΓΘ, angulum ſub ΘΓΔ angulo dato ſub NPW æqualem comprehendentes, deſcripta, & in alterutrâ aſymptoto, ut ΓΔ, ſumatur ad libitum punctum Λ, & ducatur ΛΞ alteri aſymptoto ΓΘ parallela, & parallelogrammum ΓΞ compleatur: deinde in curvâ KL ad abſciſſam NO, & ad punctum M ordinatim applicetur M Π; ſumatur ſpatium hyperbolicum Λ Ξ Υ Σ, rectâ Σ Υ aſymptoto Γ Θ parallelâ abſciſſum, æquale ſpatio W P M Π, & fiat P Φ = Γ Σ, eâque ratione deſcribatur curva ω Ψ Φ Ω; dico P I curvæ quæſitæ ordinatam eſſe ut ſpatium M P Φ Ψ. Hoc autem manifeſtum eſt; fluxio enim ſpatii M Π W P æqualis eſt fluxioni ſpatii Λ Ξ Υ Σ, ideoque $PW \times \dot{z}$ = fluxioni lineæ Γ Σ ductæ in Σ Υ vel in $\frac{\Gamma\Lambda \times \Lambda\Xi}{\Gamma\Sigma}$; erit igitur $PW \times z$ ut fluxio lineæ Γ Σ ſive lineæ P Φ per ipſam P Φ diviſa; ſed $PW \times \dot{z}$ eſt ut $\frac{\ddot{y}}{\dot{y}}$; unde erit $P\Phi \times \dot{z}$ ut $\dot{y}$, & neceſſario y ſive P I ut ſpatium M P Φ Ψ. *Prima* igitur *regula curvam, qualem problema requirit, ope ſpatii hyperbolici*, &c. ut ſupra.

In exemplum hujus regulæ loco curvæ K L (in *Fig.* 2.) ſumatur linea recta lineæ N O parallela, & erit linea ω Ψ Φ Ω ea, quæ logarithmica dicitur, cui N O aſymptotos eſt; ideoque & linea E F etiam logarithmica, per punctum M tranſiens, & aſymptoton habens lineæ N O parallelam; propterea quod area M P Φ Ψ hic erit ut P Φ — M Ψ (*a*). Si vero ordinatæ ε η ζ, β α γ ducantur æqualiter diſtantes à puncto M, ordinatæque M Ψ proximæ, erunt ε η, α β æquales quando primum naſcuntur, quoniam ſpatia ε ζ Ψ M, M Ψ γ α tunc æqualia ſunt; ex oſtenſis autem eſt $\epsilon\eta \times \alpha\beta = M\epsilon q$ vel $M\alpha q$, unde $\epsilon\eta = M\epsilon$; & ε η ad $\frac{M\Psi\zeta\eta}{M\Psi}$ ut radius ad ſinum anguli ſub N M Ψ. Quoniam igitur P I ſemper eſt ut ſpatium Ψ M P Φ, erit P I ubique ad $\frac{\Psi MP\Phi}{M\Psi}$ ut radius ad ſinum anguli ſub N M Ψ; &

(*a*) Vid. Barrov. Lection. Geometr. p. 123.

denique limes ordinatarum negativarum ad ſpatium totum comprehenſum a parte $\Psi\,\omega$ lineæ logarithmicæ $\omega\,\Psi\,\Omega$ ab ordinatâ $M\,\Psi$, & ab aſymptoto $M\,O$ ad ordinatam $M\,\Psi$ applicatum ut radius ad ſinum anguli ſub $N\,M\,\Psi$: eſt autem rectangulum ſub $M\,\Psi$ & ſub lineæ logarithmicæ $\omega\,\Psi\,\Omega$ ſubtangente ad ſpatium prædictum etiam ut radius ad ſinum anguli ſub $N\,M\,\Psi$: adeo ut limes ordinatarum negativarum lineæ curvæ $E\,F$ æqualis erit huic ſubtangenti; unde ſi $\Psi\,M$ retro producatur ad δ, ut $M\,\delta$ huic ſubtangenti ſit æqualis, & ducatur $\theta\,\delta\,\lambda$ lineæ $N\,O$ parallela, erit illa curvæ $E\,F$ aſymptotos; erit autem curvæ hujus $E\,F$ ſubtangens lineæ $M\,\delta$ æqualis; propterea quod $M\varepsilon =$ eſt εn. *Unus igitur caſus curvæ logarithmicæ commode invenitur per regulam primam*, &c. ut ſupra.

Hæc autem regula primum oſtendit modum, quo problema ſolvitur.

Regula Secunda.

Deſcribatur curva quæcunque $\kappa\,M\,\mu$ per punctum M tranſiens in *Fig*. 2. vel $\kappa\,n\,c$, $m\,p\,\mu$ in *Fig*. 4. ubi curva invenienda duobus cruribus $e\,M\,F$, $E\,M\,f$ conſtat; ut curvarum $\kappa\,M\,\mu$, & $\kappa\,n\,c$, $m\,p\,\mu$ ordinatæ ut $P\,\nu$, $Q\,\rho$, quæ æqualiter a puncto M principio abſciſſæ diſtant, ſint æquales, ſed a contrariis partibus abſciſſæ poſitæ, ita ut mutato abſciſſæ ſigno ordinatæ ſignum etiam mutetur.

Exponatur porro (in *Fig*. 5.) hyperbola æquilatera $a\,b$ cujus axis tranſverſus $a\,g$, conjugatus $h\,q$, centrum d, aſymptoti $d\,r$, $d\,s$; ſumatur $d\,t = P\,\nu$, & ducatur $t\,v\,w$ ad $h\,q$ perpendicularis, junctâ $d\,w$, ſumatur quoque $d\,x = M\,n$, & ducatur $x\,y$ item rectæ lineæ $h\,q$ perpendicularis, junctâ $d\,y$. Jam ſit curva $K\,L$ (in *Fig*. 2.) vel $K\,k\,L\,l$ (in *Fig*. 4.) talis ut ſpatium

ΠMPW æquale ſit ſpatio adw, ſi curva $\kappa\mu$ per punctum M tranſit, aliter æquale ſpatio $daw - day$; hac enim ratione curvæ K L, & K k L l non deſinent conditionem habere, quæ in regulâ priori requiritur, nempe ut ordinatæ ad æquales diſtantias a puncto M ſint æquales, & ab eâdem abſciſſæ parte poſitæ. Nam area hyperbolica adw affirmativa eſt, quando dt vel $P\nu$ eſt affirmativa, & eadem area negativa eſt, quando dt vel $P\nu$ negativa eſt, quia area tota hyperbolica ab eâdem parte lineæ hq jacet; ideoque area curvarum K L, K k L l ad ordinatam $M\Pi$ terminata ſignum ſuum mutabit, quando abſciſſa M P, magnitudine ſervatâ, ſignum mutat; & curvæ ordinata nec magnitudinem nec ſignum mutabit, mutatione ſigni abſciſſæ. Sit porro ' $adq =$ parallelogrammo $\Gamma\Xi$ in hyperbolâ priori: quo efficietur ut $tw + tv$ ſit ad ad ut $\Gamma\Sigma$ ad $\Gamma\Lambda$ (*a*); ſi igitur $\Gamma\Lambda$ fiat $= ad$, erit $tw + tv = \Gamma\Sigma = P\Phi$. Porro ducantur ordinatæ $\epsilon\eta\zeta$, $\alpha\beta\gamma$ ordinatæ $M\Psi$ proximæ; deinde in *Fig*. 2. ubi curva $\omega\Psi\Omega$ ſimplex eſt, cum $\epsilon\eta$ ſit ad $\alpha\beta$ ut ſpatium $M\Psi\zeta\epsilon$ ad ſpatium $M\Psi\gamma\alpha$, erit $\epsilon\eta = \alpha\beta$; unde & earum utraque $= M\epsilon = M\alpha$. Ideoque $\epsilon\eta$ ad $\frac{M\Psi\zeta\epsilon}{M\Psi}$ ut radius ad ſinum anguli ſub $NM\Psi$, & ubique PI ad $\frac{M\Psi\Phi P}{M\Psi}$ in eâdem ratione. In *figurâ quartâ* ubi curva $\omega\psi\Psi\Omega$ ex duobus cruribus compoſita eſt, $\epsilon\eta$ eſt ad $\alpha\beta$ ut ſpatium $\Psi M\epsilon\zeta$ ad ſpatium $\psi M\alpha\gamma$ ſive ut $M\Psi$ ad $M\psi$, propterea quod $M\epsilon =$ eſt $M\alpha$. Cum igitur neceſſe ſit, ut $\epsilon\eta \times \alpha\beta =$ ſit $M\epsilon q$, ſcilicet ut crura M F, M E in angulo propoſito ſe mutuo interſecent, erit ratio $\epsilon\eta$ ad $M\epsilon$ ſubduplicata rationis $\epsilon\eta$ ad $\alpha\beta$ vel ſubduplicata rationis $M\Psi$ ad $M\psi$:

(*a*) Vid. Philoſ. Tranſact. No. 338. prop. 4.

ideoque sn ad ſpatium $M\Psi\zeta s$ applicatum ad mediam proportionalem inter $M\Psi$, $M\psi$ ut radius ad ſinum anguli ſub $NM\Psi$; & generatim PI ad ſpatium $M\Psi\Phi P$ applicatum ad mediam proportionalem inter $M\Psi$ & $M\psi$ in eâdem ratione. Eſt autem $M\Psi = yx + dx$, & $M\psi = yx - dx$, & ad media eſt proportionalis inter $yx + dx$ & $yx - dx$. Unde utrobique dictis ad, a; dt vel Pr, R; erit $P\Phi = \sqrt{aa + RR} + R$; $R = \frac{1}{2} a \times \overline{\frac{P\Phi}{a} - \frac{a}{\Phi P}}$, & PI ad $\frac{MP\Phi\Psi}{a}$ ut radius ad ſinum anguli ſub $NM\Psi$.

Regula igitur *ſecunda pendet a primâ, & curvam problemati ſatisfacientem ſine ope ſpatii hyperbolici*, &c. ut ſupra. Nam hic ſine ſpatio hyperbolico curva invenitur, cujus quadraturâ problema ſolvitur.

Duæ autem ſunt in hac regulâ formulæ. Formula prior nimirum $P\Phi = \sqrt{aa + RR} + R$, curvarum geometrice rationalium, quæ maxime hic requiruntur, inventioni accommodatur; facile enim eſt ita ſumere quantitatem indeterminatam R, ut curva $\omega\Psi\Phi\Omega$ quadraturam admittat.

Ne caſus magis compoſiti memorentur, ponatur R vel $Pr = cz^{\frac{m}{n}}$, ut m & n numeri ſint impares vel inter ſe primi, vel eorum alter unitas: hac enim ratione curva, cujus ordinata eſt Pr, conditionem habebit in hac regulâ neceſſariam, & erit $P\Phi = \sqrt{aa + RR} + R = \sqrt{aa + ccz^{\frac{2m}{n}}} + cz^{\frac{m}{n}} = z^{\frac{m}{n}} \sqrt{cc + aaz^{\frac{-2m}{n}}} + cz^{\frac{m}{n}}$. Si igitur $\frac{m}{n} + 1$ ſit vel numero $\frac{-2m}{n}$ æqualis, vel ejuſdem multiplex, id eſt, ſi ſumatur $m =$

— 1, & n numero cuilibet impari æqualis; pars ordinatæ $z^{\frac{m}{n}} \sqrt{cc + aaz^{\frac{-2m}{n}}} + cz^{\frac{m}{n}}$ ſub vinculo incluſa, ideoque & ordinata tota quadraturam admittet. (a)

Verbi cauſâ, ponatur $\frac{m}{n} = -\frac{1}{3}$, $c = 1$, & P Φ $= z^{-\frac{1}{3}} \sqrt{1 + aaz^{\frac{2}{3}}} + z^{-\frac{1}{3}}$. Unde erit area M Ψ Φ P $= \frac{\overline{1 + aaz^{\frac{2}{3}}}|^{\frac{3}{2}}}{aa} + \frac{1}{2} z^{\frac{2}{3}}$, & P I $= \overline{\frac{1}{aa} + z^{\frac{2}{3}}}|^{\frac{3}{2}} + \frac{3z^{\frac{2}{3}}}{2a}$, curvaque quæſita hac æquatione comprehendetur $a \times \text{P I}q - 3z^{\frac{2}{3}} \times \text{P I} = \frac{1}{a^5} + \frac{3z^{\frac{2}{3}}}{a^3} + \frac{3z^{\frac{4}{3}}}{4a} + az^2$. In hac æquatione cum $z^{\frac{2}{3}}$ ſignum non mutabit, mutatione ſigni abſciſſæ z; pro eâdem ipſius magnitudine tam negativâ quam affirmativâ P I eandem habebit magnitudinem, & ſub eodem ſigno; unicuique autem magnitudini abſciſſæ z reſpondet & affirmativa & negativa ordinata: adeo ut curva quæſita habebit formam hic appoſitam (in *Fig*. 6.); e tribus conſtans cruribus a b c, d e, d f punctis b, d æqualiter a puncto M diſtantibus; quippe eſt M d = M b $= \frac{1}{a^3}$: quando enim eſt $z = 0$, erit P I$q = \frac{1}{a^6}$; & P I $= \pm \frac{1}{a^3}$.

(a) Vid. in Tract. de quadr. curv. Newton, tab. curv. ſimplicior. quæ quadrari poſſunt.

Hæc autem regulæ hujus formula prior ſecundum exhibet curvas quæſitas inveniendi modum.

In formulâ poſteriori, cum R ſit $= \frac{1}{2} a \times \overline{\frac{P\Phi}{a} - \frac{a}{P\Phi}}$, R vel $P\nu$ ejuſdem magnitudinis manebit, ſed ſignum mutabit, quando abſciſſa magnitudinem ſuam ſigno mutato retinet, ſi $P\Phi$ talis ſumatur, ut mutando abſciſſæ ſignum $\frac{P\Phi}{a}$ convertatur in $\frac{a}{P\Phi}$, & contra ut $\frac{a}{P\Phi}$ convertatur in $\frac{P\Phi}{a}$. Et hæc formula poſterior tertium continet problema ſolvendi modum.

Verbi causâ, ſit $P\Phi = a \times \frac{c-z}{c+z}$, quando z eſt affirmativa, & erit R vel $P\nu$ eodem tempore $= \frac{1}{2} a \times \overline{\frac{c-z}{c+z} - \frac{c+z}{c-z}}$, quando autem z negativa eſt, fiet $P\Phi = a \times \frac{c+z}{c-z}$, & R vel $Q\rho = \frac{1}{2} a \times \overline{\frac{c+z}{c-z} - \frac{c-z}{c+z}}$. Hinc autem R æqualis erit $\frac{\mp 2acz}{cc-zz}$, & $Rzz \mp 2acz = ccR$; ideoque curva $x M \mu$ linea tertii ordinis, imo ſpecies earum quinquageſima nona; propterea quod æquationis $ccRR + aacc = 0$ radices ſunt impoſſibiles (*a*). Linea autem curva hinc invenienda, ſi fiat (in *Fig.* 7.) NM vel MO $= c$, logarithmica eſt, cui recta AB eſt aſymptotos. Cum

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(*a*) Vid. Newton. Enumerat. linear. tert. ordin. ad Fig. 63.

X enim

enim P Φ ſit $= a \times \frac{c - z}{c + z}$, erit eadem $= \frac{ac}{c + z} - \frac{az}{c + z}$. Si igitur (in *Fig*. 8.) in rectâ lineâ quacunque $\alpha \varepsilon$ ſumatur $\alpha \kappa =$ O M $= c$, & ei ad perpendiculum erigantur $\alpha \beta$, $\kappa \mu$ quarum $\kappa \mu$ ſit $= a$, & ſi aſymptotis $\alpha \varepsilon$, $\alpha \beta$ per punctum μ deſcribatur hyperbola $\zeta \eta$, & ſumptâ $\kappa \nu =$ M P $= z$, ducatur $\nu \rho$ aſymptoto $\alpha \beta$ parallela ; parti $\frac{ac}{c + z}$ ordinatæ P Φ reſpondet area, quæ erit ad aream $\kappa \mu \rho \nu$ ut ſinus anguli ſub N P I ad radium, & alteri parti $\frac{az}{c + z}$ ejuſdem ordinatæ reſpondet area, quæ erit ad $a \times \kappa \nu - \kappa \mu \varrho \nu$ in eâdem ratione (*a*). Unde P I, quæ eſt ad $\frac{\text{M P} \Phi \Psi}{a}$ ut radius ad ſinum anguli ſub N M Ψ, erit $= \frac{2 \kappa \mu \rho \nu}{a} - \kappa \nu$. Si igitur ſumatur O ϛ $=$ O M, & ducatur ϛ M ordinatæ P I retro productæ occurrens in χ, ut ſit P $\chi =$ P M $= \kappa \nu$, erit χ I $= \frac{2 \kappa \mu \rho \nu}{a}$: ideoque linea M I logarithmica, cui A B aſymptotos eſt, & ϛ M ordinatim applicata, efficiens cum aſymptoto A B angulum ſub A ϛ M verſus contingentem æqualem dimidio anguli ſub A O N. *Alter* igitur *hujus lineæ caſus deducitur*, &c. ut ſupra.

Magis generatim, ſi r ordinatam curvæ alicujus denotat, quæ inſtar curvarum κ M μ, & κ n c, m p μ ad abſciſſam N O deſcripta ordinatas habeat æquales, quæ

(*a*) Vid. Newton. de quadr. curv. tab. curv. ſimpl. quæ cum circ. & hyperb. compar. poſſunt, form. prim.

æqualiter diſtant a puncto M, ſed a contrariis partibus abſciſſæ poſitas, poni poteſt ordinata $P\Phi = a \times$

$$\left[\frac{\overline{b \mp cr + drr \mp er^3 + \&c} \times \overline{f \mp gr + \&c}\,|^{\lambda} \times \overline{h \mp kr + lrr \mp \&c}\,|^{\mu}\,|^{\nu}}{\overline{b \pm cr + drr \pm er^3 + \&c} \times \overline{f \pm gr + \&c}\,|^{\lambda} \times \overline{b \pm kr + lrr \pm \&c}\,|^{\mu}\,|^{\nu}}\right]^{\pi}$$

Ex priori hujus regulæ ſecundæ formulâ deducitur quoque theorema, cujus ſupra fit mentio, ad inveniendas curvas tam rationales quam irrationales utile, quod quartus erit modus problema ſolvendi.

Theorema.

Quoniam eſt $P\Phi = \sqrt{aa + RR} + R$, & $R = Pr$, manifeſtum eſt, ſi $\frac{R}{a}$ vel $\frac{Pr}{a}$ ſit ut fluxio ordinatæ, quæ abſciſſæ ſuæ ad perpendiculum inſiſtat, alicujus curvæ, erit $\frac{\sqrt{aa + RR}}{a}$, ut ejuſdem curvæ fluxio; curvæ autem hujus ordinata æqualis erit areæ curvæ $x\mu$ ad a applicatæ, ſi angulus ſub M P, rectus ſit, & cum area curvarum (in *Fig*, 2, 4.) $xM\mu$, & x n c, m p μ eodem ſigno afficiatur, tam quando abſciſſa eſt affirmativa, quam quando eſt eadem negativa, quoniam areæ ad diverſas abſciſſæ partes in illis diverſis caſibus jacent; & præterea cum eiſdem abſciſſæ magnitudinibus areæ æquales reſpondeant, curvæ, quales problema requirit, inveniri poſſunt curvarum ope, quarum ordinatæ ad eaſdem abſciſſæ magnitudines æquales ſint, & ab eâdem abſciſſæ parte poſitæ, ſi modo ordinatæ inſiſtunt abſciſſæ ad perpendiculum.

Deſcripta ſit ejuſmodi curva n o, quæ tangat abſciſſam in puncto M (ut in *Fig*. 9.) ſi evaneſcat, quando abſciſſa eſt = 0, fluens quantitas fluxioni longitudinis curvæ n o reſpondens; aliter, quæ habeat ordinatam primam M m (ut in *Fig*. 10.) æqualem magnitudini fluentis iſtius quantitatis, quando abſciſſa eſt = o. Erigantur ordinatæ P p, Q q; deinde erit P I curvæ quæſitæ ordinata, quæ ab alterâ parte puncti M jacet, vel = M p + P p, vel = M m p + P p; ordinata autem Q R, quæ ab alterâ parte puncti M cadit, vel = M q − Q q, vel = M m q − Q q.

Obſervandum autem eſt hoc theorema aliquando partem duntaxat curvæ quæſitæ deſcribere.

Ex ratione autem, qua hoc theorema inveſtigatur, manifeſtum eſt duo crura curvæ hic deſcriptæ ejuſdem lineæ eſſe partes: nimirum utriuſque naturam eâdem æquatione definiri. Hanc autem curvam in ſitu inverſo diſpoſitam ſe interſecare in angulo æquali angulo ſub N O B inde manifeſtum eſt, quod rectangulum ſub fluxione P I & ſub fluxione Q R, ordinatarum ſcilicet æqualiter a puncto M diſtantium, æquale eſt quadrato fluxionis abſciſſæ: ſi enim curvæ n o ordinatæ w r, x t applicentur ordinatis Q q, P p proximæ, & P x, Q w ſint æquales, & ducantur r s, t v abſciſſæ N O parallelæ, erunt triangula p t v, q r s rectangula ſimilia & æqualia: *in omni* autem *triangulo rectangulo quadratum ab alterutro latere angulo recto adjacenti æquale eſt rectangulo ſub ſummâ alterius lateris angulo recto adjacentis laterisque angulo ei ſubtendentis, & ſub differentiâ eorundem laterum.* Igitur $\overline{tv}^2 = \overline{Px}^2 = \overline{pt + pv} \times \overline{pt - pv} = \overline{pt + pv} \times \overline{qr - qs}$: eſt autem ultima ratio P x ad p t + p v ea, quam fluxio abſciſſæ habet ad fluxionem ordinatæ P I; & ratio P x vel Q w ad q r − q s ea, quam fluxio abſciſſæ habet ad ordinatæ Q R fluxionem. Unde conſtat propoſitum. *Regula* igitur *ſecunda theorema*, &c. ut ſupra.

Jam ſi n o ſit circuli circumferentia, linea E F cyclois erit, quando angulus ſub N O B vel ſub N P I rectus eſt. Porro ſi curvæ n o longitudo cum rectâ conferri poteſt, quarum curvarum ſimpliciſſima eſt parabola ſemicubica, curva inventa rationalis erit. Speciatim parabola ſemicubica, ſi rite diſponatur, ejus curvæ partem dimidiam exhibebit, quam in exemplum formulæ prioris regulæ ſecundæ delineavimus; ſcilicet (in *Fig.* 6.) crus d e, partemque inferiorem b c cruris a b c. Reliquæ autem illius partes deſcribi poſſunt, ſi retro producatur ordinata I P donec pars producta æqualis ſit M m p — P p, & producatur R Q ab altero abſciſſæ latere, donec pars producta æqualis ſit M m q + Q q

Nunc tranſeundum eſt ad regulam tertiam, quæ etiam curvas geometrice rationales largitur.

Regula Tertia.

Regula hæc tertia duos quoque complectitur problema ſolvendi modos a prioris regulæ formulis propoſitione nonâ tractatus de quadraturâ curvarum *Newtoni* derivatos.

Propoſitione iſtâ ad formulam regulæ præcedentis priorem adhibitâ invenitur area curvæ, cujus abſciſſa eſt z, & ordinata $\sqrt{aa + RR} + R$, æqualis areæ curvæ, cujus abſciſſa eſt R & ordinata $\frac{\dot{z}}{\dot{R}}\sqrt{aa + RR} + \frac{\dot{z}}{\dot{R}}R$. Hinc autem quinto modo ſolvitur problema.

Verbi causâ, ut exemplum generale, quod antea (a) exhibui, inveſtigetur, poſitis $MP = z$, & $P\nu = R$, ut prius, fiat $\frac{\dot{z}}{\dot{R}} = R^{\frac{m-n}{n}} \times \overline{c + dR^2}$, & erit $z = R^{\frac{m}{n}}$

(a) In Act. Erud. Menſ. April. 1721.

$\overline{\times\frac{n}{m}c+\frac{n}{m+2n}}dR^2$; ſint autem m & n numeri impares vel inter ſe primi vel eorum alter unitas; ut ſigna abſciſſæ z & ordinatæ R ſimul mutentur, ſicut in regulâ priori requiritur; jam erit ordinata $\frac{z}{\dot{R}}\sqrt{aa+RR}+\frac{z}{\dot{R}}R=R^{\frac{m-n}{n}}\times\overline{c+dR^2}\times\sqrt{aa+RR}+R^{\frac{m}{n}}\times\overline{c+dR^2}$; area igitur curvæ, cujus abſciſſa eſt z & ordinata $\sqrt{aa+RR}+R$, æqualis erit areæ curvæ, cujus abſciſſa eſt R & ordinata $R^{\frac{m-n}{n}}\times\overline{c+dR^2}\times\sqrt{aa+RR}+R^{\frac{m}{n}}\times\overline{c+dR^2}$, ſi modo hæc poſterior ordinata cum abſciſsâ ſuâ angulum contineat æqualem angulo ſub N M Ψ; unde hujus poſterioris curvæ quadraturâ linea exhibetur problemati ſatisfaciens. Erit autem hæc linea curva geometrice irrationalis, niſi m & n certos quoſdam numeros deſignant, vel certa quædam ſit relatio inter coefficientes c, d; hæ autem conditiones ratione ſequenti inveniuntur. Erit (*a*) area curvæ, cujus abſciſſa R & ordinata $R^{\frac{m}{n}}\times\overline{c+dR^2}+R^{\frac{m-n}{n}}\times\overline{c+dR^2}\times\sqrt{aa+RR}$, ad $R^{\frac{m+n}{n}}\times\overline{\frac{n}{m+n}c+\frac{n}{m+3n}dR^2}+R^{\frac{m}{n}}\times\overline{aa+RR}\big|^{\frac{3}{2}}$ $\times\overline{\frac{n}{maa}c+\frac{d-\left|\frac{m+3n}{maa}\times c\right.}{\frac{m+2n}{n}aa}R^2+\&c.}$ ut ſinus an-

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(*a*) Per prop. quint. quadr. curv. Newton.

guli ſub NMΨ ad radium. Hæc autem ſeries terminabitur, & quadraturam finitam dabit, ſi n ſit unitas & m numerus negativus ternario major, vel ſi ultimus terminorum hic ſcriptorum ſit nihilo æqualis, id eſt, ſi ſit $d = \frac{m + 3n}{maa} c$, vel ſi ſit $d = 0, n = 1, \& m = -3$. Et hic quidem ultimus caſus curvam exhibet, quæ theoremate præcedenti a parabolâ ſemicubicâ invenitur.

Magis generatim ponere licet $\frac{\dot{z}}{\dot{R}} = R^{\frac{m-n}{n}} \times \overline{c + dR^2}$ $\overline{+ eR^4 + \&c \ldots + fR^p}$, ubi p numerum quemcunque parem denotat; unde fiat $z = R^{\frac{m}{n}} \times \overline{\frac{n}{m} c +}$ $\overline{\frac{n}{m+2n} dR^2 + \frac{n}{m+4n} eR^4 \ldots + \frac{n}{m+pn} fR^p}$, & curvæ $\omega\Omega$ ordinata $= R^{\frac{m-n}{n}} \times \overline{c + dR^2 + eR^4}$ $\overline{\ldots + fR^p} \times \sqrt{aa + RR} + R$. Hinc (*a*) ſi n ſit unitas & m numerus negativus numero $p + 1$ major, curva dabitur geometrice rationalis, vel ſi certa quædam relatio ſit inter coefficientes $c, d, e, \&c \ldots f$, quæ relatio facile invenitur ut antea.

Porro ad alteram regulæ ſecundæ formulam adhibendo propoſitionem nonam memoratam libri de quadraturâ curvarum, ſextus oritur problema ſolvendi modus.

Literâ r denotante ut ſupra, fieri poteſt ordinata $P\Phi = a \times \frac{b + cr + drr + \&c}{b - cr + drr - \&c}$, area curvæ, cujus abſciſſa eſt z & ordinata $P\Phi$, æqualis erit areæ curvæ,

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(*a*) Per prop. proxim. citat.

cujus abſciſſa eſt r & ordinata $a \times \frac{\dot{z}}{\dot{r}} \times \frac{b + cr + drr + \&c}{b - cr + drr - \&c}$

Ponatur igitur $\frac{\dot{z}}{\dot{r}} = r^{\frac{m-n}{n}} \times \overline{b + cr + drr + \&c} \times \overline{b - cr + drr - \&c}|^{p} = r^{\frac{m-n}{n}} \times \overline{bb + 2bd - cc \times rr + ddr^4 + \&c}|^{p}$, & curva, cujus ordinata eſt r conditionem hic neceſſariam habebit. Erit enim $z = r^{\frac{m}{n}} \times \overline{bb + 2bd - cc \times rr + ddr^4 + \&c}|^{p+1} \times \overline{A + Brr + Cr^4 + \&c}$, cujus ſeriei coefficientes A, B, C &c dantur per propoſitionem quintam Tractatus de Quadraturâ Curvarum. Manifeſtum autem eſt nec terminos hujus ſeriei nec quantitatem $\overline{bb + 2bd - cc \times rr + ddr^4 + \&c}|^{p+1}$ ſigna ſua mutare mutatione ſigni quantitatis r; quantitas autem $r^{\frac{m}{n}}$, ſi m, n numeri ſint impares, ſignum mutabit, quando ipſa r ſignum mutat; ideoque ordinata r & abſciſſa z ſigna ſimul mutabunt. Ordinata autem $a \times \frac{\dot{z}}{\dot{r}} \times \frac{b + cr + drr + \&c}{b - cr + drr - \&c} =$ erit $ar^{\frac{m-n}{n}} \times \overline{bb + 2bd - cc \times rr + ddr^4 + \&c}|^{p-1} \times \overline{b + cr + drr + \&c}|^{2}$. Et hinc facile inveniri poſſunt curvæ rationales,

Pro exemplo ſimplici ponatur $p = 1 = m = n$, d, &c $= 0$: unde erit $\frac{\dot{z}}{\dot{r}} = bb - ccrr$, & $z = bbr - \frac{1}{3}ccr^3$. Ordinata autem curvæ metiendæ $= abb + 2abcr + accrr$; ejuſdem igitur area eſt ad $abbr + abcrr + \frac{1}{3}accr^3$ ut ſinus anguli ſub N M Ψ ad radium; ideoque erit P I $= bbr + bcrr$

$+\frac{1}{3}ccr^3$. Hinc autem invenitur parabolam ſemicubicam problemati ſatisfacere, quam ita deſcribere oportet. Datâ (in *Fig.* 11.) lineâ rectâ A B, & in eâ puncto C, una cum lineâ rectâ C D angulum ſub B C D cum lineâ C B conſtituente æqualem angulo, in quo curva ſe interſecare requiritur. Ducatur ad libitum H G I ad C D parallela, ſumaturque in eâ G H $=$ 2 C G; deinde dividatur angulus ſub A C D in duas partes æquales lineâ rectâ C E, & denique ad diametrum H I & verticem H deſcribatur parabola ſemicubica K H L, quæ tranſeat per punctum C, ita ut C E ordinatim applicetur ad diametrum H I. Hæc parabola ad eandem lineam ſimiliter applicata, ſed ſitu inverſo, ſe interſecabit in angulo æquali angulo ſub B C D.

Si placet curvas hac regulâ inventas theoremate præcedente conſtruere, ex iis, quæ hic tradita ſunt, curva huic negotio apta inveniri poteſt; erit enim curvæ illius ordinata æqualis areæ curvæ $x\mu$ ad a applicatæ, quando angulus ſub M P ν rectus eſt. Verbi causâ, hujus areæ fluxio, nimirum $P\nu\times\dot{z}$ in exemplo ſecundo prioris partis hujus regulæ erit $=RR\times R^{\frac{m-n}{n}}\overline{\times c+dR^2+eR^4\ldots+fR^p}=RR^{\frac{m}{n}}\times\overline{c+dR^2+eR^4\ldots+fR^p}$; ideoque curvæ hic requiſitæ ordinata erit $=\frac{1}{a}R^{\frac{m+n}{n}}\times\overline{\frac{n}{m+n}c+\frac{n}{m+3n}dR^2+\frac{n}{m+5n}eR^4\ldots+\frac{n}{\overline{m+p+1}\times n}fR^p}$.

In exemplo poſterioris partis hujus regulæ erit R $(=\frac{1}{2}a\times\overline{\frac{P\Phi}{a}-\frac{a}{P\Phi}})=\frac{2bcr+2cdr^3+\&c}{\overline{bb+2bd-cc}\times rr+ddr^4+\&c}$;

ideoque $R \times z = r r^{\frac{m}{n}} \times \overline{2bc + 2cdr^2 + \&c} \times$ $\overline{bb + \overline{2bd - cc} \times rr + ddr^4 + \&c})^{p-1}$; hæc igitur eſt fluxio ordinatæ curvæ quæſitæ.

Si ſit $m = 1 = n = p$, d, &c $= 0$, erit $R \times \dot{z} = 2bcr\dot{r}$, & ordinata curvæ quæſitæ $= bcrr$; quoniam igitur $z =$ erit $bbr - \frac{1}{3}ccr^3$, erit curva quæſita in hoc caſu parabola divergens cum nodo, quæ definitur hac æquatione $3ezz = y^3 - 2eyy + eey$ (*a*). Et hac curvâ deſcribetur parabola ſemicubica ſupra inventa.

Verbi causâ, ad rectam lineam (in *Fig*. 12.) A B ducatur perpendicularis C D, & ad illam ut axim deſcribatur ejuſmodi parabola divergens F E C E G. Deinde ducatur ad libitum H I angulum quemcunque datum cum rectâ A B conſtituens, & ducatur H K L M ad C D parallela; deinde ſumatur H N = H K + arc. C K, H O = H L + arc. C K L, & ab alterâ parte puncti H, H P = C E M — H M; & curva hac ratione deſcripta parabola ſemicubica erit.

Hinc apparet quomodo curvæ, quarum inveſtigationi regula hæc tertia aptatur, theoremate præcedenti conſtrui poſſunt, poſtquam earum formæ cognoſcuntur, ſed hæ curvarum formæ, a quibus rationales deriventur, regulâ hac tertiâ optime inveniuntur.

Hæ ſunt tres regulæ, quarum ſupra ſit mentio. Ultima ſententia, quæ ſub notis fictis celata fuit, exemplo ſequenti illuſtrari poteſt. Sit y vel $= a + bx + \sqrt{c + 2dx + ex^2}$ vel $= \frac{a + bx + cxx}{d + ex}$, quæ

(*a*) Vid. Enumerat. linear. tert. ord. Fig. 73.

duæ æquationes omnes complectuntur ſectiones conicas. Inde vero inveniemus $\frac{\ddot{y}}{\dot{y}}$ vel $=$

$$\frac{ec-dd}{\overline{d+ex\mp b\sqrt{c+2dx+exx}}\times\overline{c+2dx+exx}}\dot{z},$$

vel $=$

$$\frac{2cdd+2aee-2bde}{\overline{d+ex}\times\overline{bd-ae+2cdx+cexx}}z;$$

quæ æquationes oſtendunt in nullâ ſectione conicâ, quomodocunque diſponatur, quantitatem $\frac{\ddot{y}}{\dot{y}}$ conditionem habere, quam hoc problema requirit; ideoque nullam ſectionem conicam problemati ſatisfacere. Quod comprobari etiam poteſt examinando rectangulum ſub fluxionibus primis ordinatarum æqualiter ad diverſas partes a principio abſciſſæ diſtantium.

Hinc autem cognoſcitur nullam lineam curvam geometrice rationalem problema ſolvere, quæ parabolâ ſemicubicâ ſit ſimplicior.

Si vero talis inter quantitates a, b, c, d, e relatio ſtatui potuiſſet ut $\frac{\ddot{y}}{\dot{y}}$ conditionem in hoc problemate neceſſariam obtineret, nempe ut quantitas, quæ in $\dot{z}$ ducitur, eadem eſſe potuiſſet, & ſub eodem ſigno, pro eâdem magnitudine tam negativâ quam affirmativâ abſciſſæ z, quo eveniret ut $\frac{\ddot{y}}{\dot{y}}$ foret $= -\frac{\ddot{v}}{\dot{v}}$, ſi abſciſſa in oppoſitas partes a ſuo principio, æqualibuſque momentis fluere ponitur: tum profecto ſectio conica hinc determinanda vel problema ſolveret, vel ſectionis

proble-

problemati ſatisfacientis ordinata ad ordinatam hujus rationem haberet datam.

Jam vero his regulis alias aliquot, quas ab amico accepi, ad problema ſolvendum adjungam.

Regula Quarta.

Iiſdem poſitis ac in regulâ primâ, ſit (in *Fig.* 13.) N O ad A B, C D perpendicularis; ſint P I, Q R ordinatæ æqualiter a puncto M diſtantes, & ſit curva G H per punctum I ducta ſimilis & æqualis curvæ f E F. Ordinatis P I, Q R parallelæ & proximæ ducantur π j l, δ r, & lineæ rectæ I k, R s lineæ N O parallelæ. Angulus ſub s R r = eſt angulo ſub k I l; unde anguli ſub j I k, s R r ſimul ſumpti æquales ſunt angulo dato ſub j I l; & quantum angulus ſub j I k dimidium anguli ſub j I l ſuperat, tantum angulus ſub s R r ab eodem dimidio deficit. Si igitur (in *Fig.* 14.) radio quolibet m n circuli arcus n o deſcribatur, & ſumatur angulus ſub n m p = dimidio anguli dati ſub j I l, angulus ſub n m q = angulo ſub j I k, & angulus ſub n m t = ei ſub s R r, ſectores q m p, p m t erunt æquales. Poſitâ autem I k = R s = 1, erit j k ut tangens anguli ſub j I k vel anguli ſub n m q, & r s erit ut tangens anguli ſub s R r vel anguli ſub n m t; ideoque & fluxio ordinatæ P I erit ut tangens anguli ſub n m q, nimirum ut n v; & fluxio ordinatæ Q R ut tangens anguli ſub n m t, nimirum ut n w; curvæ igitur Ψ Φ Ω, cujus areæ ordinata P I proportionalis eſt, ordinata Γ Φ poteſt eſſe æqualis tangenti n v, & ordinata Q χ ab alterâ parte puncti M = n w. Quoniam autem ſectores p m q, p m t ſunt æquales, conſtitui poteſt ſector p m q æqualis areæ M Π W P curvæ cujuſcunque K L conditionem habentis in regulâ primâ indicatam; & ſector que ſi ducatur linea recta $\varepsilon\eta\zeta$ lineæ $M\Psi$ parallela & proxima; cum angulus ſub $\varepsilon M \eta$ = ſit dimidio anguli ſub j I l, vel angulo ſub n m z, erunt triangula $\varepsilon M\eta$, n m z ſimilia, & prima ratio $\varepsilon\,\eta$ ad εM eadem cum ratione z n ad n m; ideoque $\varepsilon\eta = \frac{M\Psi\zeta\varepsilon}{nm}$, propterea quod εM = eſt $\frac{M\Psi\zeta\varepsilon}{M\Psi}$, & $M\Psi$ = n z. Hic autem habetur ſeptimus modus, quo problema ſolvi poteſt.

Si loco curvæ K L linea recta ſumatur, quicunque ſit angulus ſub n m z eadem deſcribetur curva; adeo ut hac ratione invenitur una eademque curva, quæ diverſis ſitibus in angulo quocunque dato problema ſolvit. Hæc autem curva a circuli & hyperbolæ quadraturâ dependet; ſi enim ducantur m τ, n σ ad m n perpendiculares, quarum n σ = ſit m n, & aſymptotis m n, m τ hyperbola $\omega\,\sigma\,\psi$ deſcribatur, & deinde q $\varphi\,\nu$, p $\theta\,\varsigma$ ducantur lineis m τ, n σ parallelæ; quando M P = eſt arcui circuli p q, erit ordinata $PI = \frac{\theta\varphi\upsilon\varsigma}{mn}$, ſi m n = ſit 2 $M\Pi$ (a).

Regula Quinta.

Deſcribatur (in *Fig.* 13.) curva $\varkappa M \mu$ ut in regulâ ſecundâ, & (in *Fig.* 15.) radio = m n deſcribatur ſemicirculus $\alpha\beta\gamma$, cujus centrum δ, ſit autem $\delta\beta$ diametro $\alpha\gamma$ perpendicularis. Sumatur $\delta\varepsilon = P\nu$, ducatur $\varepsilon\zeta$ ad $\delta\beta$ parallela, jungaturque $\delta\zeta$. Deinde ſit curva K L ejus naturæ, ut area $M\Pi W P$ ſemper æqualis ſit ſectori $\beta\delta\zeta$. In circuli arcu (*Fig.* 14.) n o ductis pν ſinu arcus p q, & p θ ſinu arcus n p, producatur m p ad z, ducaturque zξ ad pν parallela. Porro dictis mn $=$ mp, a; mθ, b; n z, c; pν, R; nv, y; erit ut mp : pν :: mz : zξ, ſed ut mθ : mn (mp) :: mn : mz; ex æquo igitur ut mθ (b) : pν(R) :: mn : zξ :: mv ($\sqrt{aa+yy}$) : zv ($y-c$) unde $by - bc = R\sqrt{aa+yy}$, & denique $y =$ nv $=$ PΦ $= \frac{bbc + aR\sqrt{aa-RR}}{bb-RR}$.

(a) Vid. Barrov. lect. geometr. pag. 110.

Hinc autem modo octavo ſolvitur problema.

Regula Sexta.

Per propoſitionem nonam Tractatus de Quadraturâ Curvarum area curvæ, cujus abſciſſa eſt z & ordinata $\frac{bbc+aR\sqrt{aa-RR}}{bb-RR}$, æqualis eſt areæ curvæ, cujus abſciſſa eſt R & ordinata $\frac{\dot{z}}{\dot{R}} \times \frac{bbc+aR\sqrt{aa-RR}}{bb-RR}$ Unde habetur modus nonus problema ſolvendi.

Literæ m & n eadem denotent, ac in regulâ tertiâ, & fiat $\frac{\dot{z}}{\dot{R}} = R^{\frac{m-n}{n}} \times \overline{bb-RR}|^{p}$, & ordinata $\frac{\dot{z}}{\dot{R}} \times \frac{bbc+aR\sqrt{aa-RR}}{bb-RR}$ fiet $= bbcR^{\frac{m-n}{n}} \times \overline{bb-RR}|^{p-1} + aR^{\frac{m}{n}} \times \overline{bb-RR}|^{p-1} \times \sqrt{aa-RR}$. Unde ſi n unitatem denotet, & p numerum quemcunque integrum & affirmativum, curva geometrice rationalis invenietur.

Regula Septima.

Ducatur (in *Fig.* 15.) $\beta\lambda$ ſemicirculum $\alpha\beta\gamma$ contingens in β, & producatur $\epsilon\zeta$ ad μ, ductâ $\delta\nu\mu$. Sit autem curva (in *Fig.* 13.) K L ejus naturæ, ut area M Π W P = ſit ſectori $\delta\beta\nu$. Dictis igitur m n, a; n z, c; & tangente arcus p q, R; erit n v = P Φ = $\frac{aac+aaR}{aa-cR}$. Et hic eſt decimus problema ſolvendi modus.

Quando angulus interſectionis rectus eſt, & $c=a$, hæc regula ſub formulâ poſteriori regulæ ſecundæ comprehenditur.

Item ſi loco $\varkappa M\mu$ linea recta ſumatur, quicunque ſit interſectionis angulus, caſus ille curvæ logarithmicæ invenietur, quem in regulâ ſecundâ tradidimus.

Regula Octava.

Ut antea, eſt area curvæ, cujus abſciſſa z & ordinata $\frac{aac+aaR}{aa-cR}$, æqualis areæ curvæ, cujus abſciſſa eſt R & ordinata $\frac{z}{R}\times\frac{aac+aaR}{aa-cR}$. Hic autem eſt undecimus modus problema exequendi.

Literis m, n iiſdem denotantibus, ut antea, ſit $\frac{\dot{z}}{\dot{R}}=$ $R^{\frac{m-n}{n}}\times\overline{a^4-ccRR}|^{p}$; & ordinata $\frac{\dot{z}}{\dot{R}}\times\frac{aac+aaR}{aa-cR}$ fiet $=R^{\frac{m-n}{n}}\times\overline{a^4c+\overline{a^4+a^2cc}\times R+aacRR}$ $\times a^4-$

$\overline{\times a^4 - ccRR}|^{p=1}$: quæ formula curvas geometrice rationales facile præbet.

Si ſit $m = 1 = n = p$; eadem parabola ſemicubica atque ex regulâ tertiâ invenietur.

Regula nona.

Si (in *Fig.* 16.) NO ad lineas AB, CD perpendicularis ſit, & ducatur curva KL, cujus ordinatæ PW, QX, quæ æqualiter a puncto M diſtant, æquales ſint, & ab eâdem abſciſſæ parte poſitæ; radio ordinatæ PW æquali deſcribatur circuli ſegmentum a b c, quæ angulum comprehendat angulo æqualem, in quo curva ſe ipſam ſecare requiritur. Ducatur autem & alia curva $\varkappa$ M μ cujus ordinatæ Pν, Qρ æqualiter a puncto M diſtantes ſint æquales & a contrariis partibus abſciſſæ NO poſitæ. Deinde ſumptâ Mf = Pν ductâque f h lineæ NO ad perpendiculum, junctâque c h, manifeſtum eſt, ſi curva quæſita EF ejus ſit naturæ, ut contingens in puncto I ſemper ſit parallela lineæ c h, propoſito ſatisfaciet. Nam cum ſit WP = QX, idem circuli ſegmentum ordinatis PW, QX convenit; adeo ut ſi ſumatur Mg = Qρ, ducatur g k ad NO perpendicularis, & jungatur c k, linea recta contingens curvam quæſitam EF in puncto R parallela erit lineæ c k. Quoniam igitur Qρ = eſt Pν, ideoque Mg = M f in ſitu hujus curvæ EF inverſo, & quando punctum R in punctum I cadit, contingens in puncto R lineæ puncta a, h conjungenti parallela erit, & cum contingente in puncto I angulum conſtituet æqualem ei ſub a h c, nimirum angulo in ſegmento a b c comprehenſo. Invenitur igitur hujuſmodi curva, ſi fiat ut $\dot{y} : \dot{z} :: fh : fc$. Quamobrem ſi pro PW ponatur m; pro a M

= M c ponatur n; pro intervallo inter punctum M & centrum ſegmenti ponatur p; & pro P ν = M f, q; habebimus $\dot{y} : \dot{z} :: \sqrt{mm - qq} \pm p : n + q$, & $\dot{y} = \frac{\sqrt{mm - qq} \pm p}{n + q} \dot{z}$. Dantur autem rationes inter m, n, p ob datum ſegmenti a b c angulum, & invenietur y vel P I metiendo curvam, cujus abſciſſa eſt z & ordinata $\frac{\sqrt{mm - qq} \pm p}{n + q}$. Hic autem exhibetur duodecimus modus problema tractandi.

Si angulus ſub a h c ſit rectus, erit $p = 0$, $n = m$, & ordinata curvæ metiendæ $\sqrt{\frac{m - q}{m + q}}$. Quam profecto ordinatam problemati ſatisfacere, intelligi quoque poteſt ex poſteriori regulæ ſecundæ formulâ.

Si loco linearum curvarum K L, κ M μ rectæ ſumantur, quando angulus ſub a h c rectus eſt, erit curva E F cyclois; quæ facile determinatur formâ undecimâ tabulæ curvarum ſimpliciorum, quæ cum circulo & hyperbolâ comparari poſſunt in Tractatu de Quadraturâ Curvarum *Newtoni*.

Regula Decima.

Porro area curvæ, cujus abſciſſa eſt z & ordinata $\frac{\sqrt{mm - qq} \pm p}{n + q}$, æqualis eſt tum areæ curvæ, cujus abſciſſa eſt m & ordinata $\frac{\dot{z}}{\dot{m}} \times \frac{\sqrt{mm - qq} \pm p}{n + q}$; tum areæ curvæ, cujus abſciſſa eſt q & ordinata $\frac{\dot{z}}{\dot{q}} \times \frac{\sqrt{mm - qq} \pm p}{n + q}$. Unde habentur duo alii modi, quibus problema ſolvi poteſt; quorum poſteriori, ratione ſequenti, curvæ geometrice rationales inveniri poſſunt.

Sint δ, ε numeri impares, n numerus par, & ponatur $\frac{\dot{z}}{\dot{q}} = q^{\frac{\delta-\varepsilon}{\varepsilon}} \times \overline{n^n - q^n}$, item $m = 1 + \frac{1}{4}qq$. Unde erit $z =$ areæ curvæ, cujus abſciſſa eſt q & ordinata $q^{\frac{\delta-\varepsilon}{\varepsilon}} \times \overline{n^n - q^n}$, & ordinata $\frac{\dot{z}}{\dot{q}} \times \frac{\sqrt{\overline{mm-qq}} \pm p}{n+q}$ fiet $= \overline{n^{n-1} - n^{n-2}q + n^{n-3}qq + \&c \ldots - q^{n-1}}$ $\times \overline{1 - \frac{1}{4}qq \pm p}$.

His quatuordecim diverſis modis generalibus amicus meus problematis ſolutionem abſolvit. Demonſtrationes autem illius ex compoſitione uſus in hoc problemate curvarum Geometris notarum ſic ſe habent.

De Caſu primo Linearum logarithmicarum.

Sit (in *Fig.* 17.) A B linea logarithmica aſymptoton habens C D; eique ordinatim applicetur E F, quæ ſit ſubtangenti logarithmicæ æqualis. Ad lineam rectam E F & ad quodcunque in eâ punctum I conſtituatur alia linea logarithmica G H I priori ſimilis & æqualis, ſed ſitu inverſo diſpoſita. Deinde ſi contingentes H L, H M ducantur, dico angulum ſub L H M angulo ſub C E F eſſe æqualem.

Ordinatim applicetur H N, fiat E O = E N, ordinatim applicetur O P, & ducatur contingens P Q. Puncta P & H æqualiter diſtant a rectâ E I, unde punctum P in curvâ A B puncto H in curvâ G I reſpondet, & angulus ſub O P Q = eſt angulo ſub N H M, propterea quod curvæ A B, G I ſimiles ſunt & æquales. Quoniam vero curva A B eſt logarithmica & E N, E O æquales, erit $NH \times OP = EF\,q$. Eſt autem $EF = NL = OQ$, unde ut $NH : EF\ (NL) :: EF\ (OQ) : OP$. Cum igitur anguli ſub HNL, QOP ſint æquales, triangula HNL, QOP ſunt ſimilia, & angulus ſub QPO, qui æqualis eſt angulo ſub NHM, æqualis quoque erit angulo ſub NLH. Unde anguli ſub NHM & ſub NLH æquales erunt, & angulus ſub LHM angulo ſub CNH ſive angulo ſub CEF æqualis. Q. E. D.

De Caſu altero Linearum Logarithmicarum.

Sint (in *Fig.* 18.) AB, CD duæ lineæ rectæ parallelæ, intra quas quælibet alia linea recta EF ducatur. Ad aſymptoton AB deſcribatur linea logarithmica GH, cujus ſubtangens ſit æqualis lineæ EF, & ordinatim applicatæ comprehendant cum aſymptoto angulos verſus contingentes æquales parti dimidiæ anguli ſub AEF. Quibus poſitis, ſi ad aſymptoton CD alia deſcribatur linea logarithmica ILM priori ſimilis & æqualis, & ſi ducantur contingentes LN, LO, dico angulum ſub OLN angulo ſub BEF eſſe æqualem.

Ducatur NP, ut angulus ſub ANP angulo ſub AEF ſit æqualis, & erit $NP = EF$. Sumatur NQ lineæ EF ſive ſubtangenti lineæ logarithmicæ æqualis, jungaturque QL. Quoniam igitur QL punctum Q conjungit cum puncto contactus L, QL ordinatim ad aſymptoton AB applicabitur, ideoque angulus ſub LQN verſus contingentem LN æqualis erit parti dimidiæ anguli ſub AEF vel anguli ſub ANP; eſt autem $NP = EF = NQ$, quoniam igitur NP, NQ ſunt æquales, & angulus ſub LQN æqualis dimidio anguli ſub ANP, recta QL producta tranſibit per P efficiens triangulum PNQ iſoſceles. Eâdem ratione ſi ducatur OS, ut angulus ſub COS æqualis ſit angulo ſub A E F erit O S = E F; ſi vero ſumatur O R = E F, ducaturque R L, ordinatim ea applicabitur ad aſymptoton C D, & producta tranſibit per S, propterea quod linea I M ſimilis eſt & æqualis lineæ G H. Erit autem angulus ſub P R L (= angulo ſub L S Q) = angulo ſub L Q S = angulo ſub N P Q. Unde erit angulus ſub L S Q = angulo ſub N P Q, & triangula S L Q, P N Q ſimilia ſunt, angulusque ſub S L Q = angulo ſub P N Q = angulo ſub B E F. Eſt autem & L S = L Q, O S = N Q, item angulus ſub O S L (= angulo ſub O R S) = angulo ſub N Q L. Triangula igitur O S L, N Q L æqualia ſunt, habentia baſes O L, N L æquales, & angulos ſub N L Q, O L S, etiam æquales: auferatur communis angulus ſub N L S, & relinquetur angulus ſub O L N = angulo ſub S L Q = angulo ſub B E F. Q. E. D.

De Cycloide.

Sint (in *Fig.* 19.) A B, C D duæ rectæ lineæ parallelæ, quas E F ad perpendiculum ſecet. In diametrum E F deſcribatur ſemicirculus E G F, & eo ſemicirculo deſcribatur ſemicyclois F H. Jam ſi alia ſemicyclois I L Q priori ſimilis & æqualis ſed ſitu inverſo intra parallelas deſcribatur, & ſi contingentes L M, L N ducantur, dico angulum ſub M L N rectum eſſe.

Sit I O P ſemicirculus, quo deſcribitur ſemicyclois I Q, ejus diameter I P; ducatur L G O, lineis A B, C D parallela, & jungantur F G, G E, I O. Erit deinde contingens L M parallela rectæ F G, & contingens L N parallela rectæ I O, quæ parallela eſt rectæ E G. Angulus igitur ſub M L N = eſt angulo ſub F G E recto, ideoque angulus ſub M L N rectus eſt. Q. E. D.

De

De Parabolâ Semicubicâ.

Si (in *Fig.* 20.) rectam lineam AB alia recta linea CD interſecat in puncto D cum lineâ AB angulum quemcunque conſtituens; & ſi ſumatur DE = $\frac{1}{2}$ DC; deinde ducatur EF, ut DF ſit = DE; & denique diametro CF & vertice C deſcribatur parabola ſemicubica GCH, quæ tranſeat per punctum E, habeatque ordinatim applicatas ad diametrum CF lineæ FE parallelas: his poſitis, ſi parabola hæc ad lineam AB in ſitu inverſo deſcripta ſit, ut eandem in ſitu jam dicto deſcriptam interſecet, & contingentes ad punctum interſectionis ducantur, illæ contingentes ſe interſecabunt in angulo æquali angulo ſub CDB.

Sumatur in parabolâ GCH punctum quodvis I, ducatur ILC, & ſumptâ EM = EL ducatur MNC. Deinde ordinatim applicentur OIP, NQR, ducaturque CEV, item EX diametro CO parallela. His poſitis erit VX : XE :: EF : FC, & XE : XP :: DF : EF. Unde ex æquo ut VX : XP :: DF : FC, dividendoque ut VX : VP :: DF : DC. Quoniam igitur eſt DF = DE = $\frac{1}{2}$ DC, eſt etiam VX = $\frac{1}{2}$ VP. Porro ut IO*q* : EF*q* :: CO*c* : CF*c* :: VO*c* : EF*c*. Quatuor igitur ratione continuatâ proportionalium eſt VO ſecunda, quarum IO eſt prima & EF ultima. Eſt autem & IO : OV :: LF : EF. Ideoque ſunt IO, OV, FL, FE quatuor ratione continuatâ proportionales; unde ut VO : LF :: LF : EF :: VO — LF : LE, componendoque ut LF + EF : EF :: VO — EF (= VX) : LE. Demonſtratum autem fuit VX æqualem eſſe dimidio lineæ VP. Ut igitur 2 LF + 2 EF : EF :: VP : LE :: 2 LFE + 2 EF*q* : EF*q*. Jam vero ut IO : LF (:: IV : LE) :: LF*q* (2 LFE + LE*q* — EF*q*) : EF*q*, propterea quod lineæ IO,

VO

V O, L F, E F ſunt quatuor ratione continuatâ proportionales; quoniam igitur ut $VP : LE :: 2\,LFE + 2\,EFq : EFq$, erit ut $PI : LE :: 3\,EFq - LEq : EFq$. Eodem modo demonſtratur ut $NR : EM :: 3\,EFq - EMq : EFq$. Cum igitur $EM =$ ſit EL, erit $NR = PI$, ſunt autem parallelæ, ideoque puncta N, I æqualiter diſtant a linea A B. Si igitur parabola ſemicubica G C H in ſitu inverſo ad lineam A B deſcribatur, punctum N incidere poteſt in punctum I. Parabolæ huic detur ille ſitus inverſus *hcg*, & ducantur contingentes $I\Gamma S$, EW, ΔNT, $tI\delta$; item lineæ W L Y, W Z M. Erit ex naturâ parabolæ hujus $OS = \frac{2}{3}\,OC$, $FW = \frac{2}{3}\,FC$, & $QT = \frac{2}{3}\,QC$. Eſt autem & $FD = \frac{1}{3}\,FC$; unde F D, D E, & D W ſunt æquales, & angulus ſub F E W rectus: &, cum EL ſit $= EM$, erunt & anguli ſub E W L, E W M æquales. Quoniam autem L M F lineis I O, N Q parallela eſt, & lineæ O C, F C Q C ſimiliter dividuntur in punctis S, W, T, erit W L Y contingenti $I S \Gamma$ parallela, & W Z M contingenti ΔNT. Eſt igitur angulus ſub $WYD =$ angulo ſub $I\Gamma t$, & angulus ſub $WZD =$ angulo ſub $N\Delta D =$ angulo ſub $It\Gamma$. Porro cum anguli ſub E W L, E W M ſint æquales, & anguli ſub D E W, D W E etiam æquales propter linearum D W, D E æqualitatem, erit angulus ſub $YWD =$ angulo ſub W Z D. Ideoque angulus ſub C D B, qui æqualis eſt ſummæ angulorum ſub W Y D & ſub Y W D, æqualis erit ſummæ angulorum ſub $I\Gamma t$, & ſub $It\Gamma$, nimirum angulo ſub $\Gamma I \delta$ æqualis. Q. E. D.

Lond. Aug. 27. 1722.

VIII. *An*

VIII. *An Account of a Book, intituled,* Harmonia Menſurarum, ſive Analyſis & Syntheſis per Rationum & Angulorum menſuras promotæ: accedunt alia Opuſcula Mathematica: per *Rogerum Coteſium.* Edidit & auxit *Robertus Smith*, Coll. Trin. Cantab. & Reg. Soc. Socius; Aſtronomiæ & Experimentalis Philoſoph. poſt *Coteſium* Profeſſor. *Cantabrigiæ* 1722. in 4to. Proſtant apud Bibliopolas Londinenſes.

THE Book conſiſts of three Parts. In the firſt, called *Logometria*, the Author's chief Deſign is to ſhew how that ſort of Problems, which are uſually reduced to the Quadrature of the *Hyperbola* and *Ellipſis*, may be reduced to the the Meaſures of Ratio's and Angles; and afterwards be ſolved more readily by the Canons of Logarithms and Sines and Tangents. He defines the Meaſures of Ratio's to be quantities of any kind, whoſe Magnitudes are analogous to the Magnitudes of the Ratio's to be meaſured. In this Senſe any Canon of Logarithms is a Syſtem of numeral Meaſures of the Ratio's of the abſolute Numbers to an Unit: The Parts of the Aſymptote of the Logiſtic Line, intercepted between its Ordinates, are a Syſtem of Linear Meaſures of the Ratio's of thoſe Ordinates: The Areas of an *Hyperbola*, intercepted between its Ordinates to the Aſymptote, are a Syſtem of Plane Meaſures of the Ratio's of thoſe Ordinates: And ſince there may be infinite Syſtems of Meaſures according as various kinds of Quantities are made uſe of, ſuch as Numbers, Time, Velocity, and the like; or according as the Meaſures of any one Syſtem may be all increaſed or diminiſhed in any given proportion; in ſuch Variety much Confuſion may poſſibly ariſe as to the Kind and abſolute Magnitudes of particular Meaſures, which happen to fall under Conſideration. Our Author very happily removes this Difficulty; by ſhewing that the Nature of the Subject points out the Meaſure of a certain immutable Ratio for a *Modulus* in all Syſtems, whereby to determine the Kind and abſolute Magnitudes of all other Meaſures in each Syſtem.

The firſt Propoſition is to find the Meaſure of any propoſed Ratio. This he conſiders in a way ſo ſimple and general, as naturally leads to the Notion and Definition of a *Modulus*; namely, that it is an invariable Quantity in each Syſtem, which bears the ſame Proportion to the Increment of the Meaſure of any propoſed Ratio, as the increaſing Term of the Ratio bears to its own Increment. He then ſhews, that the Meaſure of any given Ratio is as the *Modulus* of the Syſtem, from whence it is taken: and that the *Modulus* in every Syſtem is always equal to the Meaſure of a certain determinate and immutable Ratio, which he therefore calls the *Ratio Modularis*. He ſhews that this Ratio is expreſſed by theſe Numbers 2,7182818 &c. to 1, or by 1 to 0,3678794 &c. So that in *Briggs*'s Canon the Logarithm of this Ratio is the *Modulus* of that Syſtem: In the Logiſtic Line the given Subtangent is the *Modulus* of that Syſtem: In the *Hyperbola* the given Parallelogram, contained by an Ordinate to the Aſymptote, and the Abſciſs from the Center, is the *Modulus* of that Syſtem: and in other Syſtems the *Modulus* is generally ſome remarkable Quantity. In the ſecond Propoſition he gives a conciſe uncommon Method for calculating *Briggs*'s Canon of Logarithms; together with Rules for finding intermediate Logarithms and Numbers, even beyond the Limits of the Canon. In the 3d Propoſition he conſtructs any Syſtem of Meaſures by a Canon of Logarithms; not only when the Meaſure of ſome one Ratio is given, but alſo without that *Datum*, by ſeeking the *Modulus* of the Syſtem by the Rule abovementioned. In the 4th, 5th, and 6th Propoſitions he ſquares the *Hyperbola*, deſcribes the Logiſtic Line and Æquiangular Spiral by a Canon of Logarithms, and ſhews ſome curious Uſes of theſe Propoſitions in their *Scholia*. Take an eaſy Example of the Logometrical Method, in the common Problem for finding the Denſity of the Atmoſphære. Suppoſing Gravity uniform, every one knows, that if Altitudes are taken in any Arithmetical Progreſſion, the Denſities of the Air in thoſe Altitudes will be in a Geometrical Progreſſion; that is, the Altitudes are the Meaſures of the Ratio's of the Denſities below and in thoſe Altitudes, and ſo the difference of any two Altitudes is the Meaſure of the Ratio of the Denſities in thoſe Altitudes. Now to determine the abſolute or real Magnitude of theſe Meaſures, the Author ſhews, *a priori*, that the *Modulus* of the Syſtem is the Altitude of the Atmoſphære, when reduced every where to the ſame Denſity as below. The *Modulus* therefore is given (as bearing the ſame Proportion to the Altitude of the Mercury in the Barometer, as the ſpecific Gravity of Mercury does to the ſpecifick Gravity of Air) and conſequently the whole Syſtem is given. For ſince in all Syſtems the Meaſures of the ſame Ratio's are analogous among themſelves; the Logarithm of the Ratio of the Air's Denſity in any two Altitudes will be to the *Modulus* of the Canon, (that is, to the Logarithm of

the

the *Ratio Modularis* defined above,) as the difference of thoſe Altitudes is to the aforeſaid given Altitude of the homogeneous Atmoſphære.

He concludes the Logometria with a *General Scholium*, containing great Variety of elegant Conſtructions both Logometrical and Trigonometrical; ſuch as give the Length of Curves either Geometrical or Mechanical; their Area's and Centers of Gravity; the Solids generated from them, and the Surfaces of theſe Solids; together with ſeveral curious Problems in Natural Philoſophy, concerning the Attraction of Bodies, the Denſity and Reſiſtance of Fluids, and the Trajectories of Planets. Several of theſe Problems have two Caſes; one conſtructed by the Meaſure of a Ratio, and the other by the Meaſure of an Angle. The great Affinity and beautiful *Harmony* of the *Meaſures* in theſe Caſes, has given occaſion to the Title of the Book. The Meaſures of Angles are defined (juſt as the Meaſures of Ratio's) to be Quantities of any Kind, whoſe Magnitudes are analogous to the Magnitudes of the Angles. Such may be the Arcs or Sectors of any Circle, or any other Quantities of Time, Velocity, or Reſiſtance, analogous to the Magnitudes of the Angles. Every Syſtem of theſe Meaſures has likewiſe its *Modulus* homogeneous to the Meaſures in that Syſtem, and may be computed by the Trigonometrical Canon of Sines and Tangents, juſt as the Meaſures of Ratio's by the Canon of Logarithms; for the given *Modulus* in each Syſtem bears the ſame Proportion to the Meaſure of any given Angle, as the *Radius* of a Circle bears to an Arc which ſubtends that Angle, or the ſame as this conſtant Number of Degrees 57,2957795130 bears to he Number of Degrees in the ſaid Angle. Upon the vhole our Author thus expreſſes himſelf, *p.* 35. " Ex

“ adductis hactenus exemplis, Geometris integrum erit
“ de methodo noſtrâ judicare; quam quidem, ſi pro-
“ ba fuerit, ulterius excolere pergent & excolendo la-
“ tius promovebunt. Patet utique campus ampliſſi-
“ mus in quo vires ſuas experiri poterunt, præſertim ſi
“ Logometriæ Trigonometriam inſuper adjungant,
“ quibus miram quandam affinitatem in ſe invicem
“ euntibus intercedere notabam. Hiſce quidem prin-
“ cipiis haud facile crediderim *generaliora* dari poſſe;
“ cum tota Matheſis vix quicquam in univerſo ſuo am-
“ bitu complectatur præter Angulorum & Rationum
“ Theoriam. Neque ſane *commodiora* ſperabit, qui
“ animadverterit effectionis facilitatem per ampliſſi-
“ mas illas, omnibuſque ſuis numeris abſolutas, tum
“ Logarithmorum, tum Sinuum & Tangentium tabu-
“ las; quas anteceſſorum noſtrorum laudatiſſimæ
“ ſolertiæ debemus acceptas. Ut vero tanti beneficii
“ uberior nobis exſurgat fructus, id nunc exponendum
“ reſtat, quibus artibus ad iſtius modi concluſiones re-
“ ctiſſimâ perveniatur. In hunc finem *Theoremata*
“ quædam tum *Logometrica* tum *Trigonometrica* ad-
“ jeciſſem, quæ parata ad uſum aſſervo; ni conſul-
“ tius viſum eſſet, quum abſque nimiis ambagibus ea
“ tradi non poſſent, intacta potius præterire atque
“ aliis denuo inveſtiganda relinquere.

Why the Author takes his Principles to be ſo general, will farther appear by an Inſtance or two. In the Problem already mentioned he meaſures the Ratio of the Air's Denſities in any Altitudes, by the Altitudes themſelves, making uſe of the Altitude of an uniform Atmoſphære for the *Modulus*. So likewiſe when he conſiders the Velocities acquired, and the Spaces deſcribed in given Times, by a Body projected upwards or downwards in a reſiſting Medium with any given Velocity; he ſhews, that the Times of Deſcent, added

to a given Time, are the Meaſures of Ratio's, to a given *Modulus* of Time, whoſe Terms are the Sum and Difference of the ultimate Velocity and the preſent Velocities that are acquired: that the Times of Aſcent, taken from a given Time, are the Meaſures of Angles, to a given *Modulus* of Time, whoſe Radius is to their Tangents in the Ratio of the ultimate Velocity to the preſent Velocities: and laſtly, that the Spaces deſcribed in Deſcent or Aſcent, are the Meaſures of Ratio's to a given *Modulus* of Space, whoſe Terms are the abſolute accelerating and retarding Forces ariſing from Gravity and Reſiſtance taken together at the Beginning and End of thoſe Spaces.

This general Account may ſuffice to illuſtrate what I am going to ſay; that ſince the Magnitudes of Ratio's (as well as their Terms) may be expounded by Quantities of any Kind, the Mathematician is at Liberty upon all Occaſions to chuſe thoſe which are fitteſt for his Purpoſe; and ſuch are they without doubt, that are put into his Hand by the Conditions of the Problem. He may indeed repreſent theſe Quantities by an *Hyperbola*, or any other Logometrical Syſtem, were not his Purpoſe anſwer'd with greater Simplicity by the very Syſtem itſelf, which occurs in each particular Problem. And the ſame may be ſaid for the Syſtems of Angular Meaſures, inſtead of recurring upon all Occaſions to Elliptical or Circular Area's.

As to the Convenience of calculating from our Author's Conſtructions, he ſhews that the Meaſures of any Ratio's or Angles are always computed in the ſame uniform Way; by taking from the Tables the Logarithm of the Ratio, or the Number of Degrees in the Angle, and then by finding a fourth Proportional to three given Quantities; for that will be the Meaſure required. The ſimpleſt Hyperbolic Area may indeed be ſquared by the ſame Operation taught in the Author's fourth Propoſition; but the ſimpleſt Elliptic Area requires ſomewhat more: Thoſe that are more complex in both Kinds (which generally happens) require an additional Trouble to reduce them to the ſimpleſt: to ſquare them by infinite Series is ſtill more operoſe, and does not anſwer the End of Geometry. Upon the whole therefore it may deſerve to be conſidered, for what Purpoſes ſhould Problems be always conſtructed by Conic Areas, unleſs it be to pleaſe or aſſiſt the Imagination. The Deſign of Theoretical Geometry differs from Problematical; the former conſiſts in the Diſcovery and Contemplation of the Properties and Relations of Figures for the ſake of naked Truth; but the Deſign of the latter is to do ſomething propoſed, and is beſt executed by the leaſt *Apparatus* of the former.

The *Logometria* was firſt publiſhed by the Author himſelf, in the *Philoſoph. Tranſact.* of the Year 1714. No 338. But his Logometrical and Trigonometrical Theorems abovementioned were not publiſhed till after his Deceaſe. Theſe Theorems make the ſecond Part of the Book, and are calculated to give the Fluents of Fluxions (reduced to 18 Forms) by Meaſures of Ratio's and Angles; in ſuch a manner, that any Perſon may perfectly comprehend their Conſtruction and Uſe, though altogether unacquainted with Curvilinear Figures, as expreſſed by Æquations. And this Circumſtance does alſo render the Application of them to the *Analyſis* and *Conſtruction* of *Problems* extremely eaſy. Of this kind the Author has given a great many choice Examples both in abſtract and phyſical Problems; which make up the third and laſt Part of the Book.

The Author, a little before his Deceaſe has informed us (in a Letter of *May* 5. wrote to his Friend Mr. *Jones*)
“ that Geometers had not yet promoted the inverſe
“ Method of Fluxions, by Conic Areas, or by Mea-
“ ſures of Ratio's and Angles, ſo far as it is capable of
“ being promoted by thoſe Methods. There is an
“ infinite Field (ſays he) ſtill reſerved, which it has
“ been my Fortune to find an Entrance into. Not to
“ keep you longer in Suſpenſe, I have found out a ge-
“ neral and beautiful Method by Meaſures of Ratio's
“ and Angles for the Fluent of any Quantity which
“ can come under this Form $\frac{d\dot{z}z^{\theta n+\frac{\delta}{\lambda}n-1}}{e+fz^n}$, in
“ which *d*, *e*, *f* are any conſtant Quantities, *z* the vari-
“ able, *n* any Index, θ any whole Number affirmative or
“ negative, $\frac{\delta}{\lambda}$ any Fraction whatever. The Fluents of
“ this Form which have hitherto been conſidered are
$\frac{d\dot{z}z^{\theta n-1}}{e+fz^n}$ & $\frac{d\dot{z}z^{\theta n+\frac{1}{2}n-1}}{e+fz^n}$: Theſe you remem-
“ ber are Sir *Iſaac Newton*'s two firſt, and from
“ theſe all his others are eaſily deduced. And as his
“ irrational Forms of the quadratick Kind are derived
“ from the rational, ſo from my general rational
“ Form I deduce irrational ones of all Kinds. For in-
“ ſtance, if $\frac{\delta}{\lambda}$ repreſent any affirmative or negative
“ Fraction, the Fluent of any Quantity of this Form
“ $d\dot{z}z^{\theta n-1} \times \overline{e+fz^n}|^{\frac{\delta}{\lambda}}$, or of this $d\dot{z}z^{\theta n-1}$
“ $\times \overline{\frac{e+fz^n}{g+hz^n}}|^{\frac{\delta}{\lambda}}$ and ſo of ſome others, depends upon

“ the Meaſures of Ratio's and Angles. Mr. *Leibnitz*
“ in the *Leipſic Acts* of 1702, *p.* 218 and 219, has very
“ raſhly undertaken to demonſtrate, that the Fluent
“ of $\frac{\dot{x}}{x^4+a^4}$ cannot be expreſſed by Meaſures of Ratio's
“ and Angles; and he ſwaggers upon the Occaſion
“ (according to his uſual Vanity) as having by
“ this Demonſtration determined a Queſtion of the
“ greateſt Moment. Then he goes on thus; as the
“ Fluent of $\frac{\dot{x}}{x+a}$ depends upon the Meaſure of a
“ Ratio, and the Fluent of $\frac{\dot{x}}{xx+aa}$ upon the Meaſure
“ of an Angle; ſo he had more than once expreſſed
“ his Wiſhes, that the Progreſſion may be continued,
“ and it be determined to what Problem the Fluents of
“ $\frac{\dot{x}}{x^4+a^4}$, $\frac{\dot{x}}{x^8+a^8}$, *&c.* may be referred. His Deſire
“ is anſwered in my general Solution, which
“ contains an infinite Number of ſuch Progreſſions.
“ I can go yet farther, and ſhew him how by Meaſures
“ of Ratio's and Angles, without any Exception
“ or Limitation, the Fluent of this general Quantity
“ $\frac{d\dot{z}z^{\theta n+\frac{\delta}{\lambda}n-1}}{e+fz^n+gz^{2n}}$ or even this $\frac{d\dot{z}z^{\theta n+\frac{\delta}{\lambda}n-1}}{e+fz^n+gz^{2n}+hz^{3n}}$
“ may be had; where θ, as before, repreſents any Integer,
“ and the Denominator λ of the Fraction $\frac{\delta}{\lambda}$, repreſents
“ any Number in this Series, 2. 4. 8. 16. 32. *&c.*
“ any whole Number being denoted by its Numerator
“ δ. In truth I am inclined to believe, that Mr. *Leibnitz*'s
“ grand Queſtion ought to be determined

« the contrary Way; and that it will be found at laſt,
« that the Fluent of any rational Fluxion whatever,
« does depend upon the Meaſures of Ratio's and
« Angles, excepting thoſe which may be had in finite
« Terms even without introducing Meaſures.

Dr. *Taylor* knowing by this Letter what the Author had done, was pleaſed to propoſe the Invention of the Fluents of the two laſt Fluxions as a Problem to the Mathematicians in foreign Parts. Mr. *Bernoulli* in the *Leipſic Acts* of 1719. *p*. 256, did ſhew accordingly how they are reducible to Conic Area's. The Editor has publiſhed the Author's own Solution by Meaſures of Ratio's and Angles; and upon this Foundation has conſtructed new Tables of Logometrical and Trigonometrical Theorems, for the Fluents of Fluxions reduced to 94 Forms, part rational and part irrational. He has likewiſe added general Notes upon the chief Difficulties in the Book, together with a Method of compoſing Synthetical Demonſtrations of Logometrical and Trigonometrical Conſtructions, illuſtrated by various Examples.

The firſt Treatiſe in the *Miſcellaneous Works* is concerning the *Eſtimation of Errors in Mixt Mathematicks.* It conſiſts of 28 Theorems, to determine the Proportions among the leaſt contemporary Variations of the Sides and Angles of Plane and Sphærical Triangles, while any two of them remain invariable. An Example will ſhew their great Uſe in Aſtronomy. The Time of the Day or Night is frequently to be determined by the Altitude of ſome Star. Let it then be propoſed to find the Error, that may ariſe in the Time, from any given Error in taking the Altitude. By applying the 22d Theorem to the Triangle form'd by the Complements of the Star's Altitude and Declination and by the Complement of the Pole's Elevation, the Author ſhews, that the Variation of the Angle at the Pole, and conſequently the Error in Time, will be as the Error in the Altitude directly, as the Sine Complement of the Poles Elevation inverſely, and as the Sine of the Star's *Azimuth* from the Meridian inverſely. Conſequently, if the Error in the Altitude be given, under a given Elevation of the Pole, the Error in Time will be reciprocally as the Sine of the *Azimuth* contained by the Meridian and the Vertical which the Star is in. This Error therefore will be the ſame, whatever be the Altitude of the Star in the ſame Vertical; and will be leaſt when the Vertical is at right Angles to the Meridian. . But will be abſolutely the leaſt in the ſame Circumſtance, if the Obſerver be under the Æquator. In which Caſe, if the Error in the Altitude be one Minute, the Error in the Time will be four Seconds, If the Obſerver recedes from the Æquator towards either Pole, the Error will be increaſed in the Proportion of the Radius to the Sine Complement of the Latitude: So that in the Latitude of 45 Degrees it will be $5\frac{2}{3}$ Seconds, and in the Latitudes of 50 and 55 it will be $6\frac{2}{9}$ and $6\frac{37}{38}$ Seconds reſpectively. If the Star be in any other vertical Oblique to the Meridian, the Error will ſtill be increaſed in the Proportion of the Radius to the Sine of that oblique Angle. Laſtly, if the Error in the Altitude be either bigger or leſs than one Minute, the Error in Time will be bigger or leſs in the ſame Proportion. Much after the ſame manner may the Limits of Errors be computed in other Caſes, which ariſe from the Inaccuracy of Obſervations, and from hence the moſt convenient Opportunities for obſerving are alſo determined.

The Second Treatiſe is concerning the *Differential Method*. The Author having wrote it, before he had ſeen Sir *Iſaac Newton*'s Treatiſe upon that Subject, has handled it after a manner ſomewhat different.

The Title of the Third Treatiſe is *Canonotechnia* or concerning the Conſtruction of Tables by Differences. It conſiſts of ten Propoſitions, moſt admirably contrived for expeditious Computation of intermediate Terms in any given Series. The laſt Propoſition, which contains a general Solution of the whole Deſign, is this; *Datis ſeriei cujuſcunque terminis aliquot æquidiſtantibus, quorum intervalla ſecanda ſunt in æquales quotlibetcunq; partes, propoſitum ſit invenire terminos interſerendos.*

The Book concludes with three ſmall Tracts, concerning the Deſcent of Bodies, the Motion of Pendulums in the Cycloid, and the Motion of Projectiles, composed in a very natural and eaſy manner.

The Author has wrote ſome other Pieces, yet unpubliſh'd, which the Editor has given an Account of in his Preface to the Book.

The Reader will find every Subject treated with uncommon Elegance and Simplicity.

FINIS.

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PHILOSOPHICAL TRANSACTIONS.

For the Months of *September* and *October*. 1722.

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ERRATUM.

N° 372. p. 146. l. 2. read, *May 5th*, 1716.

I. *Obſervations*

I. *Obſervations upon a* Fœtus, *and the Parts of Generation of a Sheep. In a Letter to the* Royal Society, *from Mr.* Leeuwenhoek, *F. R. S. Tranſlated from the* Dutch *by Dr.* Sprengell, *F. R. S.*

A Certain Butcher kept, about a League from this Town, ſeveral young Wethers at Graſs, with one Ewe, which within two Years time had twice lamb'd. This Ewe being very fat, he reſolv'd to kill; and to this end he brought it to another Paſture juſt by the Town, where he had ſeveral other young Wethers; but it happen'd, that among them there was one young Ram, which he judged to the be about twenty Weeks old; this young Ram cover'd the Ewe in his preſence; ſoon after which the Butcher kill'd the Ram, but left the Sheep about five Days longer in the Paſture before he kill'd it; out of which Sheep's Belly he ſaid that he cut 28 Pounds of Fat. But obſerving, upon opening the Ewe, that the *Uterus* was four times bigger than ordinary, he brought the *Uterus*, with the *Ovaria*, to me, aſſuring me, that it was not yet quite five Days ſince the young Ram had cover'd the Sheep, and that there was no other Ram thereabouts.

This *Uterus* I put in an earthen glaz'd Pan, and cover'd it over, in order to diſſect it next Morning, it being pretty late in the Evening when the Butcher brought it me. I then began firſt to try to penetrate into the Womb from the *Vagina*, with the Point of a ſmall Pair of Sciſſars, but I found it ſo cloſe, that I could not enter it, therefore I cut a piece off from the Womb, out of which ran a clear Water, and within it lay the *Fœtus* with all its Coverings. I ſpread this upon the backſide of a *China* Tea-Diſh, and finding that it ſtill contained more Water, I made a ſmall Inciſion to drain it, and to let it dry, that I might obſerve it the better. I could plainly ſee the *Vertebræ* of the Neck and Back, as alſo the Joints of its ſhort Tail; I thought likewiſe that I ſaw the Eyes. But when it was quite dry, I could not obſerve its Backbone ſo well as before, when it was as yet moiſt, tho' the Painter, who made the Draught, and had ſharper and younger Eyes than mine, ſaw the Bones of the Back very diſtinctly. My Deſign in drying it was to cut it in ſmall Slices, that then I might the better obſerve the inner Parts, for it was ſo extremely ſoft and tender when moiſt, that with the leaſt Touch its Parts would be diſorder'd and confounded. Therefore I cut this *Fœtus* into fifteen Slices, and obſerv'd them with a Microſcope, but could not be very certain what I ſaw. I thought that I ſaw the Inteſtines, as alſo the Bladder; and coming to the Breaſt, I fancied that I ſaw the Heart; but I ſaw and obſerv'd, with a great deal of Pleaſure, that two Blood-Veſſels lay near together in the Brain, and how they were ſpread into Branches. I had this *Fœtus* drawn as it lay in its Teguments. See *Fig.* 1. A B being the *Fœtus*, and A C D E I K and A F G H L K, the Membranes wherewith it was involved, in the manner as I had ſpread and dried them, wherein the Blood-Veſſels, as much as poſſible, are delineated. Now ſome Perſons might expect, that I ſhould have look'd for the Extremities of the Blood-Veſſels: but no, the Blood-Veſſels have no end, as I have frequently ſaid. Beſides, they become gradually ſo exquiſitely fine, that the Blood which paſſes thro' them, can exhibit no red Colour to our Eyes; ſo that there is no tracing them when entring into the Veſſels that return the Blood back to the Heart, except in living Animals, where one may ſee the Blood enter into the returning Veſſels. Before the Butcher gave me this *Vterus*, he ſqueez'd it betwixt his Fingers, and told me that he could feel nothing in it; and this I believe he had done ſeveral times before, by which means he tore off the Veſſels by which the *Fœtus* was faſten'd to the *Vterus*; which I ſuppoſe was the occaſion that, upon opening the *Vterus*, the *Fœtus* with its Coverings came ſo eaſily forth.

I alſo took a Draught of the *Tuba Fallopiana*. See *Fig*. 2. M N O P. At P, is the imaginary Orifice, which is thought to ſuck the Egg from the *Ovarium*, according to the old abſurd Notion; at M is ſhewn where the *Tuba* increaſes in Bigneſs, and at Q R the fleſhy Subſtance, which I cut away from the *Vterus*. I then had alſo cut off the ſo call'd *Ovaria*, and the pretended *Ova*, which latter were much too big to think that they could paſs upon Conception thro' the *Tubæ Fallopianæ*. I therefore took the length of the *Fœtus* with a Pair of Compaſſes, and meaſur'd it upon a divided Braſs Rule, and this I did alſo as to its Breadth; I then took the middle Number between theſe, and multiplied it twice by itſelf, to bring it to a Cube Number. I next took the Length of the *Axis* of an *Ovum*, as it lay in the *Ovarium* inclos'd in its Membranes, and taking the Cube of that length, and dividing one Cube Number by the other, I found that ſuch an *Ovum* was about ſeven times bigger that the *Fœtus*, notwithſtanding it had had near five Days Growth. I ſhew'd this *Fœtus*, with its Covertures, to two Phyſicians and one Surgeon, and I gave them the pretended *Ovaria* in their Hands, and they agreed, that not one *Ovum* was miſſing out of the *Ovaria*. Then I ask'd them what they thought? how it was poſſible that ſuch an *Ovum* could paſs thro' the Falloppian Tube? Whereupon the one ſaid, that the *Ovarium* was quite out of doors, and that it was nothing but ſome fleſhy Subſtance. But the other ſaid, that notwithſtanding this, all Animals came from an Egg, and the laſt told me, that he believed that Tube to be neither of a Sheep nor a Lamb; but I ſhew'd them that it was from an Ewe which at leaſt had lamb'd twice, and yet that the Tube was neither thicker nor wider than the Tube of a Lamb. Here I could obſerve more than ever, how hard it is to bring Mankind off from their Prejudices, when once they have laid them down as Principles.

After having kept theſe *Ovaria* ſome Days, by which means they were pretty much ſhrunk in the drying, I order'd them to be drawn, that the Bigneſs of the Eggs might be obſerv'd. See *Fig*. 3. A B C D E is the *Ovarium*, which was on that ſide of the *Uterus*, in which the *Fœtus* had lain. You muſt obſerve that the *Uterus* of a Sheep is divided by a Membrane, ſo that the young ones cannot touch one another. D E A is the part where it was faſtened. In this *Ovarium* you may obſerve a round Protuberance, which is beſet with ſeveral others; this great Protuberance is what is called an *Ovum*. This *Ovarium* is not here repreſented ſo large, as it was, when I cut it off from the Parts that it grew to. There was beſides, at one ſide of that *Ovarium*, a large round Body, grown to the *Ovarium*, which ſeem'd alſo to be an *Ovum*. This is delineated in *Fig*. 4. F G, on which there appear'd ſeveral other little round Bodies protuberant from it. Now on the other ſide of the *Uterus*, there was a large fleſhy prominent *Ovum* (as it is call'd) which might plainly be obſerved without a Microſcope, whoſe Bigneſs is likewiſe drawn in *Fig*. 5. at H I, upon which you may likewiſe obſerve a ſmaller round Body, and out of that again other ſtill ſmaller round Bodies appear protuberant. After I had quite dried theſe Eggs, repreſented in *Fig*. 3, 4 and 5, I ſtill obſerv'd more and more of the prominent round Bodies upon them, inſomuch that upon one of them, I told ſixteen round little Bodies, whereof ſome, by loſing all their Moiſture, were ſunk in and had a Dent in the middle. Furthermore, I cut theſe Eggs, with a very fine ſharp Knife, into thin Slices, and then obſerving them with a Microſcope, I ſaw Blood-Veſſels in them, and alſo other ſorts of Veſſels, which I did not take for Blood-Veſſels, and among the reſt one ſo big, that a Hair of ones Head might enter it, beſides abundance of others exceeding ſmall. After many Obſervations, I could think no otherwiſe, but that the ſo call'd Eggs conſiſted of nothing but Veſſels, and that the ſuperfluous Moiſture, which was ſent to theſe Eggs, did not circulate (except only what was in the Blood-Veſſels) and by overcharging the Veſſels did elevate them into theſe ſmall Protuberances, and ſometimes burſting them, did thereby leave a Dent in the middle; which Dent having been obſerv'd by ſome Perſons, they firmly believ'd, that that was the Place where the *Ovum* was ſuck'd out, from whence ſprung the *Fœtus*. I was very ſorry that I did not get this *Uterus* without its having been ſqueez'd, for I do not queſtion, but that I might otherwiſe have plainly diſcover'd all the Members of this *Fœtus*, ſince I could plainly obſerve its Backbones even with the naked Eye, and that in a *Fœtus* not of quite five Days Growth. I hope after this, nobody will pretend to ſay, that the Animal *in Utero* at the Beginning, is nothing but an unform'd Maſs. Theſe Obſervations I made in the Month of *September*, 1718.

Delft, *June* 13.
1722.

II. *Obſervations upon the* Callus *of the Hands and Feet, by the ſame curious Gentleman. Tranſlated by* John Chamberlayne, *Eſq*;

IN the Month of *September*, 1719. feeling an acute Pain in one of my Feet, at the Joint between the Foot and the little Toe, which I imagined to proceed from the more than uſual Thickneſs of the *Callus* or hard Skin, upon that part; I cauſed my Servant, partly with his Nails, and partly with a Penknife, to take off that hard Skin, and let it fall upon a blue Paper, that I had ſet my Foot upon.

This *Callus*, or hard Skin, was compos'd of little ſcaly Shivers lying upon one another, and the whole Piece was as large as a ſmall Nail of a Man's Hand.

I view'd the ſaid Shivers thro' a Microſcope, but could not ſatisfy myſelf, becauſe they lay ſo irregularly on each other.

Moreover, I took a little Bit of the aforeſaid hard Skin, laid it on a clean Glaſs-Plate, ſteep'd it in pure Rain-Water, and gently dividing it with a Piece of a Quill, I was amazed to ſee into what a vaſt Number of Particles it ſeparated, and that with as much Readineſs, as if they had never been join'd.

Afterwards I took two or three of the ſaid Particles, of which many were of the Figure of a Weaver's Shuttle, being broad in the middle and pointed at each End, with a Line in the middle, like thoſe upon the uppermoſt or outſide Skin of Fruits, or of our Bodies, but generally irregular; they were very thick in Proportion to their Bigneſs. I took, I ſay, two or three of the ſaid Particles, and laid them on a clean Glaſs, and put to them a Drop of Water, as large as a coarſe Grain of Sand, and divided the ſame as well as I was able; and viewing the divided Particles thro' a Microſcope, I was aſtoniſhed at the prodigious number of exceeding ſmall Particles that occurr'd to my Sight, and which were of the ſame Figure as beforeſaid.

Moreover, I took ſome of the thickeſt Pieces of the horny Skin, but not half ſo thick as the Back of a ſmall Knife, and ſlit them into as thin Slices as I could, and placed them upon a clean Glaſs, with the Deſign of diſcovering the exceeding thin Particles lying upon one another; and having moiſten'd them, they ſpread themſelves out further, and when they became dry again, they ſeparated into ſeveral Parts; and I obſerv'd likewiſe that each of theſe ſeparated Parts were compoſed of many thinner Particles lying on each other. In order to have a clearer Notion of the Contexture of thoſe Particles, by which the Skin of the Hands and Feet of thoſe who are inured to hard Labour, or walk much, increaſes in Thickneſs, and is ſurrounded, I cauſed a ſmall Portion of the aforeſaid ſeparated Particles to be drawn, as appears by *Fig*. 6. A B C D, tho' they were not all ſo exact and compleat, as they are repreſented in the ſaid Figure; and according to the beſt of my Obſervation, they had all been ſeparate Pieces, and none of them united to one another.

Again I placed ſeveral little Pieces of this hard Skin, which I had cut off at the thickeſt Part of it, before a Microſcope, and moiſten'd them with fair Rain-Water when I put them upon the Glaſs-Plate, by which Means they acquired a great Extent; and being dry'd, they ſhrunk again, and thereby appear'd in ſeveral long Particles, and each of them ſeem'd to conſiſt of other long Particles, as they are repreſented in *Fig.* 7. E F G H. So that E H or F G, was the Thickneſs of that Piece of thick Skin, which I had cut through.

From this Obſervation, I conſider'd with myſelf, whether one of theſe long ſtripe-like Particles, ſuch as appear'd to the Eye at E F or H G, might not be the Thickneſs which the horny Skin had acquir'd in the Space of a Month; and whether the very thin Particles, which appear'd in ſuch a little Stripe, were not the Accretion which the hard Skin receiv'd in one Day. This laſt mention'd little Piece, repreſented by *Fig.* 7. was not ſo large to the naked Eye as a common Grain of Sand. And whereas I placed before a Microſcope another very ſmall Piece of Skin, that was ſomething thinner than the former, I could perceive the exceeding thin Particles repreſented by *Fig.* 8. I K L M, which were the *Strata*, or Beds, in which the horny Particles of the Skin lay, and ſo compoſed the Thickneſs thereof.

For my further Satisfaction, I ſent for a Maſon's Servant, who was a laborious Workman, and cut out of his Hand, where it was moſt thick and brawny, two Bits of the hard Skin; and then I ſlit it into ſmall Slices, and obſerved eaſily enough the thin Particles lying upon each other, but could not ſeparate the little

Scales, of which each little *Stratum* of the hard Skin conſiſted, becauſe, as I fancy'd, thro' the hard working of the Man, the Particles of the ſaid Skin were ſo preſſed upon one another, and ſo cloſely join'd, that they could be no longer ſeparated.

Now foraſmuch as the Hands of the Maſons or Bricklayers are often cover'd with the ſharp Salts of the Chalk or Lime, which might prevent the Separation of the very ſmall Parts of the hard Skin; I ſent for a Maſter-Carpenter, who was likewiſe a diligent Workman, and viewing his Hands, in order to pare off ſome of the Brawn thereof, I found them as ſoft in the Palms, as if he had never been uſed to Labour; upon which I ſaid to him, you waſh your Hands very often I believe: to which he reply'd, ten times a Day at leaſt, for I hate to ſee them foul. Then I ſent for a Ploughman, who was moſtly a Tiller of Corn-Land; this Perſon's Hands were very hard; however, I cut two Pieces of horny Skin out of them, which after I had done, I cut them in little Bits, but they were ſo hard, that a ſharp Knife, which I us'd, got ſeveral Notches in doing it. And I obſerv'd that the uppermoſt part of the Skin was full of little Rents, and all the *Strata* were preſſed ſo cloſely together, that I could not make any Diſcoveries therein, ſave only that the little Beds lay on each other, and that the Thickneſs of the Skin conſiſted thereof.

Furthermore I put the two Pieces of hard Skin into warm Water, in order to ſoften them, and then to ſeparate the Parts from each other, but could not do it, becauſe they were ſo ſtrongly joyn'd together.

I have ſeveral times obſerved, in waſhing my Hands, that when I rubb'd the Palms of my Hands ſtrongly againſt one another, with very little Water between them, ſome Particles would be rubb'd off from the Skin, and continue between my Hands.

For my farther Satisfaction in this Matter, I put one of my Fingers into fair Rain-Water, and with that waſhed the Part of my Thumb which is join'd to my Hand; after which I rubb'd both Finger and Thumb hard againſt each other, then I gently ſcraped with a Pen-knife, the Matter, which I had looſen'd from the Skin by moiſtening and rubbing it; and taking off a little of that which ſtuck to the Knife, I put it upon a clean Glaſs-Plate, and ſetting it before a Microſcope, I ſaw with Aſtoniſhment the great number of Particles of Skin, which lay ſcatter'd upon the Glaſs, but were more irregular than thoſe, which I ſeparated from the brawny Skin of my Foot, and which were not very cloſely preſſed together, becauſe I don't walk much, it being troubleſome to me at my great Age.

After this I moiſten'd the back of my Hands, and then rubb'd them ten or twelve times againſt each other, and having ſo done, I ſcraped off, with a Pen-knife very gently, ſome of the Matter which I had looſen'd by rubbing; and placing it on a clean Glaſs, I view'd it with a Microſcope, after having ſeparated the Particles of the ſaid Matter from each other with a little Water, and diſcover'd abundance of little Scales which come off our Hands.

Foraſmuch as the Skin of my Hand is in no part thicker than that, which is upon my Thumb next to the Nail, having in my advanced Age chiefly uſed my Thumbs in the Examination of Microſcopical Objects, I made one of my Thumbs a little moiſt, and rubb'd the other againſt it, and placed the rubb'd off Matter before a Microſcope, thro' which I obſerv'd ſuch a prodigious Number of Particles, like thoſe repreſented by *Fig*. 6. A B C D, but all irregular, that no Man can conceive it without having ſeen it.

Now as we find, that ſuch a Quantity of Particles is ſeparated from the Hands, and is daily renewed in a well conſtituted Body; we muſt conclude, that we eat ſeveral of the ſaid rubb'd off Particles in our Bread, and that they turn to Nouriſhment; and I am of Opinion, that there is hardly any Food prepared for us, eſpecially ſuch as paſſes pretty much thro' the Hands, but that ſome of the rubb'd off Particles are mix'd therewith, eſpecially when they knead Dough from Meal or Flower; and ſtill more, when the Bakers knead with their Feet, as in making of Rye-Bread.

Since theſe Obſervations concerning the Friction or rubbing of my Hands, I take more notice thereof, when I waſh and dry them, than formerly; and I ſtand amazed at the numerous Particles that daily ſeparate themſelves from my Hands, and grow on them again; and at the particular Proviſion, that is made for producing theſe Particles, in the Palms of our Hands and Bottom of our Feet, whereas we do not by far meet with ſuch a Quantity of Particles conſtantly prodnced in other Parts of our Body; for if we obſerve thoſe, who work much with the back of their Hands, we ſhall not meet with any of that hard Skin we have been ſpeaking of, but only a kind of Tumor, or riſing, as the Dry-ſheerers, or thoſe who dreſs Cloth, have upon their left Hands.

In ſhort, the Manner of the Production of theſe ſmall Particles will be a Myſtery to us, though our Hands and Feet muſt be fortified with ſuch a Matter, to enable them to ſupport all that Force, and Preſſure which they are obliged to undergo.

Delft, July 7.
1722.

III. De Fractionibus Algebraicis Radicalitate immunibus ad Fractiones Simpliciores reducendis, deque ſummandis Terminis quarumdam Serierum æquali Intervallo a ſe diſtantibus. Auctore *Abrahamo de Moivre*, S. R. Socio.

Eruditiſſimo Viro *JOHANNI MACHIN*,
Societatis Regalis Secretario, *A. de Moivre*, S. P.

MITTO tibi excerpta quædam e Chartis meis coram Regali Societate 5° *Maii* 1720, *exhibitis, quibus eodem die manum appoſuerunt Secretarii. Pars altera harum Chartarum jam per biennium apud Cl. Præſidem repoſita fuerat; continebat autem Demonſtrationes Propoſitionum quarumdam in Libro a me Anglice emiſſo qui inſcriptus eſt*, THE DOCTRINE OF CHANCES. *Pars altera continebat explanationem uberiorem Demonſtrationum quas prior levius tetigerat. Jam cum ſæpius me inſtigaſti ut ſelectas Propoſitiones quaſdam ex his Chartis deſumptas publici juris facerem, utpote exiſtimans in illis quædam reperiri quæ ad res majoris momenti quam ſit ſpeculatio ludorum applicari poſſint; huic tuo deſiderio tandem obtempero, idque eo libentius, quo mihi videor jure aliquo a Te itidem impetrare poſſe ut pulcherrima tua inventa in Publicum proferre diutius non reluctèris.* Vale.

2 Auguſt
1722.

PRO-

PROPOSITIO I.

SI ſit Fractio quælibet $\frac{1}{1-ex+fxx-gx^3\ \&c.}$, *cujus Numerator ſit data Quantitas, & Denominator ſit Multinomium utcunque compoſitum ex datis,* 1, e, f, g, &c. *& indeterminata* x, *dico Fractionem ſupradictam ad Fractiones ſimpliciores reducibilem fore.*

Caſus Primus.

Sit Fractio propoſita $\frac{1}{1-ex+fxx}$; finge Denominatorem $1-ex+fxx=0$, ſintque $\frac{1}{m}$, $\frac{1}{p}$ radices iſtius Æquationis, ſive facto $xx-ex+f=0$, ſint m, p, radices Æquationis novæ, fac $A=\frac{m}{m-p}$, atque $B=\frac{p}{p-m}$, & erit Fractio propoſita æqualis ſummæ $\frac{A}{1-mx}+\frac{B}{1-px}$.

Caſus Secundus.

Sit Fractio propoſita $\frac{1}{1-ex+fxx-gx^3}$; fingatur $x^3-exx+fx-g=0$, ſintque m, p, q, radices iſtius Æquationis, pone $A=\frac{mm}{\overline{m-p}\times\overline{m-q}}$,

B =

$B = \frac{pp}{\overline{p-m} \times \overline{p-q}}$, $C = \frac{qq}{\overline{q-m} \times \overline{q-p}}$; & erit Fractio propoſita æqualis ſummæ $\frac{A}{1-mx} + \frac{B}{1-px} + \frac{C}{1-qx}$.

Caſus Tertius.

Sit Fractio propoſita $\frac{1}{1-ex+fxx-gx^3+hx^4}$. Fingatur $x^4 - ex^3 + fxx - gx + h = 0$, ſintque m, p, q, s, Radices iſtius Æquationes, pone $A = \frac{m^3}{\overline{m-p} \times \overline{m-q} \times \overline{m-s}}$, $B = \frac{p^3}{\overline{p-m} \times \overline{p-q} \times \overline{p-s}}$, $C = \frac{q^3}{\overline{q-m} \times \overline{q-p} \times \overline{q-s}}$, $D = \frac{s^3}{\overline{s-m} \times \overline{s-p} \times \overline{s-q}}$, eritque Fractio propoſita æqualis ſummæ $\frac{A}{1-mx} + \frac{B}{1-px} + \frac{C}{1-qx} + \frac{D}{1-sx}$.

Caſus Quartus.

Sit Fractio propoſita $\frac{1}{1-ex+fxx-gx^3+hx^4-kx^5}$, fingatur $x^5 - ex^4 + fx^3 - gxx + hx - k = 0$, ſintque m, p, q, s, t, Radices iſtius æquationis; pone $A = \frac{m^4}{\overline{m-p} \times \overline{m-q} \times \overline{m-s} \times \overline{m-t}}$, $B = \frac{p^4}{\overline{p-m} \times \overline{p-q} \times \overline{p-s}}$

$\frac{}{\times\overline{p-s}\times\overline{p-t}}$, $C=\frac{q^4}{\overline{q-m}\times\overline{q-p}\times\overline{q-s}\times\overline{q-t}}$,

$D=\frac{s^4}{\overline{s-m}\times\overline{s-p}\times\overline{s-q}\times\overline{s-t}}$, $E=\frac{}{\overline{t-m}}$

$\frac{t^4}{\times\overline{t-p}\times\overline{t-q}\times\overline{t-s}}$. Eritque Fractio propoſita æqualis ſummæ, $\frac{A}{1-mx}+\frac{B}{1-px}+\frac{C}{1-qx}+$ $\frac{D}{1-sx}+\frac{E}{1-sx}$. Lex Reductionis ita uno intuitu ſe prodit, tamque facilis eſt illius continuatio ut inutile foret illam verbis explanare.

Corollarium I.

Si Radices omnes ſint æquales, non poterit Fractio propoſita reduci ad ſimpliciores.

Corollarium II.

Si Radices aliquæ ſint æquales, aliæ vero inæquales, poterit reduci fractio propoſita ad ſimpliciores; ſit *v.g.* fractio propoſita $\frac{1}{1-ex+fxx-gx^3}$, factoque ut præſcriptum eſt $x^3-exx+fx-g=0$. Sint Radices iſtius æquationis m, p, q, quarum m & p ſint æquales: erunt fractiones ſimplices in quas reſolvitur propoſita $\frac{mm}{\overline{m-p}\times\overline{m-q}\times\overline{1-mx}}+\frac{}{\overline{p-m}}$ $\frac{pp}{\times\overline{p-q}\times\overline{1-px}}+\frac{qq}{\overline{q-m}\times\overline{q-p}\times\overline{1-qx}}$; addantur duæ priores in unam ſummam, & erit ſumma (diviſis Numeratore & Denominatore per $\overline{m-p}$)

$$\frac{\overline{mp-q}\times\overline{m+p}+mpqx}{\overline{m-q}\times\overline{p-q}+\overline{1-mx}\times\overline{1-px}},$$ ſive

$$\frac{mm-2qm+mmqx}{\overline{m-q}|^2\times\overline{1-mx}|^2},$$ ſive $$\frac{m}{\overline{m-q}\times\overline{1-mx}|^2}$$

$$-\frac{qm}{\overline{m-q}|^2\times\overline{1-mx}|^2},$$ adeoque Fractiones reductæ

erunt $$\frac{m}{\overline{m-q}\times\overline{1-mx}|^2}-\frac{qm}{\overline{m-q}|^2\times\overline{1-mx}|^2}$$

$$+\frac{qq}{\overline{m-q}|^2\times\overline{1-qx}}.$$

Corollarium III.

Si Fractiones ſimplices in quas reſolvitur Fractio propoſita involvant Quantitates imaginarias, tunc quicquid eſt imaginarii ſemper deſtruetur per additionem duarum vel plurium fractionum numero pari ſumptarum.

Corollarium IV.

Ex combinatione Fractionum ſimplicium, & apta limitatione Radicum, plurima ſuborientur Theoremata in quibus inerit concinnitas quædam minime aſpernanda Ex. *g.* ſit fractio propoſita $\frac{1}{1-ex+fxx-gx^3+hx^4}$, factoque ut antea $x^4-ex^3+fxx-gx+h=0$. Sint $m, p, q, s,$ Radices Æquationis, ſintque Fractiones in quas reſolvitur propoſita, $\frac{A}{1-mx}+\frac{B}{1-px}$

$+\frac{C}{1-qx}+\frac{D}{1-sx}$. Ponatur $q=-m$, atque $s=-p$; addantur ſimul duæ priores, itemque duæ poſteriores, & reducetur fractio propoſita ad

$$\frac{m+p-mpx}{2\times\overline{m+p}\times\overline{1-mx}\times\overline{1-px}}+\frac{m+p+mpx}{2\times\overline{m+p}\times\overline{1+mx}\times\overline{1+px}}$$

ſi vero ponatur $p=-m$, atque $s=-q$, & addantur duæ priores, itemque duæ poſteriores, reducetur Fractio propoſita ad

$$\frac{mm}{\overline{mm-qq}\times\overline{1-mmxx}}+\frac{qq}{\overline{qq-mm}\times\overline{1-qqxx}}.$$

PROPOSITIO II.

Si ſit Fractio quælibet cujus Numerator ſit data Quantitas, & Denominator ſit Trinomium vel Quadrinomium vel Quinquinomium, &c. *radicalitate non affectum & utcunque compoſitum ex datis,* 1, e, f, g, h, &c. *& indeterminata* x *atque dividatur Numerator per Denominatorem, ut habeatur Series Infinita; dico fore ut, ſi ſumantur Termini quilibet iſtius ſeriei æqualibus intervallis a ſe invicem diſtantibus, ſeries infinitæ inde reſultantes, ſummabiles futuræ ſint.*

Exemplum I.

Sit Fractio propoſita $\frac{1}{1-x-xx}$; reducatur illa ad ſeriem infinitam, nempe ad $1 + x + 2xx + 3x^3 + 5x^4 + 8x^5 + 13x^6 + 21x^7 + 34x^8$ &c. ſumanturque termini omnes alterni, incipiendo a primo, itidemque ſumantur Termini omnes alterni, incipiendo a ſecundo, hincque conficiantur ſeries binæ,

Videlicet, $1 + 2xx + 5x^4 + 13x^6 + 34x^8$ &c.
$x + 3x^3 + 8x^5 + 21x^7 + 55x^9$ &c.

Fingatur Denominator Fractionis propoſitæ, $1 - x - xx = 0$, jam cum indices poteſtatum indeterminatæ x, in novis ſeriebus ſe invicem ſuperent communi differentia 2, pone $xx = z$, atque ope duarum æquationum $1 - x - xx = 0$, & $xx = z$, exterminetur x; fietque $1 - 3z + zz = 0$; jam nunc reſtituatur x, & erit $1 - 3xx + x^4 = 0$; dividatur hæc æquatio per primam, quotiens erit $1 + x - xx$; ſumantur alternatim Termini quotientis, propter Terminos alternatim ſumptos in ſerie propoſita, hincque orientur ſummæ duæ, $1 - xx$, & x; conſtituantur hæ ſummæ Numeratores Fractionum duarum quarum communis Denominator ſit $1 - 3xx + x^4$. eruntque $\frac{1 - xx}{1 - 3xx + x^4}$ & $\frac{x}{1 - 3xx + x^4}$ ſummæ reſpectivæ novarum Serierum.

Exemplum II.

Si vero deſiderentur ſummæ terminorum intervallis binis a ſe diſtantium, fiat ut prius $1 - x - xx = 0$, jam cum indices poteſtatum in novis ſeriebus ſe invicem ſuperunt communi differentia 3, ponatur $x^3 = z$, & fiet $1 - 4z - zz = 0$, atque reſtituto x, fiet $1 - 4x^3 - x^6 = 0$; dividatur $1 - 4x^3 - x^6$ per $1 - x - xx$, quotiens erit $1 + x + 2xx - x^3 + x^4$, cujus termini ordinatim ſumpti ad intervalla bina, tres conficient ſummas, videlicet, $1 - x^3$, $x + x^4$, $2xx$, quæ ſigillatim ſumptæ, erunt illæ Numeratores, trium Fractionum, quibus ſi apponatur communis Denominator $1 - 4x^3 - x^6$, erunt tres Fractiones, $\frac{1 - x^3}{1 - 4x^3 - x^6}$, $\frac{x + x^4}{1 - 4x^3 - x^6}$, $\frac{2xx}{1 - 4x^3 - x^6}$, ſummæ tres Terminorum omnium binis intervallis a ſe diſtantium, incipiendo reſpective a primo, ſecundo & tertio Termino; atque eodem methodo colligere licet ſummas terminorum ternis vel quaternis vel quinis intervallis a ſe diſtantibus, ſive denominator ſit quadrinomium, vel multinomium quodcunque ex terminis finitis compoſitum.

PROPOSITIO III.

Si dividatur Unitas per Trinomium utcunque compoſitum ex datis 1, e, f, g, &c. *& indeterminata* x; *dico Terminum quemvis Seriei ex hac diviſione reſultantis, aſſignabilem fore.*

Sit Trinomium $1 - ex + fxx$ finge $xx - ex + f = 0$, ſint m & p, radices Æquationis; ſit $l + 1$ locus termini deſiderati, hoc eſt exprimat l intervallum inter primum Terminum & Terminum quæſitum, fac $A = \frac{m}{m - p}$ $B = \frac{p}{p - m}$. Et erit Terminus deſideratus $\overline{Am^l + Bp^l} \times x^l$.

Eodem modo ſi dividatur Unitas per quadrinomium $1 - ex + fxx - gx^3$, pone $x^3 - exx + fx - g = 0$, ſintque m, p, q, radices Æquationis, fac $A = \frac{mm}{\overline{m - p} \times \overline{m - q}}$, $B = \frac{pp}{\overline{p - m} \times \overline{p - q}}$, $C =$

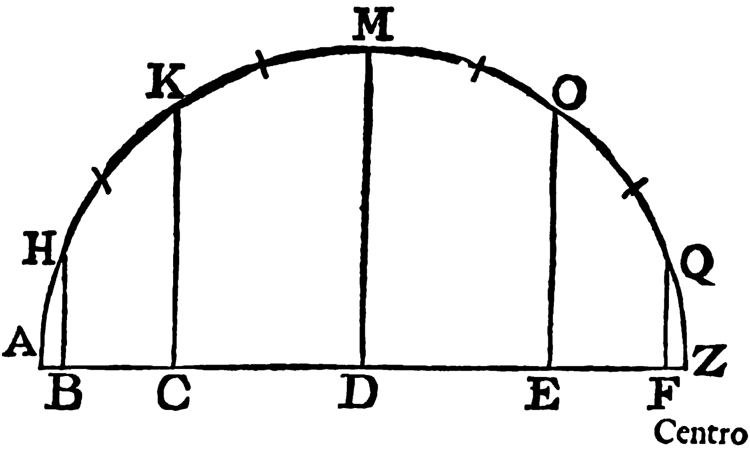
$\frac{qq}{\overline{q-m} \times \overline{q-p}}$. Et erit Terminus deſideratus $\overline{Am^l + Bp^l + Cq^l} \times x^l$, & lex eadem obtinet pro multinomiis quibuſcunque.

PROBLEMA.

A & B *quorum Dexteritates ſint in ratione data videlicet ut* a *ad* b, *ea conditione ludant, ut quoties* A *ludum unum vicerit,* B *tradat ipſi nummum unum: quoties vero* B *vicerit,* A *tradat ipſi nummum unum: & non prius ludo deſiſtant, quam eorum alter nummos omnes alterius lucratus fuerit; quæritur quantum probabile futurum ſit ut certamen intra datum ludorum numerum* x, *vel expirante illo numero, finiatur.*

Caſus Primus.

Sit *n* numerus nummorum quos uterque Colluſorum habeat; ſit etiam *n* numerus par, ponaturque *a* ad *b* habere rationem æqualitatis.



Centro D, Intervallo $DA = 1$, deſcribatur Semicircumferentia AMZ quæ dividatur in tot partes æquales quot ſunt unitates in n, tunc ex primo H, tertio K, quinto M &c. & impari quoque diviſionis termino, demittantur ad diametrum perpendicula HB, KC, MD, OE, QF &c. ponatur $Q = \frac{HB^{x+1}}{AB^{\frac{1}{2}x+1}} - \frac{CK^{x+1}}{AC^{\frac{1}{2}x+1}} + \frac{DM^{x+1}}{AD^{\frac{1}{2}x+1}} - \frac{EO^{x+1}}{AE^{\frac{1}{2}x+1}} + \frac{QF^{x+1}}{AF^{\frac{1}{2}x+1}}$ &c. donec ſinus omnes exhauriantur: quo facto, erit probabilitas certaminis finiendi intra ludos non plures quam x, ad probabilitatem non finiendi, ut $2^{\frac{1}{2}n-1}n - Q$ ad Q, accurate.

Corollarium I.

Si ſumatur pro Q Terminus primus $\frac{HB^{x+1}}{AB^{\frac{1}{2}x+1}}$ neglectis cæteris, habebitur approximatio ſufficiens niſi forte ſit x numerus valde exiguus.

Exemplum.

Sit n numerus nummorum quos uterque Colluſorum habeat $= 10$. Sit etiam $x = 76$. Si ſumatur pro Q primus terminus & negligantur cæteri, invenietur probabilitas certaminis finiendi intra ludos non plures quam 76 ad probabilitatem non finiendi ut 50747 ad 49235, ſi vero ſumatur pro Q termini duo priores neglectis cæteris, invenietur ratio probabilitatum ut 50743 ad 49247.

Corollarium II.

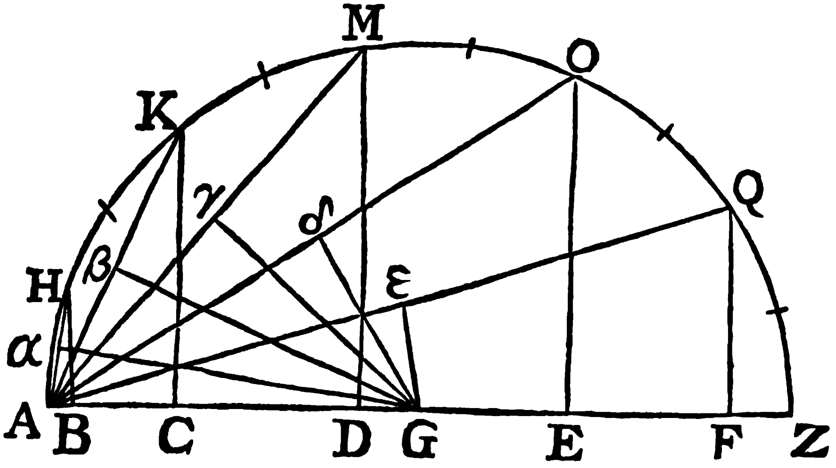
Invenire quotenis ludis, probabilitates certaminis finiendi & non finiendi erunt æquales.

Solutio.

Ponatur pro Q Terminus unicus $\frac{HB^{x+1}}{AB^{\frac{1}{2}x+1}}$, fiatque $2^{\frac{1}{2}n-1}n - Q = Q$. Et poſito n maximo numero invenietur, $x = 0.756\,nn$ proxime, aliquanto major quam $\frac{3}{4}nn$.

Caſus Secundus.

Sit n numerus impar, ponaturque a ad b habere rationem æqualitatis.



Centro G, intervallo G A deſcribatur ſemicircumferentia A M Z quæ dividatur in tot partes æquales, quot ſunt unitates in n; tunc ex primo H, tertio K, quinto M, & impari quoque diviſionis termino, demittantur ad diametrum perpendicula HB, KC, MD, OE, QF *&c.* ex diametri extremitate A, primo ſcilicet arcui contermina, ducantur ſubtenſæ AH, AK, AM, *&c.* ad quas e Centro G ducantur perpendicula $G\alpha$, $G\beta$, $G\gamma$, $G\delta$, $G\epsilon$, &c. ponatur

$$Q = \frac{BH^{x} \times G\alpha}{AB^{\frac{x+1}{2}}} - \frac{CK^{x} \times G\beta}{AC^{\frac{x+1}{2}}} + \frac{DM^{x} \times G\gamma}{AD^{\frac{x+1}{2}}} - \frac{EO^{x} \times G\delta}{AE^{\frac{x+1}{2}}} + \frac{FQ^{x} \times G\epsilon}{AF^{\frac{x+1}{2}}}$$

&c. quo facto, erit probabilitas certaminis finiendi intra ludos non plures quam x, ad probabilitatem non finiendi, ut $2^{\frac{x-3}{2}}n - Q$ ad Q accurate.

Corollarium I.

Si ſumatur pro Q terminus primus $\frac{HB^{x} \times G\alpha}{AB^{\frac{x+1}{2}}}$ neglectis cæteris, habebitur approximatio ſufficiens.

Exemplum.

Sit n numerus nummorum quos uterque Colluſorum habeat $= 45$. Sit etiam $x = 1519$. Sumatur pro Q terminus primus neglectis cæteris, & invenietur probabilitas certaminis finiendi intra ludos non plures quam 1519 ad probabilitatem non finiendi ut 49959 ad 50441, quæ proportio eſt vero proxima.

Cor

Corollarium II.

Invenire quotenis ludis probabilitates certaminis finiendi & non finiendi erunt æquales.

Solutio.

Ponatur pro Q Terminus unicus $\frac{HB^{x} \times G\alpha}{AB^{\frac{x+1}{2}}}$, fiatque $2^{\frac{x-3}{2}} n - Q = Q$; & poſito n magno numero, invenietur $x = 0.756\, nn$ proxime aliquanto major quam $\frac{3}{4}\, nn$ contra quam ſentiebat Clariſſimus Monmortius.

Caſus Tertius.

Poſitis cæteris ut in primo caſu, ſit a ad b ratio inæqualitatis (*vid. Fig.* 1.) Pone $\frac{a^{n}+b^{n}}{\overline{a+b}|^{n}} = L$, $\frac{\overline{a-b}|^{2}}{\overline{a+b}|^{2}} = d$, $\frac{ab}{\overline{a+b}|^{2}} = r$, Fac, 1, 2 r :: $\frac{HBq}{AB}$, m :: $\frac{CKq}{AC}$, p :: $\frac{MDq}{AD}$, q :: $\frac{OEq}{AE}$, s :: $\frac{QFq}{AF}$, t.

Pone $Q = \frac{HB}{2r\,AB + d}\, m^{\frac{1}{2}x} - \frac{CK}{2r\,AC + d}\, p^{\frac{1}{2}x} + \frac{MD}{2r\,AD + d}\, q^{\frac{1}{2}x}$ &c. quo facto erit probabilitas ludi finiendi intra ludos non plures quam x ad probabilitatem non finiendi ut $nr^{\frac{1}{2}n-1} - 2LQ$ ad $2LQ$.

Corollarium II.

Si ſumatur pro Q Tcrminus primus $\frac{HB}{2rAB+d}$ $m^{\frac{1}{2}x}$ neglectis cæteris, habebitur approximatio ſufficiens.

Caſus Quartus.

Poſitis cæteris ut iu ſecundo caſu, ſit a ad b ratio inæqualitatis (*vid. Fig.* 2.)

Pone quantitates L, d, r, m, p, q, s, t, &c. ut in tertio caſu.

Pone $Q = \frac{BH \times G\alpha}{2rAB+d} m^{\frac{x-1}{2}} - \frac{CK \times G\beta}{2rAC+d} p^{\frac{x-1}{2}} + \frac{DM \times G\gamma}{2rAD+d} q^{\frac{x-1}{2}}$ *&c.* quo facto erit probabilitas ludi finiendi intra ludos non plures quam x ad probabilitatem non finiendi ut $nr^{\frac{n-3}{2}} - 4LQ$ ad $4LQ$.

Corollarium.

Si ſumatur pro Q Terminus unicus $\frac{BH \times G\alpha}{2rAB+d}$ $m^{\frac{x-1}{2}}$ neglectis cæteris habebitur approximatio ſufficiens.

Quemadmodum in Progreſſione Geometricâ, Terminus quilibet ad proxime præcedentem habet rationem datam, ita ſunt aliæ Progreſſiones quæ ſic conſtitui poſſunt ut aſſumptis ad libitum Terminis duobus primis, Terminus quilibet ſubſequens ad duos proxime præcedentes habeat rationes datas, hujuſmodi eſt ſubjecta Series,

Corollarium II.

Invenire quotenis ludis probabilitates certaminis finiendi & non finiendi erunt æquales.

Solutio.

Ponatur pro Q Terminus unicus $\frac{HB^{x} \times G\alpha}{AB^{\frac{x+1}{2}}}$, fiatque $2^{\frac{x-3}{2}} n - Q = Q$; & poſito n magno numero, invenietur $x = 0.756\, nn$ proxime aliquanto major quam $\frac{3}{4} nn$ contra quam ſentiebat Clariſſimus Monmortius.

Caſus Tertius.

Poſitis cæteris ut in primo caſu, ſit a ad b ratio inæqualitatis (*vid. Fig.* 1.) Pone $\frac{a^{n}+b^{n}}{\overline{a+b}|^{n}} = L$, $\frac{\overline{a-b}|^{2}}{\overline{a+b}|^{2}} = d$, $\frac{ab}{\overline{a+b}|^{2}} = r$, Fac, $1, 2r :: \frac{HBq}{AB}, m :: \frac{CKq}{AC}, p :: \frac{MDq}{AD}, q :: \frac{OEq}{AE}, s :: \frac{QFq}{AF}, t$.

Pone $Q = \frac{HB}{2rAB+d} m^{\frac{1}{2}x} - \frac{CK}{2rAC+d} p^{\frac{1}{2}x} + \frac{MD}{2rAD+d} q^{\frac{1}{2}x}$ &c. quo facto erit probabilitas ludi finiendi intra ludos non plures quam x ad probabilitatem non finiendi ut $nr^{\frac{1}{2}n-1} - 2LQ$ ad $2LQ$.

Corol-

Corollarium II.

Si ſumatur pro Q Terminus primus $\frac{HB}{2rAB+d}$ $m^{\frac{1}{2}x}$ neglectis cæteris, habebitur approximatio ſufficiens.

Caſus Quartus.

Poſitis cæteris ut in ſecundo caſu, ſit a ad b ratio inæqualitatis (*vid. Fig.* 2.)

Pone quantitates L, d, r, m, p, q, s, t, &c. ut in tertio caſu.

Pone $Q = \frac{BH \times G\alpha}{2rAB+d} m^{\frac{x-1}{2}} - \frac{CK \times G\beta}{2rAC+d} p^{\frac{x-1}{2}} + \frac{DM \times G\gamma}{2rAD+d} q^{\frac{x-1}{2}}$ *&c.* quo facto erit probabilitas ludi finiendi intra ludos non plures quam x ad probabilitatem non finiendi ut $nr^{\frac{x-3}{2}} - 4LQ$ ad $4LQ$.

Corollarium.

Si ſumatur pro Q Terminus unicus $\frac{BH \times G\alpha}{2rAB+d}$ $m^{\frac{x-1}{2}}$ neglectis cæteris habebitur approximatio ſufficiens.

Quemadmodum in Progreſſione Geometricâ, Terminus quilibet ad proxime præcedentem habet rationem datam, ita ſunt aliæ Progreſſiones quæ ſic conſtitui poſſunt ut aſſumptis ad libitum Terminis duobus primis, Terminus quilibet ſubſequens ad duos proxime præcedentes habeat rationes datas, hujuſmodi eſt ſubjecta Series,

$$\begin{array}{cccccc} A & B & C & D & E & F \\ 1 & +3x & +7xx & +17x^3 & +41x^4 & +99x^5, \end{array}$$ &c. in qua

$C = 2Bx + 1Axx$

$D = 2Cx + 1Bxx$

$E = 2Dx + 1Cxx$

$F = 2Ex + 1Dxx$ &c.

Quantitates autem Numerales $2+1$ *ſimul ſumptas ſubque propriis ſignis connexas appellare licet Indicem Relationis.*

Eodem modo conſtitui poſſunt ſeries aliæ in quibus aſſumptis ad libitum Terminis tribus primis, Terminus quilibet ſubſequens ad tres proxime præcedentes habeat rationes datas; hujus generis eſt ſubjecta Series.

$$\begin{array}{cccccc} A & B & C & D & E & F \\ 1 & +2x & +3xx & +10x^3 & +34x^4 & +97x^5 \end{array}$$ &c. in qua

$D = 3Cx - 2Bxx + 5Ax^3$

$E = 3Dx - 2Cxx + 5Bx^3$

$F = 3Ex - 2Dxx + 5Cx^3$ &c.

Quantitates autem Numerales $3-2+5$ *ſimul ſumptæ ſubque propriis ſignis connexæ, componunt Indicem Relationis.*

Sunt aliæ ſeries in quibus Relatio ſit ad quatuor, vel ad quinque, vel ad ſex Terminos præcedentes, &c.

Series autem omnes hujus generis recurrentes appellare licebit propter Relationem Terminorum perpetuo recurrentem.

PROBLEMA II.

In ſeriebus recurrentibus, ex datis Terminis duobus primis, ſi relatio ſiat ad duos præcedentes; vel *datis Terminis tribus primis, ſi relatio ſiat ad tres præcedentes,* &c. *dato etiam indice relationis, invenire ſummam Terminorum quotlibet quorum numerus datus ſit.*

Problema ſolvitur in Tractatu noſtro qui inſcribitur, *The Doctrine of Chances.*

PROBLEMA III.

Aſſumptis ad libitum ſeriebus quotcunque recurrentibus; Terminiſque, iiſdem intervallis a principio ſerierum diſtantibus, in ſe invicem multiplicatis, invenire ſummam ſeriei ex hac multiplicatione reſultantis.

INVESTIGATIO.

I° Proponantur ſeries duæ, ſitque $m + n$ Index Relationis in prima ſerie, atque $p + q$ Index Relationis in ſecunda, ex primo Indice $m + n$, formetur Æquatio $xx - mx - n = 0$, ex ſecundo Indice $p + q$, formetur Æquatio $yy - py - q = 0$, pone $xy = z$. Atque ope trium iſtarum Æquationum, expungantur x & y, & orietur Æquatio

$$z^4 - mpz^3 \begin{array}{l} - mmq \\ - npp \\ - 2nq \end{array} zz - mnpqz + nnqq = 0.$$

in qua deleto primo termino z^4, mutatis ſignis omnibus, atque poſito $z = 1$, obtinebitur Index Relationis, quo obtento, ſeries reſultans facile ſummabitur; II° eodem modo procedere licet, ſi dentur ſeries tres vel quatuor &c. recurrentes.

Dum ſuperiores paginæ prælo ſubjiciebantur, incidi fortuito in Acta Leipſ. *annorum* 1702 *&* 1703, *quibus comperi Cla.* Leibnitium *eadem fere methodo ante me uſum fuiſſe qua hic utor in reducendis Fractionibus Algebraicis ad ſimpliciores, quod monitum velim ut a me avertam vel minimam ſuſpicionem, aliena mihimet arrogare voluiſſe; Propoſitio autem qua id præſtitimus æque ac Propoſitio noſtra tertia, ambæ deducuntur tanquam Corollaria ex altera Propoſitione maxime generali quam exhibuimus coram Regali Societate,* Maii 5° 1720; *Propoſitio ſic ſe habet.*

Data ſerie quâvis recurrente quarum Termini quotlibet primi ad libitum ſumantur; dato etiam Indice Relationis Terminorum ſequentium ad præcedentes, invenire Terminum quemlibet aſſignatum in hac ſerie, priuſquam ſumma ſeriei ſit cognita.

Qui autem rite perſpexerit Solutionem Problematis hic adducti, is utique percipiet illam pendere a Propoſitione noſtra generali, cujus demonſtrationem ſimulque modum inveſtigationis brevi ſpero publici juris faciam.

IV. *Defenſio Diſſertationis de Motu Aquarum fluentium, in Actis Philoſophicis,* N° 355. *editæ, contra Animadverſiones Viri Cl.* Petri Antonii Michelotti. *Auctore* Jacobo Jurin, *M. D. Coll. Med. Lond. Soc. in Theatro Publico Chirurg. Lond. Prælectore Anatomico, & Soc. Reg. Secr.*

IN pervolvendo Opere pererudito Viri Clariſſimi, *Petri Antonii Michelotti,* de ſeparatione fluidorum in corpore animali, qui *Venetiis* nuper ad nos delatus eſt, pluribus in locis Diſſertationem meam de Motu aquarum fluentium, in *Actis Philoſoph.* N° 355. ante aliquot annos editam, non leviter notatam deprehendi. Cum autem alia ex iis, quæ reprehendit Vir Doctiſſimus, ex minus perſpecto Diſſertationis meæ inſtituto profecta videantur; alia vero ita demonſtrari poſſint, ut ipſum Virum Cl. ea mihi non difficulter conceſſurum putem: operæ pretium erit, ſi primo in loco Diſſertationis meæ propoſitum & conſilium paulo luculentius exponam; quod cum fecero, ad reliqua deinceps breviter expendenda progrediar.

Principio igitur explicandum eſt, quid in Diſſertatione iſtâ intelligendum velim, per *Motum Aquæ ex imi vaſis foramine defluentis.* Eſt enim alius *Motus,* ſive *quantitas Motûs,* Aquæ, quæ ex vaſe per foramen delabitur: qui *Motus* eſt in ratione compoſitâ, ex ratione quantitatis aquæ dato quovis tempore effluentis ex foramine, & ratione velocitatis, quâcum effluit. Alius vero eſt *Motus* totius aquæ, ſeu Cataractæ aqueæ, quæ intra vas verſus foramen deſcendit, & mox effluxura eſt. Hic eſt in ratione ſummæ omnium factorum, ex ſingulis aquæ particulis, Cataractam conſtituentibus, ductis in velocitates earundem reſpectivas. Quorum Motuum cum alterum ſæpe pro altero accipi viderem, animus mihi erat poſteriorem illum in prædictâ Diſſertatione illuſtrare, ad calculum revocare, & liquoribus in Animalium corpore fluentibus applicare.

Hic ergo cum ſemper mihi intelligeretur per *Motum Aquæ defluentis*, ſive per *Motum Aquarum fluentium*, quod ex omnibus meis Propoſitionibus luculenter apparet, jure meo dicere poteram *Motum hunc a nemine adhuc, quod ſcirem, fuiſſe determinatum:* quippe quem nemo Mathematicorum, quos quidem ego viderim, nec etiam verbo tenus attigerit. Quod cum ita ſit, miror profecto non animadvertiſſe neque acutiſſimum *Michelottum*, nec etiam ſubtiliſſimi & perſpicaciſſimi Ingenii Virum, *Johannem Bernoullium*, me in illius Diſſertationis Procemio, quod toties citat & tantopere reprehendit *Cl. Michelottus*, ne verbum quidem ſcripſiſſe de velocitate, quâcum aqua effluit ex foramine, multo minus de Bernoullianâ determinatione illius velocitatis. Hoc ſi perſpexiſſet Vir Cl. noluiſſet ſane, pro ſuâ humanitate, tam inclementer & inique mecum agere, ut me *Bernoullianam Demonſtrationem extenuare verbis conari* * diceret, & *meram eſſe cavillationem id, quod Bernoullio objiciam.* Quod vero ſubjicit, verba iſta mea, " fieri omnino " non poſſe, ut Motus aliquis cum pondere quieſcente " conferatur," *ne umbram quidem habere rationis contra Bernoullianam Demonſtrationem pugnantis,* libens agnoſco, quippe qui, cum iſta ſcriberem, tantum de Cl. Bernoullio, quantum de Sinenſium Imperatore cogitabam. Aio autem Lectorem quemvis non iniquum, neque præjudiciis occupatum, ne *umbram quidem* veriſimilitudinis reperturum, quod iſta verba ad Bernoullianam Demonſtrationem quicquam pertineant; quibus ſcilicet de re longe diversâ agatur, nempe de quantitate Motûs totius aquæ verſus foramen contendentis. Quoniam vero ita penitus inſedit animo Viri Doctiſſimi illa Demonſtratio, eandem in illius gratiam, ubi prius meipſum ab reliquis ejus Animadverſionibus vindicavero, ad examen revocare decrevi.

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* Pag. 131, 132.

Ad *Motum* prædictum definiendum non alio nobis opus erat, quam Theoremate noſtro generali, quod tertio loco poſuimus: ſed cum Curvæ Hyperbolicæ Newtonianæ proprietatem, quâ Cataractam aquæ deſcendentis format, non indignam cenſeremus contemplatione Geometrarum, voluimus obiter quædam de Cataractâ illâ præmittere. Hanc autem ex Viri Incomparabilis, *Prop.* 36. *Lib.* 2. *Princip.* deſumptam proponebam, non ut *ex Tripode editam*, ſed evidentiâ Mathematicâ, omnibus oraculis certiore, munitam.

Quod enim Cataracta talis formari debeat ex aquâ libere deſcendente, & acceleratâ in modum corporum omnium gravium, quam nullâ aliâ aquâ circumcingatur, aperta res eſt; ut patet *Newtoni* Propoſitionem attente perlegenti. Si etiam Cataracta glacie concavâ, figuræ Cataractæ aqueæ adamuſſim congruente, & propter ſummam polituram nullam reſiſtentiam adferente ambiatur; ea glaciem ne minimâ quidem vi premet, ſed tanget ſolum liberrime cadendo, unde nihil mutabitur non modo in figurâ, ſed nec etiam in velocitate Cataractæ deſcendentis. At ſi circumpoſita Glacies in aquam reſolvatur, neutiquam jam opus eſt tanto *Machinarum* & *Arietum validiſſimorum* apparatu,

ratu, quos magno molimine ‡ adduxerunt tum ipſe *Vir Cl.* tum etiam Geometra Eximius, *Johannes Bernoullius*, ad *fragilem* noſtram *Cataractam confringendam & comminuendam*; quippe quam ipſe prius *Newtonus* hiſce verbis, * *Liqueſcat jam glacies in vaſe*; &c. diſſolverit penitus ac diſſipaverit. Nullo igitur aut *Genio* nobis opus eſt, aut *Erythræi Maris Miraculo*, ad Cataractam iſtam ſive indicandam, ſive conſervandam, quippe qui non adeo uſque ſtolidi aut inſulſi ſimus, ut conſervatum iri eam ſperemus ab omni aquæ circumpoſitæ communione puram & illibatam. Ignoſcat autem nobis, pro æquitate ſuâ, Cl. *Michelottus*, quod, quæ Providentiſſimus *Newtonus* de glacie ambiente, & eâdem poſtea in aquam reſolutâ fuſius tradiderit, ea noſmet Lectorem ex ipſo potius Cl. Auctore petere voluerimus, quam ingratâ & minime nobis neceſſariâ repetitione detineri.

Non diffitemur ſane paulum diſcriminis, ut id obiter notemus, inter caſum a *Newtono* poſitum & noſtrum intereſſe. Quem enim fingit ille Cylindrum glaciei, velocitate datâ uniformi deſcendentem, ac liqueſcentem & in aquam converſum, quam primum ſuperficiem attingit aquæ vaſe contentæ, in eum ſcilicet finem, ut vas ſemper æque plenum conſervetur; hunc nos omiſimus, & ejus loco ſuperficiem aquæ infinitam poſuimus, ut eâ ratione integrum Solidum, ſive Cataractam Hyperbolicam repræſentaremus. At hæc poſitio nihil mutat neque in velocitate, nec in *Motu* aquæ decurrentis.

Quod autem † ait Vir. Cl. *me ſumere, quod eſt in contentione*, & paulo infra, *ceſſare igitur quæſtionem, & totam Demonſtrationem abire in Hypotheſin*, non mehercule intelligo, quid ſibi velit.

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‡ Pag. 128, 129, 130. * Pincip. *Pag.* 304. † *Pag.* 127.

Mihi enim, in loco citato, nulla movebatur quæſtio de velocitate aquæ effluentis, nec demonſtrationem ullam de eâ velocitate adferebam, ſed id unum agebatur, ut ex poſitâ illâ velocitate Æquationem Curvæ Hyperbolicæ Newtonianæ deducerem. Velocitatem nempe aquæ effluentis jam antea determinaveram, vel etiam, ſi placet, ſumpſeram, poſitis ſcilicet iis, quæ a *Newtono* poſita fuerant, aquam nempe gravitatis vi libere cadere, & inter cadendum accelerari. Hoc autem quî fieri poſſet, prius tradiderat *Newtonus*, ponendo aquam per glaciem politiſſimam ambientem, vel etiam per eandem in aquam ſolutam, ſed quietem adhuc ſervantem, tanquam per infundibulum, ſine ullâ reſiſtentiâ tranſire ; quod in eum finem ponebatur, ut ſimplicior & magis Mathematica redderetur Problematis ſolutio.

Libet hic loci, propter argumenti affinitatem, erroris meminiſſe, cujus *Newtonum*, *Hugenium*, *Keillium* temere nimis, uti nobis videtur, ex Bernoullianæ demonſtrationis fiduciâ * incuſat *Cl. Michelottus* ; quod ſcilicet vim, quâ totus aquæ exilientis *Motus* generari poteſt, æqualem ſtatuerint ponderi Cylindricæ columnæ aquæ, cujus baſis eſt foramen, cujuſque altitudo dupla eſt altitudinis aquæ vaſe contentæ. Hanc paucis admodum verbis, nec tamen idcirco minus perſpicue, demonſtravit *Newtonus* in Corollario ſecundo Propoſitionis ſupradictæ. Potuiſſet alia quoque deduci demonſtratio ex contemplatione Cataractæ integræ Hyperbolicæ, quæ huic Cylindro æqualis eſt, cujuſque pondus totum in aquæ deſcenſum impenditur : ſed hâc minime opus eſt, cum idem ex ipsâ Propoſitione Bernoullianâ, quam toties laudat, ac tam vehementer defendit *Cl. Michelottus*, apertiſſime ſequatur. Id nullo negotio animadvertet *Vir Doctiſſimus*, ſi ſepoſitâ parumper Columnæ foramini incumbentis conſideratione, calculo inſtituto, ex mole aquæ dato quovis tempore ex foramine effluentis, & ex velocitate, quâcum aquam effluere ſtatuit *Cl. Bernoullius*, ejus aquæ *Motum* determinare voluerit, & deinde pondus invenire, quod eodem dato temporis ſpatio, libere cadendo gravitatis vi, eandem *Motûs* quantitatem generare poſſit. Hoc autem pondus reperiet ponderi duplæ Columnæ aquæ foramini inſiſtentis æquale, prorſus uti definivit *Newtonus* in Corollario prædicto. Idem vero pondus, alteri Libræ Radio appenſum, ab impetu aquæ, cum primum ex foramine effluit, continuato rivo in alterum Libræ æqualem Radium impingentis, atque ſtatim poſt impulſum delabentis, in quiete ſuſtinebitur ; quod poſito calculo facile patebit.

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* Pag. 112, 113.

Videor mihi non malam gratiam a *Doctiſſimo Michelotto*, pro candore ſuo, initurus, ſi altero inſuper † præjudicio, quo & alios plures teneri video, ipſum liberavero. *Newtonus*, *Prop*. 37. *Lib*. 2. *Princip. primæ editionis*, aquam demonſtravit ex foramine in fundo vaſis eâ cum velocitate erumpere, quâ aſſurgere poſſit ad dimidiam altitudinem aquæ in vaſe exiſtentis. Demonſtrandi rationem nemo refellit : concluſionem plures redarguunt. Experientia, inquiunt, contradicit, quâ deprehenditur aqua exiliens ad totam altitudinem aſſurgere : quin etiam *Newtonus* ipſe in Problematis ejuſdem ſolutione, *Prop*. 36. *lib*. 2. *editionis ſecundæ*, eam tribuit aquæ velocitatem, quâ ad totam altitudinem proſilire poſſit ; adeoque ipſe ſibi contradicere videtur. Atqui ſi res iſta accuratius & cum judicio perpendatur, reperietur primæ ſolutioni *Newtonianæ*

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† Pag. 113.

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& cum ſecundâ, & cum experientiâ ipsâ, optime convenire. Nam in ſecundâ ſolutione, aquæ venam exilientem, ad parvam a foramine diſtantiam, contractiorem diametro ſtatuit *Vir Perſpicaciſſimus*, quam in ipſo foramine, in ratione 21 ad 25. Eſt itaque ſectio venæ in eâ diſtantiâ, ad foramen ipſum, ut 21 × 21, ad 25 × 25, h. e. ut 1 ad $\surd 2$ proxime. Cumque eadem aquæ quantitas, ſive per foraminis, ſive per venæ contractæ ſectionem, dato tempore perfluat, & proinde velocitates aquæ in iis ſectionibus ſint in ratione ipſarum ſectionum reciprocâ; erit velocitas in foramine ad velocitatem venæ contractæ, ut 1, ad $\surd 2$: proinde, ſi ea ſit velocitas venæ contractæ, quâ aqua proſiliat ad integram altitudinem aquæ in vaſe, non major erit aquæ velocitas in ipſo foramine, quam quâ ad dimidiam altitudinem deferatur. Conſentiunt itaque inter ſe hæ duæ ſolutiones; & experientia porro cum iiſdem conſentire deprehenditur. Nam ſi per alterutram earum ſolutionum, ex definitâ velocitate, quâ aqua, ſive per foramen, ſive per venam contractam, tranſire ſtatuitur, calculo inſtituto inveniatur quantitas aquæ effluxuræ; reperietur eadem cum quantitate aquæ, quæ per experimenta effluere deprehenditur, proxime convenire. Certe experimentum ab ipſo *Illuſtriſſimo Newtono* ſumptum, adhibito foramine, cujus diameter erat quinque octavarum digiti partium, huic calculo reſpondit; ut etiam alia plura experimenta minoribus diametris *Londini* facta, quibus ipſe cum pluribus Regiæ Societatis Sodalibus, ante aliquot annos operam dedi. Abludunt quidem aliquantum *Diligentiſſimi* * *Poleni* experimenta, ſed tamen minorem aquæ quantitatem exhibent, quam ſecundum hunc calculum, nunquam majorem, forte quod anguſtiora fuerint vaſa pro ratione amplitudinis foraminum.

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* Polen. de Caſtellis.

Supereſt adhuc nobis conſideranda * Animadverſio una, ſive potius Scrupulus *Viri Cl.* ex eo natus, quod in *Coroll.* 17. *Theorem.* 3. Diſſertationis prædictæ majorem ſtatuimus *Motum*, ſive *Impetum*, ſanguinis in Arteriis omnibus capillaribus ſimul ſumptis, quam in ipsâ Aortâ. Hoc ut explicet *Vir Doctiſſimus*, neſcio quam Hypotheſin nobis affingit, de majore ſanguinis denſitate in capillaribus Arteriis, quam in Aorta. Nos vero nullam ejuſmodi conditionem poſuimus, ſed Corollarium deduximus ex Theoremate præcedente, in quo agitur de *Motu* aquæ per Canalem plenum quemcunq; fluentis: unde patet ſanguinem non aliter conſiderari in noſtris Corollariis, quam quatenus fluidus eſt & aquam æmulatur. Sed patet inde provenire Scrupulum *Viri Cl.* quod per ſanguinis *Impetum* intelligat, *quantitatem Motûs ejus effectam ex multiplicatione velocitatis per maſſam dato tempore tranſfluentem.* Atqui hic longe alius eſt ac noſter ſanguinis *Motus*, ſive *Impetus*, quippe qui in iſto Theoremate *æqualis* ſtatuitur *Motui molis aquæ, quæ dato quovis tempore effluit ex Canali, cujuſque ea ſit volocitas, quâ percurratur eodem dato tempore ſpatium æquale longitudini Canalis.* Facile autem ex hoc Theoremate fluit Corollarium prædictum, quippe cum dato tempore transfluat eadem ſanguinis moles per Aortam & per Arterias capillares, major autem ſit Canalis longitudo ex Aortâ & Arteriis capillaribus compoſiti, quam Aortæ ſolius. Hoc eo libentius notavi, quod videam non ſolum *Doctiſſimum Michelottum*, ſed alios etiam ſcriptores Mathematicos, pluribus in locis, ubi agitur de potentiis, quæ liquorem per Canales eodem plenos aut in motum impellunt, aut effluentem ſiſtunt, nihil aliud conſiderare præter molem

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* Pag. 101, 102.

& velocitatem fluidi effluentis; quum debuiſſet etiam longitudinis ipſorum Canalium ratio haberi. Nam cæteris paribus, eo difficilius vel expellitur fluidum ex pleno Canali, vel in effluxu ſiſtitur, quo Canalis longior fuerit; quippe quum tota moles fluidi Canale contenti in motum concitandus ſit, priuſquam ulla pars ejuſdem effluere poſſit ex orificio; ſicuti etiam tota eadem moles neceſſario ſiſtenda eſt, ſi exitum parti jamjam effluxuræ prohibere volueris.

Accedo jam ad expendendam Viri Celeberrimi, *Johannis Bernoullii*, Demonſtrationem de velocitate aquæ ex foramine vaſis pleni effluentis. In quem finem legi diligenter ac relegi, tum quæ protulit *Doctiſſimus Michelottus* de ‡ principiis illius Demonſtrationis, tum ipſam Demonſtrationem a *Cl. Hermanno* communicatam in *Actis Lipſienſibus*, Anni 1716. Quæ quamvis nullâ ex parte mihi ſatisfaciat, tamen cum imbecillitatis meæ conſcius longe facilius accidere poſſe ſentiam, ut ipſe a vero aberrem, quam ut Virum nobiliſſimis inventis clarum, & acerrimo, ſi quis alius ingenio pollentem, erroris alicujus redarguam; cunctanter idcirco & dubitantius proponam, quid in illâ Demonſtratione minus firmum mihi videatur.

“ Fundamentum Demonſtrationis (*ſcribit Vir Cl.*)
“ in hoc conſiſtit, ut conſideretur guttula liquoris infima,
“ & foramini vaſis immediate incumbens, tanquam
“ preſſa, vel (ut ego voco) animata a gravitate
“ quâdam acceleratrice quæ ſe habet ad gravitatem naturalem
“ ut altitudo aquæ vel liquoris totius foramini
“ vaſis incumbentis ad altitudinem guttulæ, ſcilicet ut
“ pondus abſolutum columnæ aquæ foramini inſiſtentis

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‡ Pag. 131.

" ad pondus abſolutum guttulæ; Sic quippe nihil aliud
" reſtat, quam ut quæratur quantam velocitatem acqui-
" rere poſſit guttula animata ab iſtâ gravitate majori
" quando cadit per lineolam ſuæ altitudini æqualem,
" hoc eſt, poſtquam tota exierit per foramen; tam diu
" enim premitur a totâ columnâ aqueâ adeoque anima-
" tur a gravitate majore quamdiu aliquid de guttulâ
" (quam ut columellam ſolidam concipio) ſupra foramen
" exiſtit."

Poſito hoc fundamento pergit *Vir Cl.* ad Demonſtrationem ſuam concinnandam: nobis vero ſuſpecta eſt ipſius fundamenti firmitudo. Ut id quo jure fiat, videatur, ita, ſi placet, procedamus.

Quoniam nullâ aliâ re utitur *Cl. Bernoullius*, ad animandam, ut vocat, guttulam infimam gravitate prædictâ acceleratrice, niſi ſolâ preſſione, ſive pondere, columnæ aqueæ foramini inſiſtentis; congelari ponatur omnis aqua columnam illam ambiens, & columna aquea per politiſſimam glaciem ſine omni reſiſtentiâ labi concipiatur. His poſitis, quamdiu foramen clauſum tenetur, urgebitur ſane guttula foramini proxima toto pondere columnæ aqueæ incumbentis, prorſus uti ſtatuit *Cl. Bernoullius*.

Reſeretur jam foramen, & permittatur liber exitus aquæ effluxuræ. Quid deinde futurum cenſes? Num urgebitur, vel *animabitur guttula infima gravitate acceleratrice, quæ ſe habet ad gravitatem naturalem, ut altitudo aquæ totius foramini incumbentis, ad altitudinem guttulæ.* Minime vero; ſed urgebitur ſolâ gravitate ſuâ acceleratrice naturali. Nam quam primum guttula infima moveri deorſum incipit, etiam velocitate, ſi placet, infinite parvâ, non amplius utique urgebitur a pondere Columnæ aqueæ inſiſtentis. Fieri enim non poteſt, ut Columna aquea guttulam ſubjectam premat, niſi ab illâ guttulâ impediatur in deſcenſu. Non autem impeditur, quia non conatur velocius deſcendere, quam infima guttula gravitate ſuâ deorſum fertur; ſed columna & gutta pari paſſu deſcendunt, adeo ut gutta neque columnam deſertura ſit, nec ab eâdem ullam vim aut preſſionem ſit paſſura.

Cedit itaque, ni fallor, & fatiſcit *Bernoullianæ* Demonſtrationis Fundamentum: ſed circumſpicienti mihi, quidnam potiſſimum tanto Viro occaſionem dederit a vero aberrandi, id præcipue occurrit, quod ſcilicet minus animum intenderit *Vir Acutiſſimus* ad diſcrimen, quod eſt inter corpus preſſum a pondere incumbente, quum pondus iſtud non niſi a naturali Gravitatis vi acceleratrice urgetur, & corpus impulſum, ſive *animatum* (quoniam iſto verbo uti voluit *Vir Cl.*) a Gravitatis vi acceleratrice præter naturam auctâ. In caſu poſteriore deſcendet corpus majore velocitate, quam quæ ex Gravitate naturali proficiſci queat, prorſus ex ſententiâ *Doctiſſimi Bernoullii*: at in priore, utut corpus preſſum, dum quieſcit, urgeatur a pondere incumbente, tamen ubi primum deſcendere incipiet, eâdem prorſus velocitate deſcendet, ac ſi prius nullo pondere incumbente preſſum fuiſſet.

Neſcio an operæ pretium ſit, rem per ſe ſatis claram exemplo illuſtrare.

Quieſcere ponatur in menſâ columna ſolida ex centum Aureis ſibi invicem impoſitis confecta, & urgeatur, ut fit, Aureus infimus pondere Aureorum incumbentium. Si fiat jam foramen in menſâ ſubter Aureos, ut labi ſinatur Aureus infimus: quamprimum iſte Aureus deſcendere incipiet, liberabitur ſtatim ab Aureorum incumbentium pondere, & eâdem velocitate deſcendet tum Aureus infimus, tum reliqui omnes, ac ſi ſolus ille Aureus in menſâ conſtitutus fuiſſet.

Mitto dicere, quod, ſi quis ex velocitate, quâcum aqua ſecundum *Cl. Bernoullii* placita ex foramine egreditur,

greditur, & ex determinatâ per eam velocitatem mole aquæ dato quovis tempore effluentis, *Motum* ejuſdem, ut ſupra monui, definire voluerit, eundem duplo majorem reperturus ſit, quam qui ex pondere Columnæ aqueæ foramini inſiſtentis, eodem tempore, Gravitatis vi generari queat. Profectò videntur iſta mihi tantam veri ſpeciem præ ſe ferre, ut multum debiturus ſim ſive *Cl. Michelotto*, ſive ipſi Demonſtrationis Auctori Celeberrimo, ſi me aliquid rectius docere dignabitur.

Liceat interim ipſis, pace tantorum Virorum, ſequentia duo Experimenta, ad controverſiam iſtam certius dijudicandam, vel de novo inſtituenda, vel ſaltem diligenter expendenda commendare. Alterum *Newtonianum*, *pag.* 305. *Princip. ſecund. Ed.* deſcriptum ; ut inveniatur, ex mole aquæ dato temporis ſpatio effluentis, velocitas, quâcum tranſit per ipſum foramen : alterum *Cl. Mariotti, Libro Du Mouvement des Eaux*, *Part.* 2. *Diſc.* 3. *Regl.* 1. quod tubo Cylindrico, utrinque aperto, parte inferiore ſurſum reflexo, & aquâ pleno ſumptum eſt ; unde facile æſtimari poſſit, utrum guttulæ primæ aquæ effluentis ad tantam altitudinem proſiliant, quantam requirit *Bernoulliana* Demonſtratio.

V. *An Account of a remarkable Inſtance of the Infection of the Small Pox. By Dr.* Jurin, *Soc. Reg. Secret.*

A Young Gentleman ill of the Small Pox, of that ſort which is call'd the cohærent, or the middle between the diſtinct and the confluent Kind, on *Wedneſday Oct.* 3d 1722, being the ſixth Day from the Eruption, grew delirious in the Night, and got out of bed in ſpite of the Oppoſition of two Nurſes that attended him; and ſeizing one of his Nurſes by the Neck with his bare Arms, he preſſed her Forehead againſt his naked Breaſt, then cover'd with the Small Pox in the State of Maturation, and held her for ſome time in that Poſture. She was heated by ſtriving with him, and in ſtruggling to get looſe, ſhe was ſenſible that ſhe bruis'd and broke ſome of the Puſtules with her Forehead. The Woman was about 40 Years of Age, of a clear, florid, ſanguine Complexion; ſhe told me ſhe had had the ſmall Pox, when ſhe was about ſeven or eight Years of Age, and had been pretty full of them, but I ſaw no Marks upon her Face. On *Friday* Morning the Small Pox began to appear upon her Forehead, and increas'd by Degrees to between 50 and 60; ſhe had likewiſe a ſmall Number of Puſtules upon the back part and ſides of her Neck, where the Gentleman had graſp'd her with his naked Arms; but had none, as ſhe told me, upon any other Part of her Body. The lower Part of her Face was perfectly clear, and thoſe upon her Forehead were chiefly confin'd to the middle and moſt prominent Part of it, which had been preſs'd againſt the Gentleman's Breaſt. They roſe gradually, and came to Maturity, in the ſame manner as the Small Pox of the milder cohærent Kind uſe to do, with a great Inflammation and Swelling of her Forehead, and the adjoining Part of her Face, eſpecially between the Eye-brows, where a ſmall Cluſter of the Puſtules were ſeated, inſomuch that on *Tueſday* the 9th of *October*, her right Eye was quite clos'd up, and the left almoſt in the ſame Condition; but all this Time ſhe had no Fever, Sickneſs, or other Symptom of the Small Pox, beſide this Eruption, and the Inflammation, and Pain that attended it. That Night ſhe caus'd a Bliſter to be applied to her Neck, upon which ſhe recover'd the Sight of her Eye the next Day, being the ſixth from the Eruption, when the Puſtules were turning, and beginning to ſcab. The Scabs agreed with thoſe of the milder cohærent Small Pox in their Appearance and Duration. I ſaw her hitherto every day, as likewiſe at ſeveral times after this, and particularly on *Monday Oct.* 22d, which was the eighteenth Day from the Eruption of the Puſtules, when ſhe had ſtill ſome ſmall Part of the Scabs remaining upon her Forehead.

In this Inſtance it is worthy of Remark: 1ſt, That this Woman, tho' ſhe had had the Small Pox before, was yet infected again by the immediate and cloſe Application of the varioſous Matter to her Skin, when her Body was heated with Exerciſe. Which ſeems to prove, that ſuch an Application is more effectual to give the Infection than the bare morbid *Effluvia*, ariſing from the Body of the ſick Perſon, and received into the ſound one by Inſpiration; for that ſhe receiv'd no Infection by Inſpiration is plain, from the appearing of the Small Pox upon thoſe Parts only where there had been ſuch an

Application and Contact. From which it appears very probable, that a Perſon who has already had the Small Pox, as the Man inoculated by Mr. *Tanner* in St. *Thomas*'s Hoſpital, may poſſibly receive it again in ſome ſlight Degree by Inoculation; that being ſtill a more cloſe and immediate application of the Variolous Matter to the Blood and Juices of the ſound Perſon, than when it is applied only by Contact to the Skin whole and unwounded.

2dly, That the Infection communicated to this Woman not being univerſal, as appears from her having no Fever, or Sickneſs, or general Eruption of the Puſtules all over her Body, but only on the Parts infected by immediate Contact, no Argument can be drawn from hence, for a Perſon's being liable to undergo the Small Pox a ſecond time, ſo as to have the uſual Symptoms of that Diſeaſe, and a general Eruption of the Puſtules; but rather the contrary.

3dly, That the Time in which this Infection ſhew'd itſelf, by the appearance of the Puſtules, is very different from that obſerved upon Inoculation; the firſt appearing in about a Day and an half; whereas in the latter Caſe, the Eruption generally ſhews itſelf on the tenth Day, or not above a Day ſooner or later, as appears from the accurate and curious Obſervations of Dr. *Nettleton*. Which difference is what ought in reaſon to be expected, ſince in one Caſe the Infection went no further than the Parts affected by immediate Contact; whereas in the other it muſt be propagated thro' the Maſs of Blood to all Parts of the Body.

VI. *An Account of two Obſervations upon the Cataract of the Eye; contain'd in a Letter from Signor* Antonio Benevoli, *Maſter-Surgeon in the Hoſpital of* S. Maria Nuova *in* Florence, *to Dr.* Valſalva; *Printed in* Italian *at* Florence, *this preſent Year, and communicated to the* Royal Society, *at the Deſire of the Author, by Sir* Thomas Dereham, *F. R. S.*

IN this Letter the learned Author informs Signor *Valſalva*, that, on the 13th of *July* in the Year 1720. he had couch'd a *German* Soldier of Cataracts in both his Eyes, who immediately after the Operation recover'd the Sight of both Eyes, and continued to ſee till his Death, which happen'd of an Acute Illneſs on the 6th of *April* in this preſent Year. Upon this, Signor *Benevoli* took the Eyes out of their Orbits, in order to examine whether the Cataracts, which this Soldier had been couch'd of, conſiſted of a membranous Pellicle, as ſome Writers maintain; or, as others pretend, of a preternatural Opacity in the Cryſtalline Humour. Proceeding immediately to the Diſſection of the left Eye, upon a careful and very exact Examination of all the Contents of it, he could not find any ſuch thing as a Pellicle within it, but diſcover'd a ſmall yellowiſh Body at the bottom of the Bulb of the Eye, of a lenticular Shape, without Adheſion to any of the other

Parts of the Eye, which, upon taking it out, appear'd to be the Cryſtalline Humour grown opake, and ſomething leſs than its natural Size, having two or three ſmall Dents, or Impreſſions, made in its Circumference, which it had received from the Needle during the Operation of couching.

The next Day he examin'd the right Eye in the ſame manner, in the Preſence of ſeveral eminent Phyſicians and Surgeons, and other curious Perſons, whom he had invited to the Demonſtration, and found in it the Cryſtalline grown opake and depreſſed in the ſame manner as the former, to the bottom of the Eye, ſtill carrying the Marks of the Needle evidently upon it, but found no Pellicle within the Eye, notwithſtanding the moſt diligent Search he could make for it. Signor *Benevoli* farther relates, that having formerly made ſome Experiments upon the Eyes of dead Subjects, at *Bologna*, in company with Dr. *Valſalva*, he had introduced the Needle into the Eye in the ſame Place, and in the ſame Manner, as is commonly practiſed in the Operation of couching, and having afterwards diſſected the ſame Eyes, he had always found, that the Needle had paſs'd into the Eye on the backſide of the Cryſtalline Humour, ſo that it had been impoſſible to bring the Needle forward from thence into that Part of the aqueous Humour, which is ſeated between the *Uvea* and the Cryſtalline Humour, in order to depreſs a Pellicle ſeated there, according to the common Opinion, unleſs he would have paſs'd his Needle thro' the Body of the Cryſtalline.

This curious Author likewiſe obſerves, that the aforeſaid Space, between the *Uvea* and the Cryſtalline Humour, is ſo very narrow, that tho' he finds it not impoſſible to introduce a Needle into that Space, yet there is by no means room enough to turn the Needle up and down in all Directions, with that Freedom, that is uſed in couching Cataracts, without wounding either the *Uvea*, or the Cryſtalline.

Laſtly, Signor *Benevoli* obſerves, that in his Practice of couching Cataracts for many Years, having generally couched about twelve or fourteen in a Year, he had always found, that he work'd upon a hard and reſiſting Subſtance, which being tenderly touch'd by the Needle, would vibrate and fluctuate backwards and forwards, and would ſometimes return againſt the Needle with a ſenſible *Impetus*, which by no means agrees with the common Notion of the Cataract's conſiſting in a Pellicle or membranous Subſtance.

FINIS.

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Numb. 374.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *November* and *December*. 1722.

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ERRATUM.

Pag. 204. lin. *penult.* read, in icone 7.

I. *Obſervatio Eclipſis Solaris, ab* Edmundo Halleio, *LL.D. R.S.S. Aſtron. Reg. & Geom. Prof. Savil. Oxon.* Novem. 27° 1722. *p. m.* Grenovici.

h	′	″	
1	29	16	Eclipſin jam inceptam vidi.
1	31	6	Diſtantia Cuſpidum 7′ 4″, unde pars deficiens 0′ 47″ ac initium verum 1^h 28′ 58″
1	34	18	Diſtantia Cuſpidum 10′ 50″
1	42	28	Repet. 16′ 20″
1	43	26	Inclinatio Cuſpidum ad dextras 44° 30′
2	32	37	Partes Lucidæ reſiduæ 17′ 20″
2	40	16	Repet. in med. Eclipſis proxime. 17′ 9″
3	28	45	Inclinatio ad ſiniſtras 19° 0′
3	31	45	Diſtantia Cuſpidum 15′ 21″
3	37	35	Repet. 10′ 50″
3	43	25	Finis Eclipſeos dubius, ob limbum Solis aſperum & undulantem nec ſat bene definitum.
3	43	45	Certe deſierat Eclipſis.

Cœlum toto tempore ſerenum & tranquillum.

II. *The ſame Eclipſe obſerved in* Fleetſtreet, London. *By Mr.* George Graham, *F. R. S.*

	h	′	″	
P. M.	1	28	38	Beginning. Apparent Time.
	2	29	34	By Eſtimation the Cuſpes Parallel to the Horizon.
	3	43	22	The End.
	2	14	44	The Duration.

Quantity eclipſed 5 Dig. $\frac{716}{1000}$

I Had very correct Obſervations both of the Sun and Stars, the 26, 27, and 28th, for determining the exact Time by my Clock.

For ſome Minutes before the Eclipſe began, I obſerv'd the Sun with a Teleſcope of 12 Foot, furniſhed with a Micrometer; keeping that Part of the Limb in the middle of the Glaſs, where I expected the Moon firſt to touch, and in leſs than four Seconds of Time, from the Moment I judged the Eclipſe begun, it was ſo conſiderably advanc'd, that I cannot doubt of having the Beginning to leſs than three Seconds. I believe the exact Time of ending was within the ſame Limit, notwithſtanding that the Undulation of the Limb was then much greater than at the Beginning. The Parts eclipſed, meaſured with the Micrometer, at the Time of the greateſt Obſcuration, were 927 ſuch Parts as the Sun's Vertical Diameter contained 1946; which was taken a little before the Beginning of the Eclipſe.

The Sky was clear, and free from Clouds, till near the End, when a narrow one obſcured ſome part of the Sun's Disk, but that part of the Limb where the Eclipſe ended, continued clear till after it was over.

FIG. VIII.

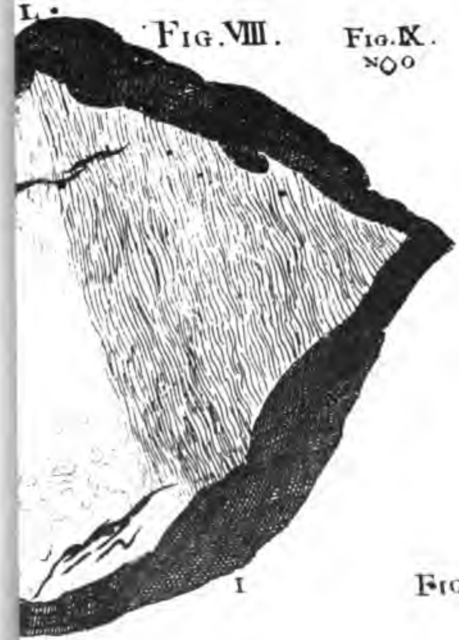


FIG. IX.

FIG. X.

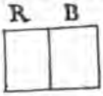


FIG. XI.

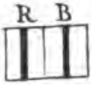


FIG. XII.

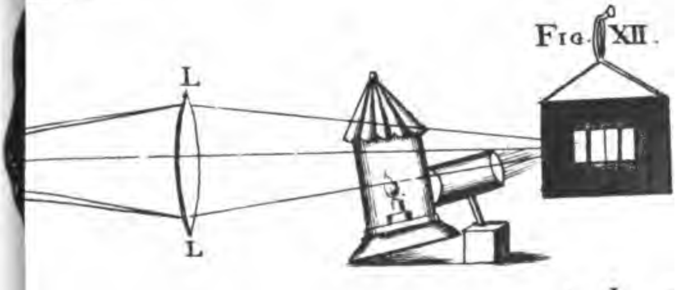
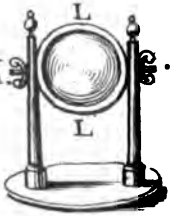


FIG. XIII.



J Sturt sculp.

By this Obſervation the Beginning differed not 2 $\frac{1}{12}$, and the End not $\frac{1}{2}$ a Minute from Dr. *Halley*'s Computation, which he ſent me the Day before. And if his Computation, which was made for *Greenwich*, had been reduced to the Meridian of *London*, the Difference would have been ſtill leſs.

The ſame Eclipſe was obſerv'd by Mr. Hawkins *at* Wakefield, *in* Yorkſhire, *to begin at* 1 h. 21 m. p. m. *and to end at* 3 h. 30 m. 3 ''. *The Sun's Diameter was obſcured ſomewhat more than* 5 *Digits.*

III. *De Particulis & Structurâ Adamantum. Epiſtola Domini* Antonii Leeuwenhoek, *R. S. Soc. ad* Jacobum Jurin, *M. D. R S. Secret.*

POſtquam inveſtigando compereram Metalla quædam, & ipſas etiam Arenas ex perexiguis ejuſdem materiæ particulis compoſitas eſſe, meditationem meam converti ad Adamantem; ſcilicet num ille etiam ex iſtiuſmodi conſtet particulis, quæ quidem ope microſcopii conſpici poſſint.

Igitur exiguum quendam Adamantem per microſcopium contemplatus, in eâ Adamantis parte quæ polita non erat, & ſplendore carebat, complures particulas oculis obſervavi; & Adamantem ex parvis particulis compoſitum eſſe comperi. Verum cum hæc nondum mihi ſatisfacerent; Adamantem in fruſta confringere decrevi, ut illum in fragmentis ſuis conſiderarem.

Ergo Adamantem, malleo impoſitum, alio malleo ſemel iterumque percuſſi, qui ſic percuſſus in quatuor aut quinque fruſta diſſiliebat. Quod cum nondum mihi ſatisfaceret, & Adamantem in exiguiſſimas miculas comminuere vellem; fruſtum, quod cæteris majus

erat, duplici chartâ circumvolvi, ne quæ Adamantis miculæ diſſiliendo perirent.

Hic ego duritiem Adamantis admiratus ſum: qui multâ vi aliquoties ictus, in quatuor tantum aut quinque fruſta ſine ullis miculis diſſiliebat—

Poſteriora iſta Adamantis fruſtula microſcopio etiam admovi: quæ cum perſpecularer, pene omnia ex minutiſſimis particulis compoſita eſſe comperi. Cum autem fruſtula illa radiis ſolaribus objicerem: quandam quaſi flammulam ex iis emicare videbam, & quidem majorem, quam unquam vidiſſem.

Fruſtulum unum oculis notabam, quod locum fracturæ ſuæ, planæ quidem ac quadratæ, ſoli directe oppoſitum habebat: qui locus, quantum viſu dijudicabam, tribus aut quatuor pilis menti virilis latitudine reſpondebat.

Ex hoc Adamantis fruſtulo tanta aſcendebat copia ardeſcentium flammularum, ut plures eſſe numero quam quadringentas judicarem. Flammularum iſtarum nonnullæ, ſed numero paucæ, ſibi erant vicinius junctæ, & reliquis majores: unde concludebam, ipſas Adamantis particulas illic etiam aliis particulis majores eſſe, vel ordinatius eſſe diſpoſitas.

Exinde oculos converti ad aliud Adamantis fruſtulum, paris circiter magnitudinis cum priori, quod radios ſolares itidem directe excipiebat; & haud paucioribus exiguiſſimæ molis particulis conſtare comperiebam. Ex unâ fruſtuli illius parte eâque circiter dimidiatâ, ſpecies illæ ardeſcentium flammularum etiam exoriebantur, ſed mole minores: in altera medietate flammula quædam cernebatur, aſſidue ſe mobilitans, cum continuâ quadam coruſcatione, quæ ſpeciem habebat debilioris fulgetri.

Cæterum, poſtquam iſta Adamantis fruſtula radiis ſolaribus ſubduxeram, adhuc multiformes rerum ſpecies oculis meis objiciebantur. Inter alia, ex ſingulis Adamantis particulis flammula quædam in altum emicare videbatur.

Porro, novem præterea Adamantis fruſtula microſcopio applicata habebam: & in eorum ſeptem particulas illas agnovi, quas ſpeciem flammularum ejaculari dixi. In duobus aliis etiam particulas illas agnoſcere poteram, ex quibus Adamantem compoſitum eſſe ſtatui: ſed illæ planitiem ſuam ita ſoli obverſam habebant, ut plures eodem tempore particulas dignoſcerem.

Mihi autem peramænum erat ſpectaculum, tot intueri imagines flammularum, quæ omnes colorem præferebant coruſcum, & pleræque viridantem. Iſtud autem inſolens mihi viſum eſt, quod ad flammularum quarundam extremitatem talis perciperetur in aere motus ac vibratio; tamquam ſi flammula adeo illic debilitata foret, ut conſpicua eſſe deſineret. Præ cæteris autem admirabar, quod ex tali Adamantis particulâ circumquaque ignis exiret, debiliter rutilans, ſicuti cum fulmen e longinquo coruſcare videmus. Quod quidem tam crebro intuente me accidebat, ut oculos avertere non poſſem, niſi jam fatigatos.

Verum tam jucundo ſpectaculo ſæpius frui decrevi, & fruſtulum iſtud Adamantis, donec ſponte a vitro decidat, microſcopio applicatum relinquere. Neque enim ope glutinoſæ alicujus materiæ affixum eſt vitro: niſi quod vitrum, antequam illi fruſtum Adamantis affigerem, humore anhelitus mei irroraſſem.

Cum tam grato ſpectaculo ſolus frui non vellem; quæ modo relata ſunt, legenda tradidi cuidam N. cui & microſcopium in manus tradidi. Cumque poſt aliquantum moræ requirerem; nonne omnia deſcriptioni meæ convenire comperiret; ille prorſus convenire reſpondit, & admirationem ſuum ſuper ſpectaculi inſolentiâ confeſſus eſt.

Porro, cum fruſtulum quoddam Adamantis adhuc microſcopio applicatum eſſet, cujus particulas, lamellarum modo ſibi incumbentes, viſu dignoſcere poteram; haud abs re me facturum putavi, ſi lamellas iſtas delineari curarem, quæ hic in icone 1 denotantur per A B C.

Deinde & aliud Adamantis fragmentum microſcopio applicaveram, cujus lamellæ, invicem ſuperſtratæ, ſeſe oculis meis diſtinctiſſime offerebant; quæ hic in icone 2 deſignantur per D E F G H I K L M. In hac autem figurâ particulæ illæ ſive lamellæ, quæ per F G, F H, F I, F K, & L denotatæ ſunt, reliquis aliquanto craſſiores eſſe videntur: verum iſtæ particulæ ex pluribus lamellis, invicem ſuperſtratis, ſunt compoſitæ. In iſtà vero fragmenti parte, quam inter D E F M exprimendam curavi, diſtinctiſſime apparent tenuiſſimæ lamellæ, ex quarum congerie totum Adamantem concretum eſſe pro certo habendum eſt.

Priuſquam ſecundam iconem in chartâ exprimendam curaveram; forte celebris Gemmarius, *N. Verbrugge*, ædes meas præteribat, quem ego ad conviſendum Adamantis fragmen, ſicut microſcopio applicatum erat, invitavi. Ille fragmen iſtud non ſine admiratione contemplatus, quaſdam ſe Adamantum glebulas, quæ ſibi inutiles eſſent, mihi miſſurum recepit.

Nec multo poſt bina mihi mittit Adamantum fragmenta: item exiguum Adamantem, ex arte quidem politum, ſed ſordidum, uti vocant, ſeu vitioſum.

Cum ſingula ſingulis microſcopiis applicaſſem, primo fragmentum iſtud, quod in *Fig.* 3. per N O P Q R denotatum eſt, delineandum curavi. Lineolæ, quæ per totum hoc fragmentum excurrunt, revera non ſunt niſi lamellæ, ex quibus Adamantes conſtare modo dixi: & apertius conſpiciendas ſe præbent ad P Q.

Cæterum ut eorum, quæ de hâc Adamantis glebulâ, ſive fruſtulo, jam dixi, clarior atque diſtinctior ſit perceptio; fruſtulum iſtud eâdem prorſus magnitudine exprimi juſſi, quam nudo delineatoris oculo, ſine microſcopii ope, offerebat: quæ vera fruſtuli magnitudo, in icone 4, exhibetur inter S T. Iſtud autem tantillum fruſtulum ex tam multis tamque exiguis particulis compoſitum eſt; ut qui non viderit, capere haud poſſit.

In altero Adamantis fragmento, quod ejuſdem propemodum magnitudinis erat, lamellæ dignoſci poterant: & pars illius circiter quinta conſtabat quinquangulo tam polito, tamquam ſi ex arte lævigatum fuiſſet, niſi quod illi affixus eſſet perexiguus Adamas, qui circiter quartam quinquanguli partem obtegebat; & uti clare viſu agnoſcebam, etiam ex lamellis, ſive particulis lamellarum formam habentibus, concretus erat.

Quantum ad perexiguum iſtum Adamantem; illum quidem ex arte politum, ſed ſordidum ſeu vitioſum eſſe dixi: ſordidos enim vocamus, dum vel ſubflavi ſunt, vel rimis aut lineolis deformes: quamvis nonnullæ hujus Adamantis rimæ atque lineolæ nudis oculis conſpici non poſſent; imo, adhibito etiam microſcopio, viſum pene effugerent.

Exiguus hic Adamas erat quadrangulus: prope unum iſtorum angulorum, intra ipſum Adamantem, varias vidi particulas ab invicem ſejunctas; niſi quod aliquantulum ſeſe attingere viderentur, quod initio inſolens mihi viſum eſt. Particularum iſtarum maxima coloris erat ſubflavi, & altero circiter latior quam craſſior: nec ſplendore cedebat vitro. Reliquis particulis variæ erant figuræ; nonnullis etiam par ſplendor ac primæ: nec pauciores eſſe judicabam quam viginti; licet delineator tantum numerum non expreſſerit.

Iſte conſpectus hanc mihi perſuaſionem induxit; quo tempore materia, Adamantum productrix, vagabatur in aëre; exiguas illas particulas, quæ itidem Adamantes erant, priori quem dixi Adamanti adjacuiſſe: materiam autem adamantinam, quamdiu talis materia aëri inſedit, iſtam Adamantum congeriem paulatim circumveſtiiſſe, & minores Adamantes incluſiſſe majori.

Qua occaſione recordor complures me habuiſſe Cryſtallos montanas figuræ ſexangulæ: in quarum nonnullis quaſi incluſæ jacebant figuræ quædam perexiguæ & oblongæ, coloris ſubcœrulei; ſed tam exiles ut, adhibito etiam optimæ notæ microſcopio, vix agnoſci poſſent.

Porro iſtius Adamantis, quem vitioſum appellavi, veram magnitudinem delineari curavi: quam in icone 5 expreſſam habes inter V & W.

Deinde & perexiguos iſtos Adamantes, qui in majorem Adamantem incluſi exhibentur in icone 5, ſeorſum delineandos curavi; quos in icone 6 deſignatos vides per XYZABC. Ubi per XYZ denotatur exterius Adamantis latus, quod licet ex arte politum, ope tamen microſcopii conſpectum, colorem tam fuſcum præferebat.

Per ZABCDE iſti denotantur exiliſſimi Adamantes; quos in majuſculo Adamante velut incluſos latuiſſe præmonui.

Cum poſtea latus Adamantis in icone 5 expreſſi, cui longe minores Adamantes incluſos fuiſſe mox dicebam, ad microſcopium admoviſſem, Adamantem iſtum variis foraminibus pertuſum eſſe comperi; quæ ego foramina tunc facta eſſe cenſui, quando latus illud poliebatur. Ita nimirum ut exiguiſſimi, quos dixi, Adamantes loca illa prius inſederint, ſed poliendo delapſi foramina illa ſive puteolos produxerint, quæ foramina in icone 5 conſpicua ſunt inter FGH.

Porro

Porrro exiguum illum Adamantem, inter V W in icone 5 expreſſum, in latus ſuum verteram; & ubi craſſiuſculus erat, novaculum illi aptaveram, ut Adamantem ipſum ictu mallei diffinderem: quod tamen, licet iterato tentanti, non ſucceſſit.

Quare Adamantem chartâ mundâ circumvolutum, imponebam malleo; & alio malleo, crebrius tamen feriendo, diffregi. Poſtquam omnes diffracti Adamantis glebulas diverſis microſcopiis applicaveram; unam, quæ plures qam reliquæ lamellas oculis exhibere videbatur, delineatori effingendam tradidi, quæ in icone 8 denotatur per IKLM. Haud tamen poſſibile delineatori fuit eâdem perfectione glebulam illam exprimere, quâ ſeſe oculis conſpiciendam offerebat.

Cum autem microſcopium, cui fruſtulum illud iconis 8 applicatum erat, diverſum eſſet ab iis microſcopiis, quorum ope alia Adamantis fruſta delineari curaveram; Delineator poſtremum hoc fruſtulum eâ magnitudine expreſſit, quàm ſine microſcopio conſpectum oculis offerebat: quæ vero fruſtuli illius magnitudo in icone 9 exhibetur inter NO.

Quædam ex his Adamentum fragmentis, ope microſcopii conſiderata, jucundos præbebant conſpectus: quos etiam nonnullis, talium rerum ſtudioſis, exhibui. Illis autem jucundiſſimum erat, in uno Adamantis fruſtulo tam multiplices agnoſcere partes: imprimis etiam quod lamellæ, ex quibus Adamantes concreti ſunt, in duobus Adamantum fruſtulis valde diſtincte poſſent internoſci; nempe dum lamellæ iſtæ juxta ductum longitudinis oculis objiciebantur.

Exinde ſtudium meum converti ad examen Cryſtalli cujuſdam montanæ, ſex lateribus præditæ, cujus longitudo circiter reſpondebat latitudini duorum digitorum, craſſitudo vero minori digito.

Cryſtallum iſtam in complura fruſta confregi, & fruſta microſcopiis applicavi; diſquirere volens num & illa ex ſuperſtratis ſibi lamellis compoſita eſſent: quâ ratione Adamantes magnitudinem ſuam adeptos eſſe dixi. Sed, tametſi perquiſitionem iſtam ſæpius iteraverim, ne tantillam quidem lamellam in iis deprehendi. Iſtud autem in Cryſtallis, quas quidem præ manibus habebam, plerumque animadverti, in omnibus earum lateribus, quæ numero ſena erant, tranſverſas protendi lineolas, alias aliis ſitu aliquantulum ſuperiores; tamquam ſi illic, increſcentibus Cryſtallis, ortæ productæque fuiſſent: quâ ſuper re, quantumcumque Cryſtallorum numerum ante conſideraverim, atque confregerim, numquam ipſe mihi ſatisfacere potui.

Delphis in Hollandiâ,
Nov. 20. 1722.

IV. *An Account of an Optical Experiment made before the* Royal Society, *on* Thurſday, Dec. 6th, *and repeated on the* 13th, 1722. *By the Reverend* J. T. Deſaguliers, *L L. D. F. R. S.*

SIR *Iſaac Newton*, in his Optics, (*B.* 1. *Prop.* 1. *Exp.* 2.) relates an Experiment made with a Card, or Paper, painted red on one half and blue on the other, which being enlighten'd by a Candle, the Image, by the Interpoſition of a *Lens*, is ſo projected on a white Paper, held on the other ſide of the *Lens*, that the Place where the blue half appears diſtinct, (or as the Opticians term it, the diſtinct Baſe of the Image of the blue half) is much nearer to the *Lens* than the Place of the Image of the red half. And this is made

apparent, by ſeeing on one of theſe Images the Repreſentation of the black Threads, that are wrapp'd round the Card, whilſt they are not viſible on the other. This is fully deſcribed in the Place abovemention'd; but yet a * Gentleman abroad has call'd the Experiment in queſtion, and denied the matter of Fact, ſaying, that he could not make it ſucceed,. but propoſes an Experiment of his own to diſprove the different Refrangibility of the Rays.

Upon this I was deſir'd to make the Experiment over again, before the Royal Society, which ſucceeded well. But becauſe there muſt be care taken in the making it, I ſhall mention all the Particulars obſerved in the Performance; which, if duly put in practice, will make the Experiment always ſucceed.

I painted one half of the Card R B, (*Fig.* X.) as B, with *Ultramarine*, made deeper with a ſmall Mixture of *Indigo*, and the other half R, I painted over with *Cinnabar* heighten'd with a little *Carmine*, ſo that the Line, that ſeparated the red from the blue, was perpendicular to the long ſides of the Cord.

Then I wrapp'd a black Silk four times together, over the middle of each painted part of the Card, as in *Fig.* XI.

Upon a ſquare Trencher, *Fig.* XII. painted black, and ſuſpended vertically againſt a Wall, I fix'd my colour'd Card with a Pin, and the Room being made very dark, I enlighten'd the Card with a ſtrong Light thrown upon it from a dark Lanthorn, that had two Convex Glaſſes in it; then ſetting up the *Lens* L L, (repreſented by *Fig.* XIII.) in ſuch manner, that its Axis paſs'd perpendicularly thro' the Image of the Card, at

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* Act. Erudit. Lipſ. Supplem. tom. 8. §. 3. p. 130. 131.

the

the diſtance of nine Feet from the Card, the Image of the Card being receiv'd upon a white Paper, at the diſtance of nine Feet on the other ſide of the *Lens*, at B, the blue half appear'd diſtinct, with the Image of the black Silk going vertically along its Plain, whilſt no Appearance of the black Silk was perceivable on the red half. Then removing the Paper about two Inches, to R, the red half of the Image had a black Line very plain upon it, whilſt it was inviſible on the blue half. This was more evident, when a ſtrong Image of the Candle was ſucceſſively thrown on that half of the Card, whoſe Image was under Examination. When the Paper was held in the middle between R and B, the black Line upon each Colour was viſible, but indiſtinct.

N. B. Care muſt be taken that the Colours be deep, becauſe having accidentally rubb'd off ſome of the blue, the Whiteneſs of the Card under it, made its Image fly out farther, almoſt as far as that of the red.

V. *Part of a Letter from Dr.* Nettleton, *Phyſician at* Halifax, *to Dr.* Jurin, *R. S. Secr concerning the* Inoculation of the Small Pox, *and the Mortality of that Diſtemper in the natural Way.*

THERE are two Propoſitions advanced by the Favourers of the Practice of Inoculation, concerning which the Publick ſeems to require more full Satiſfaction. *That the Diſtemper rais'd by Inoculation is really the Small Pox*; and, *That it is much more mild and favourable, and far leſs mortal, than the natural ſort.*

The former of theſe is not ſo much diſputed now, as it was at firſt, when this Method was introduced, nor can it be made a doubt of by any one, who has ſeen thoſe that have been inoculated, and has alſo been much converſant in the natural Small Pox. There is uſually no manner of difference to be obſerved betwixt the one ſort and the other, when the Number of Puſtules is nearly the ſame; but in both there are almoſt infinite Degrees of the Diſtemper, according to the difference of that Number. All the Variation that can be perceived of the Ingrafted Small Pox from the Natural, is, that in the former the Puſtules are commonly fewer in Number, and all the reſt of the Symptoms are in the ſame Proportion more favourable. They exactly reſemble what we call the Diſtinct Sort: the Symptoms before the Eruption are the very ſame, and when the Puſtules begin to riſe, their Appearance is the ſame, as well as their Periods of Maturation and Declenſion; they are at firſt of the ſame florid, roſy Colour, and when fully ripe, of as fair a yellow. They commonly riſe as round and as large as the other, and when they are very numerous, the Inflammation and ſwelling of the Face comes on at the uſual Time, and is followed by the ſwelling of the Hands and Feet, and only once I obſerved a Salivation, tho' the Puſtules were diſtinct. In the Natural Small Pox, when the Puſtules are very few, we ſometimes obſerve, they do not riſe to ſo great a Bulk, neither do they ripen ſo fully, nor continue ſo long as uſual; and it is the ſame in the way of Inoculation. In ſhort, as this Diſtemper is raiſed by an Ingraftment from the Small Pox, as it has the very ſame Appearance, and as it is capable of producing the ſame by Infection, there ſeems to be no room to doubt of its being the true and genuine Small Pox. And if that be allowed, it will follow from thence, as a Corollary, that *Thoſe, who have been inoculated, are in no more Danger of receiving the Diſtemper again, than Thoſe who have had it in the ordinary Way.* And this is alſo thus far confirmed by Experience. We are very ready to own, that the Operation may ſometimes fail: thoſe Gentlemen, who firſt communicated to the Royal Society ſome Account of this Practice from *Turkey*, did both of them intimate ſo much; tho' I believe that will but rarely happen. In one * Inſtance here, I obſerved no Eruption at all, neither did the Wounds inflame and ſwell any more than would have follow'd from a common Inciſion, which made me conclude, that what was apply'd had not taken Effect, and indeed the Reaſon of it was very well known to me. In three others, tho' the Wounds did inflame, and ſwell, and diſcharge conſiderably, yet the Eruptions were ſo imperfect, as to leave me a little in doubt: but two of theſe have ſince been ſufficiently try'd, by being conſtantly with thoſe who had the Small Pox, without receiving any Infection; which makes me inclined to believe they will always be ſecure from any danger of it. As to all the reſt, neither I nor any body elſe, who ſaw them, did in the leaſt queſtion, but that they had the true Small Pox.

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* *William Clark*'s Son. See Philoſoph Tranſact. No. 370. p. 45.

As to the latter Propoſition, *That the Ingrafted Small Pox is far leſs dangerous than the Natural:* the Truth of this, I ſuppoſe, can only be found by making a Compariſon, ſo far as our Experience will extend. In order to this, I have taken an Account in this Town, and ſome Part of the Country, and have procured the ſame from ſeveral other Towns hereabouts, where the Small Pox has been Epidemical this laſt Year, with as much Exactneſs as was poſſible, how many have had the Small Pox, and how many out of that Number have died. Some of theſe I did ſend you the laſt Summer, but I will beg leave to repeat them amongſt the reſt, that they may be all under one view.

	Have had the Small Pox.	Died.
In *Halifax*	276	43
In a Part of the Pariſh of *Halifax*, ſtretching towards *Bradford*	297	59
In another Part of the ſame Pariſh	268	28
In *Bradford*	129	36
In *Leeds*	792	189
In *Wakefield*	418	57
In *Rochdale*	177	38
In *Aſhton under Line*, a ſmall Market-Town in *Lancaſhire*, including two neighbouring Villages	279	56
In *Maccleſfield*	302	37
In *Stockport*	287	73
In *Hatherfield*	180	20
Total	3405	636

I am very ſenſible you will require a great number of Obſervations, before you can draw any certain Concluſions. I would only crave leave to remark, that it appears from theſe Accounts, that this laſt Year, in this Part of the Kingdom, almoſt nineteen out of every hundred, or *near one fith of thoſe, who have had the natural Small Pox, have died*; whereas *out of ſixty one which have been inoculated hereabouts, not one has died*; for as to the Caſe of Mr. *John Symſon*'s Daughter, which would have made the ſixty ſecond, I leave it out of my Account, and I will refer it to any impartial Judgment, whether I may not juſtly do ſo *. The Facts are open to every one's Enquiry, and whoever will give himſelf the Trouble, may be ſatisfied as to the Truth of them.

Halifax, *Dec*. 16.
1722.

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* See Philoſoph. Tranſact. *No.* 370. p. 41, 42.

VI. *A Letter to the Learned Dr.* Caleb Coteſworth, *F. R. S. of the College of Phyſicians,* London, *and Phyſician to St.* Thomas's *Hoſpital; containing a Compariſon between the Danger of the Natural* Small Pox, *and of that given by Inoculation. By* James Jurin, *M. D. R. S. Secret.*

S I R,

THE ſincere and diſintereſted Regard for the Good of Mankind, which you have always manifeſted, as well in your extenſive private Practice, as in that publick Poſt, which you have ſo long and ſo uſefully fill'd, muſt affect you, I am ſure, with a great and ſenſible Concern for the Deſtruction made among us by that terrible Calamity the Small Pox. We have ſeen, for ſome conſiderable Time paſt, above 100 Perſons *per* Week in this City and Suburbs, taking one Week with another, to be carry'd off by this Diſeaſe; a Conſideration certainly that ought to diſpoſe us to enter into any Meaſures, by which we may reaſonably hope to put ſome Stop to the Progreſs of ſo cruel a Diſtemper.

To this Purpoſe, Sir, the Method of Inoculation, which has lately been introduced among us, is ſtrongly recommended on the one hand, and has been oppoſed with a great deal of Warmth and Zeal on the other.

I have no Inclination to enter into this Controverſy; it is in better and abler Hands: but, as the Point in Diſpute is of the utmoſt Importance to Mankind, I heartily wiſh, that, without Paſſion, Prejudice, or private Views, it may be fairly and maturely examin'd. In order to which, if the following Extracts and Computations, concerning the comparative Danger of the Inoculated and Natural Small Pox, may be of any Uſe to your ſelf, or to other impartial and diſintereſted Judges, I ſhall think my Labour well beſtowed.

The Number of Perſons, who have had the Small Pox by Inoculation here in *England*, is, by the beſt Information I have been able to collect, as follows.

Inoculated by

Dr. *Nettleton* - - - - - - - - - -	61
Claudius Amyand, Eſq; Serjeant Surgeon -	17
Mr. *Maitland*, Surgeon - - - - - -	57
Dr. *Dover* - - - - - - - - - - -	4
Mr. *Weymiſh*, Surgeon - - - - - -	3
The Reverend Mr. *Johnſon* - - - - -	3
Dr. *Brady*, at *Portſmouth* - - - - -	4
Mr. *Smith*, Surgeon, and Mr. *Dymer*, Apothecary, at *Chicheſter* - - -	13
Mr. *Waller*, Apothecary at *Goſport* - -	3
A Woman at *Leiceſter* - - - - - -	8
Dr. *Williams* at *Haverford Weſt* - - -	6
Two other Perſons near the ſame Place - -	2
Dr. *French*, at *Briſtol* - - - - - - -	1
In all —	182

Out of this Number the Oppoſers of Inoculation affirm, that two Perſons died of the Inoculated Small Pox; the Favourers of this Practice maintain, that their Death was occaſioned by other Cauſes. If, to avoid diſpute, theſe two be allow'd to have died of Inoculation, we muſt eſtimate the hazard of dying of the Inoculated Small Pox, as far as can be collected from our own Experience, to be that of 2 out of 182, or one out of 91.

The Reverend Mr. *Mather*, in a Letter dated *March* 10. 1721. from *Boſton* in *New England*, gives an Account, That *of near* 300 *inoculated there*, 5 *or* 6 *died upon it or after it, but from other Diſeaſes and Accidents, chiefly from having taken the Infection in the common way by Inſpiration, before it could be given them in this way of Tranſplantation.*

If, as we have done before, to avoid all occaſion of diſpute, we allow 5 out of theſe 300 to have died of the Small Pox by Inoculation, notwithſtanding what Mr. *Mather* has ſaid of their dying by other Accidents or Diſeaſes; the hazard of Inoculation will thence be determin'd to be that of 1 in about 60. But here it muſt be obſerv'd, that by all the Accounts from *New England*, the Operators there appear not to have been ſo cautious in the choice of their Subjects, as here in *England*. For Mr. *Mather* tells us, that the Perſons inoculated were *young and old, from* 1 *Year to* 70, *weak and ſtrong*; and by other relations we are inform'd, that *Women with Child, and others even in Childbed*, underwent the Operation. Apparently the Greatneſs of the Danger they were in, from the Infection in the natural Way, which then raged among them with the utmoſt Fury, made them the more adventurous.

We come now, Sir, to the ſecond Part of our Deſign, which is to form an Eſtimate of the Hazard, which all Mankind, one with another, are under of dying of the natural Small Pox, that, by comparing this with the Hazard of Inoculation, the Publick may be enabled to form a Judgment, whether or no the Practice of Inoculation tends to the Preſervation of Mankind, by leſſening the Danger to which they are otherwiſe liable.

With this View I have conſulted the Yearly Bills of Mortality, as far back as the Year 1667, being the Year after the Plague and the Fire of *London*, comprehending to the preſent Time the Space of 56 Years, from 42 of which I have given Extracts in the two following Tables.

The firſt of theſe takes in the firſt 20 Years, diſtinguiſhing for every Year the total Number of Burials, and likewiſe the Number that died of the Small Pox, in two ſeparate Columns. The third Column ſhews, how many died of the Small Pox out of every Thouſand that were buried; and the fourth Column repreſents the Proportion between thoſe that died of the Small Pox, and the whole Number of Burials, by the neareſt Vulgar Fraction, having always 1 for the Numerator.

The ſecond Table gives the laſt 22 Years, after the ſame manner, and at the bottom of each Table is given the Total Number for each Series of Years, and likewiſe the Number that died each Year, taken at a Medium, one Year with another: by which it appears, that the Proportion between the Number of thoſe that die of the Small Pox, and the whole Number of Burials, is very nearly the ſame, upon an Average for each Series of Years.

The 14 intermediate Years between 1686 and 1701 are left out, becauſe in the Bills for thoſe Years, the Accounts of the Small Pox and Meaſles are not diſtinguiſhed, as in the preceding and following Years, but are join'd together in one Article, ſo that from them no certain Account can be drawn of the Number of Perſons, that died of the Small Pox.

TABLE I.

Years	Total N° of Burials	Died of the Small Pox. In all.	In 1000	In Proportion.
1667	15842	1196	75	$\frac{1}{13}$
1668	17278	1987	115	$\frac{1}{9}$
1669	19432	951	49	$\frac{1}{20}$
1670	20198	1465	73	$\frac{1}{14}$
1671	15729	696	44	$\frac{1}{23}$
1672	18230	1116	61	$\frac{1}{16}$
1673	17504	853	49	$\frac{1}{21}$
1674	21201	2507	118	$\frac{1}{8}$
1675	17244	997	58	$\frac{1}{17}$
1676	18732	359	19	$\frac{1}{52}$
1677	19067	1678	88	$\frac{1}{11}$
1678	20678	1798	87	$\frac{1}{12}$
1679	21730	1967	91	$\frac{1}{11}$
1680	21053	689	33	$\frac{1}{31}$
1681	23971	2982	125	$\frac{1}{8}$
1682	20691	1408	68	$\frac{1}{15}$
1683	20587	2096	102	$\frac{1}{10}$
1684	23202	156	7	$\frac{1}{149}$
1685	23222	2496	107	$\frac{1}{9}$
1686	22609	1062	47	$\frac{1}{21}$
20 Years	398200	28459	$71\frac{1}{2}$	$\frac{1}{14}$
Each Year at a *Medium*	19910	1423	$71\frac{1}{2}$	$\frac{1}{14}$

TABLE II.

Years	Total N° of Burials	Died of the Small Pox.		
		In all.	In 1000	In Proportion.
1701	20471	1095	53	$\frac{1}{19}$
1702	19481	311	16	$\frac{1}{62}$
1703	20720	898	43	$\frac{1}{23}$
1704	22684	1501	66	$\frac{1}{15}$
1705	22097	1095	50	$\frac{1}{20}$
1706	19847	721	36	$\frac{1}{28}$
1707	21600	1078	50	$\frac{1}{20}$
1708	21291	1687	79	$\frac{1}{13}$
1709	21800	1024	47	$\frac{1}{21}$
1710	24620	3138	127	$\frac{1}{8}$
1711	19833	915	46	$\frac{1}{22}$
1712	21198	1943	92	$\frac{1}{11}$
1713	21057	1614	77	$\frac{1}{13}$
1714	26569	2810	106	$\frac{1}{9}$
1715	22232	1057	48	$\frac{1}{21}$
1716	24436	2427	99	$\frac{1}{10}$
1717	23446	2211	94	$\frac{1}{11}$
1718	26523	1884	71	$\frac{1}{14}$
1719	28347	3229	114	$\frac{1}{9}$
1720	25454	1440	57	$\frac{1}{18}$
1721	26142	2375	91	$\frac{1}{11}$
1722	25750	2167	84	$\frac{1}{12}$
22 Years	505598	36620	72	$\frac{1}{14}$
Each Year at a *Medium.*	22982	1665	72	$\frac{1}{14}$
42 Years	903798	65079	72	$\frac{1}{14}$
Each Year in 42 at a *Medium.*	21519	1550	72	$\frac{1}{14}$

By theſe Tables it appears, that *upwards of ſeven* per Cent. *or ſomewhat more than a fourteenth part of Mankind, die of the Small Pox*; and conſequently the hazard of dying of that Diſtemper, to every individual born into the World, is at leaſt that of one in fourteen. And that this Hazard encreaſes after the Birth, as the Child advances in Age, will appear from what follows.

From this Eſtimate it is demonſtrable, that, in the Caſe of Perſons actually having the Small Pox, the Hazard that they run, one with another, of dying of that Diſtemper, is greater than that of one in fourteen; or, which is the ſame thing, *there muſt be fewer than thirteen, that recover, for one that dies of the Small Pox*. For ſince one fourteenth part of Mankind die of the Small Pox, and the other thirteen parts die of other Diſeaſes; if theſe thirteen have all had the Small Pox, and recover'd from it, before they fell ill of thoſe other Diſeaſes of which they died, then juſt thirteen will have recover'd from the Small Pox, for one that dies of that Diſtemper: but, as it is notorious, that great Numbers, eſpecially of young Children, die of other Diſeaſes, without ever having the Small Pox, it is plain, that fewer than thirteen muſt recover from this Diſtemper, for one that dies of it.

To determine exactly how many of theſe thirteen Parts of Mankind, die without having the Small Pox, is a very difficult Task: but it is eaſy to ſee, that a conſiderable Deduction is to be made from them.

In the firſt place, the two Articles of *Stilborn* and *Abortive Children*, which are put into the yearly Bills, as part of the Number of Burials, are unqueſtionably to be deducted.

With theſe two, Sir, you will not I believe, think it unreaſonable to join the following Heads, which, by the beſt Information I can procure, comprehend only very young Children, or at moſt not above one or two Years of Age: *Overlaid, Chryſoms and Infants, Convulſions, Horſeſhoehead, Headmoldſhot, Teeth, Water in the Head, Worms, Rickets, Livergrown, Chin-Cough, and Hooping-Cough,* which Articles in the Yearly Bills for 22 Years laſt paſt, amount at a Medium to 386 in each 1000, of the whole Number of Burials.

It is true indeed, that, in all Probability, ſome ſmall part of theſe muſt have gone thro' the Small Pox, and therefore ought not to be deducted out of the Account: but then, on the other hand, as it is certain, that of the remaining $\frac{614}{1000}$ of Mankind, that are above one or two Years of Age, there are great Numbers, that never have the Small Pox, it will I preſume be judged to be no unequal Suppoſition, if I ſuppoſe all that are contain'd under the Heads abovemention'd, to have miſs'd that Diſtemper, when by way of Compenſation, I allow all the remainder of Mankind to undergo it; which Conceſſion is ſo large, that it will abundantly make up for what I aſſume too much in the former Suppoſition.

Allowing therefore, that out of every 1000 Children that are born, 386 die under one or two Years of Age, without having the Small Pox, and 72 do ſome time or other die of that Diſtemper; it follows, that the Hazard of dying of it, to the remainder of Mankind, above one or two Years of Age, who are ll ſuppoſed to undergo that Diſeaſe ſooner or later, is at of 72 out of 614, or nearly 2 out of 17: ſo that *no more, than between* 7 *and* 8, *can recover from that Diſtemper, for one that dies of it.* And if any conſiderable part of the aforeſaid remainder of Mankind, more than is allow'd for above, do eſcape having the Small Pox, then the Proportion of thoſe that recover from it, will be ſtill ſmaller.

This Conſideration ſhews the Fallacy of one plauſible Argument, that has been often made uſe of on occaſion of the preſent Diſputes about Inoculation: Which is, that whatever be the Danger of dying of the Small Pox, to thoſe that actually have that Diſeaſe, yet, as great Numbers of Perſons never have the Small Pox at all, this Danger is what any particular Perſon may never be in; and therefore it will be Madneſs to undergo the hazard of Inoculation, be it great or ſmall, in order to prevent a Diſeaſe, which poſſibly may never befal one.

For if two parts in ſeventeen of all Mankind, that are above one or two Years of Age, muſt ſooner or later die of the Small Pox, it is plain, that how many parts ſoever of theſe ſeventeen are ſuppoſed to eſcape that Diſtemper, the Mortality among the remainder, who undergo it, muſt in proportion be ſo much the greater. As for inſtance, if 7 parts eſcape having the Small Pox, and 10 undergo it, then 2 out of 10, or 1 out of 5, that have the Small Pox, muſt die of that Diſeaſe.

And as it can never be known, whether or no any particular Perſon be one of thoſe, that are to have the Small Pox; his Hazard of dying of that Diſtemper, being made up of the Hazard of having it, and the Hazard of dying of it, if he has it, will be exactly the ſame, namely, that of 2 in 17, or 1 in 8 or 9, whether the proportion of Mankind, that eſcape having the Small Pox, be great or ſmall.

But as what has been ſaid concerning the Hazard of the natural Small Pox, is taken from an Account of 42 Years; whereas the Hazard of Inoculation is eſtimated only from what has happen'd in the ſpace of about 18 Months, ſince which time it had its firſt Riſe among us; it will perhaps be asked by ſome Perſons, why we do not likewiſe make the Eſtimate of the hazard of the natural Small Pox, from the laſt two Years alone, without running back into ſo great a number of Years, before Inoculation was begun?

To which we anſwer, that the Proportion of thoſe that die of the Small Pox, varies ſo much in different Years, as appears from the Tables above, that it was impoſſible to come at any Certainty in this point, from the conſideration of the two laſt Years alone: and if any one ſuſpects us of Partiality in proceeding after the manner we have done, he need only caſt his Eyes upon the ſecond Table, where he will find, that the Mortality of the natural Small Pox, for the two laſt Years, has conſiderably exceeded the Medium we have determin'd, from taking in two and fourty Years.

There is another Method, which, if it were put in practice in ſeveral large Towns, or Pariſhes, and for a ſufficient Number of Years, would enable us to come at a nearer and ſtill more certain Computation of the Proportion between thoſe that recover, and thoſe that die of the Small Pox: which is, to ſend a careful Perſon once a Year, from houſe to houſe, to enquire what Perſons have had the Small Pox, and how many have died of it, in the preceding Year. This has been done by Dr. *Nettleton* the laſt Year, at ſeveral Towns in *Yorkſhire*, &c. and the ſame was done at *Chicheſter* for the ſame Year, to the 15th of *October* laſt, by a Perſon of Credit, whoſe Account was communicated to me by my learned and ingenious Friend, Dr. *Whitaker*.

Snch another Account has been tranſmitted to me from *Haverford Weſt*, in *South Wales*, by the Learned Dr. *Perrot Williams*, Phyſician in that Place. The Sum of theſe Accounts is as follows.

	Sick of the Small Pox.		Died.
Several Towns in *Yorkſhire* — —	3405	—	636
Chicheſter — — — — —	994	—	168
Haverford Weſt — — — —	227	—	52
Total	4626	—	856

From which it appears, that, upon a *Medium* between theſe Accounts, there *died of the Small Pox almoſt* 19 *per Cent.* or *nearly one in five*, of Perſons of all Ages, that underwent that Diſtemper. Which is the more to be remarked, for that *out of* 82 *Perſons*, that had the Small Pox by Inoculation, the ſame Year, and in the Neighbourhood of the ſame Places, *not one miſcarry'd.*

Mr. *Mather* obſerves, in his Letter mention'd above, that *out of more than* 5000 *Perſons that had the Small Pox* at *Boſton* in *New England*, *within little more than half a Year*, *near* 900 *died*, which is more than one in ſix ; and this Account added to thoſe from *Yorkſhire*, *Chicheſter*, and *Wales*, reduces the Proportion of thoſe that die of the Small Pox to ſomewhat more than 18 *per Cent.* ſo that the Hazard of dying of that Diſtemper, to thoſe who are taken ill of it, is that of *one in between five and ſix*, or ſomething above *two in eleven.*

The Reſult therefore, Sir, of theſe Computations is, that, if the ſame Proportions ſhould ſtill continue, as have hitherto been determin'd by Obſervation, we muſt expect, That of all the Children that are born, there will, ſome time or other, die of the Small Pox, *one in fourteen.*

That of Perſons of all Ages taken ill of the natural Small Pox, there will die of that Diſtemper, *one in five or ſix, or two in eleven.*

That of Perſons of all Ages inoculated, without regard to the Healthineſs or Unhealthineſs of the Subject, as was practiſed in *New England*, there will die *one in ſixty.*

That of Perſons inoculated with the ſame Caution in the choice of the Subjects, as has been uſed by the ſeveral Operators one with another, here in *England*, (if we allow in the two diſputed Caſes abovemention'd, that the Perſons died of the inoculated Small Pox) there will die *one in ninety one.*

But if thoſe two Perſons be allowed to have died of other Accidents or Diſeaſes, then we ſhall have reaſon to think, as far as any Judgement can be made from our own Experience here in *England*, that *none at all* will die of Inoculation, provided that proper Caution be uſed; as we are inform'd, on all hands, is the Caſe in *Turkey:* where *out of many thouſands*, that, in the ſpace of about fourty Years paſt, have been inoculated in and about *Conſtantinople*, by one *Greek* Woman, who ſtill continues that Practice, notwithſtanding her extreme old Age, *not ſo much as one Perſon has miſcarried*, as I am aſſured by the ingenious Dr. *le Duc*, a Native of *Conſtantinople*, who was himſelf inoculated there under the Care of his Father, an eminent Phyſician in that City.

I am, &c.

James Jurin

POSTSCRIPT.

SINCE this Paper was drawn up and communicated to the Royal Society, the following Account of the Succeſs of Inoculation in and about *Boſton*, in *New England*, was procured at my Deſire, by my Ingenious Friend Dr. *Nesbitt*, from Capt. *John Osborne*, who reſided in that Town and Neighbourhood during the whole time of that Practice. I think proper to inſert it here, as it confirms the Extract given above from Mr. *Mather*'s Relation, and is a more particular Account of the Matter of Fact, than any that I have yet ſeen.

In *May*, 1721. the Small Pox was, by the Providence of God, brought into the Town of *Boſton*; in *June* it began to ſpread pretty much, and in the Month of *July* it was got into moſt parts of the Town, and a conſiderable Number of People died of it. At this time Inoculation was firſt put in practice by Dr. *Boyleſton*, who then perform'd it upon his own Child and a Negro-Servant, who both did well; notwithſtanding which, this Attempt gave great Uneaſineſs to the Neighbours. However the Practice went on, to the Number of about 40 Perſons, one of which was a Woman * of about 40 or 45 Years of Age, who got well over the Small Pox, as her Husband publickly declared, but had been before troubled with Hyſterick Fits, of which ſhe died ſome little time after. When about 70 Perſons had paſſed under the Operation, myſelf and Wife, who had hitherto been at a Place called *Roxbury*, a Mile from *Boſton*, went into

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* *Mrs.* Dixwell.

Town and received the Small Pox by Inoculation. We had it with all the Gentleneſs and Moderation that was poſſible, neither of us having an hundred Puſtules, or being ſenſible of any Fever worth mentioning; ſo that we did not find it neceſſary to keep our Beds for it.

In *Auguſt* the Small Pox in the natural Way proved more mortal, and Inoculation made a greater Progreſs, the People continuing to come into the Practice of it. A ſecond Perſon that died after Inoculation, was an Apothecary's Houſekeeper, that was out of Town, till an *Indian* Maid got the Diſtemper in the ſame Houſe, and remov'd, and died. Upon which this Woman coming to Town, her Maſter undertook to perform the Operation upon her, which by the Way was the firſt and laſt that he ever perform'd; and on the third Day after the Inoculation, the Small Pox came out upon her very full; from which it was plain, that ſhe had taken the Infection before, in the common way.

The third Perſon that died after being inoculated, was a † Gentleman, that lodged in the ſame Houſe with my Wife and ſelf at *Roxbury*, who was under great and extreme Infirmity of Body, as myſelf was Witneſs, that we fear'd he would have lived but a ſhort time under it. His Friends much perſuaded him to make uſe of Inoculation, believing that it would have carry'd off his Illneſs; but when he made the Experiment, he had not Strength to go thro' with it. He was about 45 Years of Age, and by the way was a Gentleman of great Worth and Piety.

His Siſter ‡ was the fourth Perſon that died upon this Operation. She was about fourty Years of Age, of great Indiſpoſition of Body, and weak, as was her Brother.

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† John White, *Eſq*;

‡ *Mrs.* Wells.

The fifth, that died upon Inoculation, was a Woman Servant in a Houſe, where the whole Family, to the number of eight, were inoculated at the ſame time. She lay in a cold upper Room during her Illneſs, and was much neglected, the whole Family being down together, ſo that ſhe died merely for want of a little Attendance. This was in the Town of ***Roxbury***, where obſerve, that 13 Men, Maſters of Families, got the Small Pox, and all died; which inclined the People to make uſe of Inoculation, having before been much againſt it, and there were 43 Men inoculated there, who all did well. The * Miniſter of the Town was the firſt, that put it in Practice there, much againſt the Mind of his People at firſt, though afterwards they were very well pleas'd with it, ſeeing with what great Succeſs it was attended; and then whole Families came into it, and underwent the Operation. There were in all at leaſt 280 Perſons inoculated, that I knew of, and I ſuppoſe there might be about 20 or 30 more, but of thoſe I can give no certain Account.

John Osborne.

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* *The Reverend Mr.* Walter.

VII. *Dr*

VII. *De Sectione Anguli, Autore* A. de Moivre, R. S. S.

INeunte Anno 1707, incidi in Methodum quâ, Æquatione datâ hujus formæ.

$$ny + \frac{nn - 1}{2 \times 3} Ay^3 + \frac{nn - 9}{4 \times 5} By^5 + \frac{nn - 25}{6 \times 7} Cy^7 \text{ \&c.} = d,$$

Vel iſtius,

$$ny + \frac{1 - nn}{2 \times 3} Ay^3 + \frac{9 - nn}{4 \times 5} By^5 + \frac{25 - nn}{6 \times 7} Cy^7$$

&c. $= a$; ubi quantitates A, B, C, &c. repræſentant Coefficientes Terminorum præcedentium, Radices determinavi ad hunc modum.

Poſito $a + \sqrt{aa + 1} = v$ in primo caſu.

$a + \sqrt{aa - 1} = v$ in ſecundo.

Erit $y = \frac{1}{2} \sqrt[n]{v} - \frac{\frac{1}{2}}{\sqrt[n]{v}}$ in primo caſu.

$y = \frac{1}{2} \sqrt[n]{v} + \frac{\frac{1}{2}}{\sqrt[n]{v}}$ in ſecundo.

Solutiones autem iſtæ inſertæ fuerunt in Philoſophicis Tranſactionibus, Num. 309, pro menſibus *Jan. Feb. Mart*. ejuſdem anni.

Jam quibus perſpectum erit quo artificio Formulæ iſtæ inventæ fuerint, his procul dubio patebit aditus ad demonſtrationem ſequentis Theorematis.

Sit x Sinus Verſus Arcus cujuſlibet.
t Sinus Verſus Arcus alterius.
1 Radius Circuli.

Sitque Arcus prior ad poſteriorum ut 1 ad n, Tunc, aſſumptis binis Æquationibus quas cognatas appellare licet,

$$1 - 2z^n + z^{2n} = -2z^n t$$
$$1 - 2z + zz = -2zx.$$

Expunctoque z, orietur Æquatio qua Relatio inter x & t determinatur.

COROLLARIUM I.

Si Arcus poſterior ſit Semicircumferentia, Æquationes erunt.

$$1 + z^n = 0$$
$$1 - 2z + zz = -2zx.$$

e quibus ſi expungatur z, orietur Æquatio quâ determinantur Sinus Verſi Arcuum qui ſint ad Semicircumferentiam, ſemel, ter, quinquies, &c. ſumptam, ut 1 ad n.

CORROLLARIUM II.

Si Arcus poſterior ſit Circumferentia, Æquationes erunt

$$1 - z^n = 0$$
$$1 - 2z + zz = -2zx.$$

e quibus ſi expungatur z, orietur Æquatio quâ determinantur Sinus Verſi Arcuum qui ſint ad Circumferentiam, ſemel, bis, ter, quater, &c. ſumptam, ut 1 ad n.

CORALLARIUM III.

Si Arcus poſterior ſit 60 Graduum, Æquationes erunt

$$1 - z^n + z^{2n} = 0$$
$$1 - 2z + zz = -2zx.$$

e quibus ſi expungatur z, orietur Æquatio quâ determinantur Sinus Verſi Arcuum qui ſint ad Arcum 60 Graduum.

$$\text{per} \left\{ \begin{matrix} 1, & 7, & 13, & 19, & 25 \\ 5, & 11, & 17, & 23, & 29 \end{matrix} \;\&c. \right\} \text{multiplicatum}$$

ut 1 ad n.

Si Arcus poſterior ſit 120 Graduum, Æquationes erunt

$$1 + z^n + z^{2n} = 0$$
$$1 - 2z + zz = -2zx.$$

e quibus ſi expungatur z, orietur Æquatio quâ determinantur Sinus Verſi Arcuum qui ſint ad Arcum 120 Graduum.

$$\text{per} \left\{ \begin{matrix} 1, & 4, & 7. & 10, & 13 \\ 2, & 5, & 8, & 11, & 14 \end{matrix} \;\&c. \right\} \text{multiplicatum}$$

ut 1 ad n.

Novemb. 15.
1722.

VIII. *An Account of a new ſort of* Moloſſes *made of Apples; and of the degenerating of Smelts. By the Honourable* Paul Dudley, *Eſq; F. R. S.*

THE Apple, that produces the *Moloſſes*, is a Summer-Sweeting, of a middling Size, pleaſant to the Taſte, and full of Juice, ſo that ſeven Buſhels will make a Barrel of Cyder. The manner of making it is thus; you grind and preſs the Apples, and then take the Juice and boil it in a Copper till three Quarters of it is waſted, which will be done in about ſix Hours gentle boiling; and by that Time it comes to be of the Sweetneſs and Conſiſtency of *Moloſſes*.

Some of our People ſcum the Cyder as it boils, others do not, and yet there ſeems to be no great difference in the Goodneſs.

This new *Moloſſes* anſwers all the Ends of that made of the ſweet Cane imported from beyond Sea. It ſerves not only for Food and brewing, but is of great Uſe alſo in preſerving of Cyder; two Quarts of it put into a Barrel of rack'd Cyder, will both preſerve and give it a very agreeable Colour.

The Apple *Moloſſes* was diſcovered a few Years ſince, by a * Gentleman of my Acquaintance, at *Woodſtock*, in this Province, a Town remote from the Sea, and where the *Weſt India Moloſſes* is dear and ſcarce; he ingenuouſly confeſſes the Diſcovery was purely accidental; but ever ſince he has ſupplied his Family with *Moloſſes* out of his Orchard, and his Neigbours alſo now do the like to their great Advantage.

---

* J. Chandler, *Eſq;*

Our Country Farmers run much upon planting Orchards of theſe Sweetings, for fatting their Swine, and aſſure me it makes the beſt ſort of Pork. And I know the Cyder made of them to be better than that of other Fruit for Taſte, Colour, and keeping.

Two ſhort Miles from my Houſe we have a fine Pond, of half a Mile over, little or no Communication with the Sea. An ingenious man, ſome threeſcore Years ago, for an Experiment, took a Pail of large Smelts from the River and put them into this Pond, where they have increaſed abundantly, but are degenerated to a very ſmall ſort; for our River-Smelts I ſuppoſe are full as large as thoſe of the *Thames*, ſome of them I know, will weigh two Ounces and an half, whereas theſe ſmall ones will not weigh five Penny-Weight. We reckon the Pond-Smelt eats much better than the other, and then they are very tranſparent, and of a beautiful ſhining Pearl-Colour.

Roxbury, New England,
October 25, 1722.

Paul Dudley.

FINIS.

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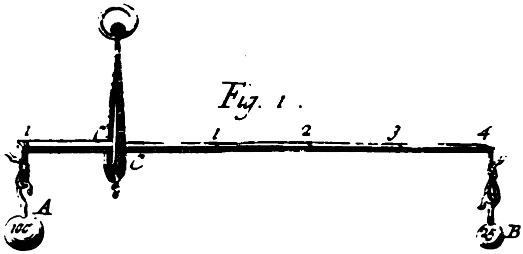
Fig. 1.



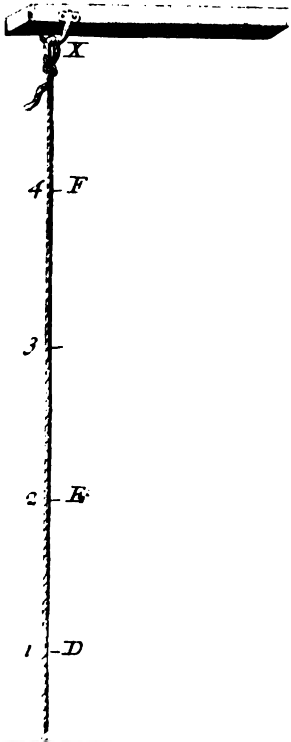
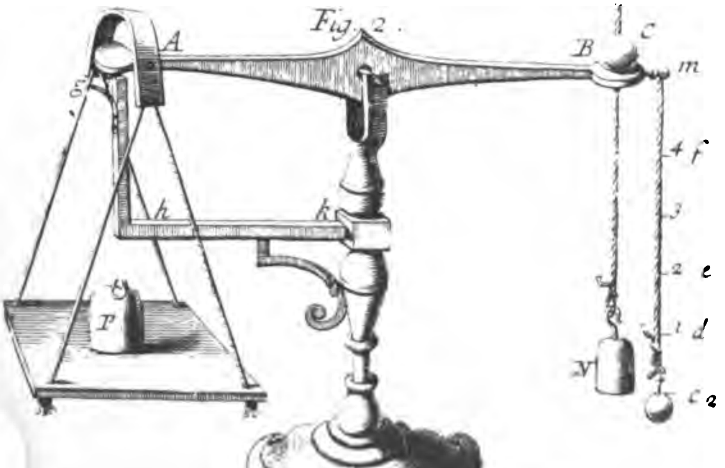


Fig. 2.



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PHILOSOPHICAL TRANSACTIONS.

For the Months of *January* and *February*. 1723.

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I. *Obſervations on the Eclipſe of the Moon,* June 18, 1722. *and the Longitude of* Port Royal *in* Jamaica *determined thereby. By Dr.* Halley, *Aſtronomer Royal, F.R.S.*

THE Eclipſe of the Moon which happened in *June* laſt, 1722, was ſo far hid by the cloudy Sky, that neither myſelf, nor any of our Aſtronomical Friends, in or about *London*, could furniſh an Obſervation thereof worthy to be laid before the Society. But the ſame having been well obſerved at *Jamaica*, by the late curious Capt. *Candler*, Commander of his Majeſties Ship *Launceſton*, (whoſe Example it were to be wiſhed, others in the like Poſt would imitate) and at *Berlin*, by Mr. *Chriſtfried Kirck*, Aſtronomer of the Royal Academy of Sciences there. I thought it not amiſs to prefix to their Accounts that little I was able to note concerning it.

Sunday, *June* 18, *mane*, Having perfectly rectified my Clock ſo as to ſhew the Apparent Time, neither the tranſit of the Moon over the Meridian, nor the beginning of the Eclipſe which ſoon followed, could be ſeen thro' the very thick Cloud. At 13 *h.* 12′ *T. app.* a ſmall Particle of the Moons Body was ſeen through a very little *hiatus* in the Cloud, by which glimpſe I could only be aſſured that the Eclipſe was not yet Total. At 13 *h.* 29′ by ſuch another view. I was ſatisfied that it was now become Total; but in a Moment, it again diſappeared, till 14 *h.* 49′ 10″, when the Cloud beginning to break, I got time to meaſure with the Micrometer, the *Partes Lucidæ* now recovered in the Moon's Diameter, which I found 14′. 00″, though this not ſo well as I could wiſh, by reaſon of a thinner Sort of Cloud which perpetually intercurr'd, and render'd the Edge of the Shadow ſomewhat dubious.

At 15 *h.* 15′ the Moon was pretty well got out of the thick Cloud, but being very low, and the Daylight become ſtrong, ſhe ſhone very faintly, and the Shadow became worſe and

From 15 *h*. 26′ to 15 *h*. 27′ *T. app*. I doubted of the End, and am confident it did not exceed the 27th Minute. It ended overagainſt the North Part of the *Palus Mæotis* of Mr. *Hevelius*, much about the middle of the Weſtern or Right-hand Limb of the Moon, ſhe being then very near ſetting.

Capt. *Barth. Candler*, being then at *Port Royal*, in *Jamaica*, had much better Fortune, and a ſerene Sky from the Beginning to the End; who having uſed due care to be aſſured of his Times, by Altitudes taken with an Inſtrument of three Foot *Radius*, was pleaſed to ſend us the Reſult of his Obſervation as follows.

	h.	′	″
The Eclipſe began	6	59	10
Immerſion	8	7	50
Emerſion	9	11	0
The End	10	19	40
Whence the Middle	8	39	25

And ſuppoſing the Eclipſe to have ended at *Greenwich*, at 15 *h*. 26′ ½ the Difference of Longitude between *Port Royal* and *Greenwich*, will be 5 *h*. 6′ 50″, or 5 *h*. 6′ ½ from *London*, that is, 76 *gr*. 37′ ½.

Mr. *Kirck* being in a more Eaſterly Meridian, could ſee nothing of the Emerſion, but has carefully noted the Time of the Beginning and Immerſion, as he obſerved them at *Berlin*, *viz*. the Beginning of the Eclipſe at 12 *h*. 59′ 55″ and the Immerſion at 14 *h*. 8′ 8″. Now by comparing ſeveral Obſervations made at both Places, we have formerly concluded *Berlin* to be 54 Min. of Time, or 13½ *grad*. of Longitude more Eaſterly than *London*; wherefore at *London* it began at 12 *h*. 5′ 55″ and immerged at 13 *h*. 14′ 8″ that is, the beginning was later here than at *Jamaica* 5 *h*. 6′45″, and the Immerſion later 5 *h*. 6′ 18″, punctually agreeing with what reſulted from my own Obſervation of the End as aboveſaid; and ſufficiently with what I had long ſince determined from Obſervations ſent me from *Jamaica* by my old Aſtronomical Friend Mr. *Charles Boucher*.

II. *The Longitude of* Carthagena *in* America. *By the ſame.*

HAving lately, by the Favour of Sir *Hans Sloane*, received a Packet of Obſervations from *Cartagene* in *America*, made by Colonel *Don Juan de Herrera*, Chief Engineer of that City, I find among them one Immerſion of the firſt Satellite of *Jupiter* into his Shadow, obſerved there by a Teleſcope of 17½ Feet, on *April* 9. *Stilo vet*. 1722. at 15 *h*. 58 ′ 44 ″ apparent Time; and two Emerſions of the ſame, *viz*. *July* 5. 11 *h*. 23′ 41″ and *July* 21. 9 *h*. 42′ 17″ *Stil. vet*. all which tally with Obſervations made at *Wanſted*, by the Reverend Dr. *Pound* and Mr. *Bradley*, who obſerved there the very next Eclipſes to all the three; that is to ſay, the Immerſion by a fifteen Foot Tube, on *April* 11. 15 *h*. 28 ′ 40″ *Temp. æqu*. or 15 *h*. 30 ′ 25 ″ *Temp. app*. And the firſt Emerſion, *July* 7. 10 *h*. 59 ′ 28″ *Temp. æq*. by the Reflecter, and 18 ″ after, or 10 *h*. 59 ′ 46 ″ by the 15 foot Glaſs, that is, 10 *h*. 54 ′ 12 ″ apparent Time. The other was obſerved at *Wanſted*. *July* 23. 9 *h*. 19 ′ 10″ *Temp. æq*. both by the Reflecter and 15 foot Glaſs; that is to ſay, at 9 *h*. 13 ′ 35 ″ apparent Time. Subſtract from each of theſe one Period of this Satellite, or 1 *d*. 18 *h*. 28 ′ 36 ″ and *April* 9. 15 *h*. 58 ′ 44 ″ at *Cartagene* will be 21 *h*. 1 ′ 49 ″ of the ſame Day at *Wanſted*, and the difference of Meridians 5 *h*. 3 ′ 5 ″. Likewiſe by the firſt Emerſion, *July* 5. 11 *h*. 23′ 41″ at *Cartagene* was at *Wanſted* 16 *h*. 25′ 36″ of that Day, whence the Difference of Meridians 5 *h*. 1 ′ 55″. But by the laſt E-

merſion, *July* 21. 9 *h*. 42′ 17″ at *Cartagene* was 14 *h*. 44′. 59″ at *Wanſted*; whence *Wanſted* is 5 *h*. 2′ 42″ more eaſterly than *Cartagene*: and taking the *Medium* of all three, 5 *h*. 2′ 34″ or 75° 38′ may be taken for the true difference of Longitude, that is, 75 ½ from *London*, which compared with Capt. *Candler*'s Obſervation of the late Lunar Eclipſe, ſhews *Cartagene* to be about 20 Leagues to the Eaſtwards of *Port Royal* in *Jamaica*.

III. *Cometæ* Berolini, *anno* 1718. *viſi Obſervationes a* 18 Januarii; *Stylo novo, ad* 5 Febr. *ex Epiſtolâ Viri Cl.* Chriſtfridi Kirchij, *Reg. Soc. Scient.* Berolin. *Aſtron. ad* Edm. Hallejum, *LL. D. R. S. S. deſumptæ.*

MOnere hic debeo obſervationes Cometæ à me inventi, in *Novis Literariis Lipſienſ.* non eſſe accuratas; primo quidem, cum eas tantum amico cuidam feſtinanter tranſmiſerim, ut etiam ille Cometam quæreret; deinde, cum etiam vitium typographicum irrepſerit; nam die 23 Januarii mane, Cometa cum θ & φ Caſſiopeæ (non vero δ & φ) conſtruebat triangulum æquicrurum; & veſperi φ Perſei, Cometa & θ Caſſiopeæ ad ſenſum erant in linea recta. Pleniorem Cometæ hiſtoriam jam paratam habeo, ex qua hæc breviter attingam. Obſervavi eum a die 18 *Jan.* ad 5 *Februarii.* Loca ejus ex obſervationibus ad horam 10 veſpertinam cujuſque diei, quo Cometa obſervari potuit, reducta, hæc Tabella exhibet.

	Longitudo.			Latitudo.		
	°	′		°	′	
18 *Jan.*	27	26	♋	69	18	S.
21 *Jan.*	16	25 ½	♉	48	42	S.
23 *Jan.*	9	28 ½	♉	39	45	S.
26 *Jan.*	5	25 ½	♉	32	55	S.
27 *Jan.*	4	41	♉	31	24	S.
28 *Jan.*	4	4	♉	30	13	S.
30 *Jan.*	3	4	♉	28	23 ½	S.
31 *Jan.*	2	43	♉	27	40	S.
1 *Febr.*	2	25	♉	27	1	S.
2 *Febr.*	2	10	♉	26	22	S.
5 *Febr.*	1	39	♉	24	53	S.

Via ejus tranſiit ſupra tergum Urſæ minoris, prope Polarem, per crura & genua Cephei, Caſſiopeæ & Andromedæ. Nodus ejus deſcendens fuit in 21 ½ gradu Arietis, cum aliqua mutatione: Angulus orbitæ cometicæ & Eclipticæ 69 ½ grad. circiter, etiam cum aliqua variatione. Via Cometæ 2 fere gr. à Polo mundi tranſiit, & Æquatorem ſecavit in 20 ½ gr. à puncto æquinoctiali. Perigæum Cometæ fuit in 6° 6′ ♍. cum latitudine ſeptentrionali 62° 7′. Cometa in Perigæo fuit, *D.* 18 *Jan.* hor. 3. min. 9. mane. Motus Cometæ diurnus in orbita propria, in Perigæo (12 ſcilicet horis ante, & 12 poſt Perigæum) 22° 8′; ultimis vero diebus apparitionis 32′. Suppoſita Terra quieſcente, & Cometa in recta linea trajiciente, motus Cometæ fuit 391 partium, qualium diſtantia minima Cometæ à Terra 1000. De Parallaxi Cometæ nihil certi affirmare poſſum, niſi quod multum ſupra Lunam fuerit elevatus Cometa. Probabiliter vero conjicio, illum intra Planetarum orbes exſtitiſſe, imo in Perigæo multo propiorem nobis fuiſſe

Martis Sphærâ. Sit enim ſemidiameter orbitæ Terræ 10000 partium, erit ita motus diurnus Martis 139 vel 140. Si vero Cometam in orbita Martis exſtitiſſe ſuppono, cum latitudine 62° 7′ & motu diurno 22° 8′, ejus velocitas eſſet 2847 partium, ſi ſcilicet ſimul fuiſſet in oppoſitione Solis; cum autem differentia Longitudinis Solis & Cometæ in Perigæo tantum fuerit 141° 40′, motus diurnus Cometæ evadit 3200 part. & proportio motus Cometæ ad motum Martis ut 23 ad 1. Quare colligo Cometam intra ſphæram Martis exſtitiſſe. Si vero quis Cometam ad Saturni orbitam evehere vellet, deberet ipſi velocitatem tribuere, quæ eſſet ad velocitatem Saturni ut 600 ad 1; & quod uno die majus ſpatium percurriſſet, quam Terra dimidio anno abſolvere ſoleat. Ne dicam de diametro Cometæ, quæ non multo minor exiſtere debuiſſet tribus diametris Solis.

Comparationem inſtitui hujus Cometæ cum aliis, & invenio Cometam, quem *Regiomontanus* anno 1472 vel 1475. menſe *Jan.* & *Febr.* obſervavit, viam tenuiſſe non multo diverſam à via noſtri Cometæ; tranſiit enim per Urſam minorem & Cephei femora, per pectus vel collum Caſſiopeæ & cingulum Andromedæ; ac velocitas ejus maxima uno die fuit 40 grad. Anno 1556, alius Cometa eſt obſervatus, cujus Nodos *Camerarius* in 11° ♎ & ♈ ponit, & qui prope pedes Urſæ minoris, per Cepheum, ſupra Caſſiopeam, & per partes ſuperiores Andromedæ tranſiit, motu valde veloci in Perigæo. Quod ſi *Regiomontanus* Cometam anno 1475 obſervavit, (de quo tamen Aſtronomi valde dubitant) admirabilis eſſet convenientia inter hoſce tres Cometas: intervallum enim prioris à medio eſſet 81 annorum, & à medio Cometâ ad ultimum 162 ann. ut ita revolutio Cometæ poſſet eſſet 81 annorum; nec etiam Hiſtoria aliorum Cometarum hiſce male reſponderet.

IV. *Extracts of ſeveral Letters to the Publiſher, from the Reverend Dr.* Langwith, *Rector of* Petworth *in* Suſſex, *concerning the Appearance of ſeveral Arches of Colours contiguous to the inner Edge of the common Rainbow.*

WHEN the Primary Rainbow has been very vivid, I have obſerv'd in it, more than once, a ſecond Series of Colours within, contiguous to the firſt, but far weaker, and ſometimes a faint Appearance even of a third. Theſe increaſe the Rainbow to a Breadth much exceeding what has hitherto been determin'd by Calculation. I remember, I had once an Opportunity of making an ingenious Friend take notice of this Appearance, who was much ſurpriz'd at it, as thinking it not to be reconciled with the Theory.

Petworth, Jan. 12.
172$\frac{1}{2}$.

SInce my laſt I have obſerv'd ſomething of the ſame Nature, though not in the ſame Degree of Perfection, with what I ſent you an Account of before. On *Saturday* laſt, a little before ſix in the Evening, Wind at N. W. by W. we had here a lively, diſtinct, primary Rainbow, the inner and purple Colour of which had a far greater Mixture of red in it, than I could ever obſerve in Sir *Iſaac Newton*'s oblong *Spectrum.* Under this was a ſpace, of a Breadth conſiderably leſs than that of the *Limbus* of the Rainbow, in which I could not diſtinguiſh any Colours: Still lower was a faint interrupted Arch of red, inclining to purple, which appear'd and vaniſh'd ſeveral times, while I was intent upon obſerving it.

My Suſpicion about this Phænomenon is, that the extraordinary Redneſs in the purple of the firſt Rainbow, is owing to the Mixture of the red Rays of a ſecond Series of Colours, with the purple Rays of the firſt; that the colourleſs Space conſiſted of Rays which are too weak to affect the Sight with diſtinct Colours; and that the innermoſt broken Arch was the ending of a ſecond and beginning of a third *Iris.*

P. S. I forgot to tell you, that as in the Account I ſend you here, I ſaw the purple of the ſecond *Iris* without the other Colours, ſo I have ſeen the other Colours, but not very diſtinct, without the purple: as alſo, that I could never ſee more than one Series of Colours near the Horizon.

Petworth, March 22.
172$\frac{1}{2}$.

YEſterday in the Evening, about a quarter before Six, Wind S. W. we had one of the fineſt Rain- that ever I beheld.

The firſt Series of Colours was as uſual, only the Purple had a far greater Mixture of red in it, than I have ever ſeen in the priſmatick Purple: Under this was a colour'd Arch, in which the green was ſo predominant,

dominant, that I could not diſtinguiſh either the yellow or the blue: Still lower was an Arch of purple, like the former, highly ſaturate with red, under which I cou'd not diſtinguiſh any more Colours.

I had the Pleaſure of viewing this delightful Object for a conſiderable time, without that vaniſhing and returning of Colours which I deſcrib'd in my laſt.

The Order of the Colours in this compounded Rainbow was, you ſee, red, yellow, green, blue, a Mixture of purple and red, green, (or rather a Mixture of yellow, green, and blue) a Mixture of purple and red.

I begin now to imagine, that the Rainbow ſeldom appears very lively without ſomething of this Nature, and that the ſuppos'd exact Agreement between the Colours of the Rainbow and thoſe of the Priſm, is the reaſon that it has been ſo little obſerv'd.

Petworth, March 27.
1722.

I Am afraid I ſhall tire you with the Hiſtory of this Phænomenon; but I have ſeen it in ſuch Perfection ſince the writing my laſt, that I cannot help being particular in my Account of it.

Auguſt the 21ſt, about half an hour paſt 5 in the Evening, Weather temperate, Wind at N. E. the Appearance was as follows; *viz.*

The Colours of the Primary Rainbow were as uſual, only the purple very much inclining to red, and well defin'd: Under this was an Arch of green, the upper part of which inclin'd to a bright yellow, the lower to a more duſky green: Under this were alternately two Arches of reddiſh purple and two of green: Under all a faint Appearance of another Arch of purple, which vaniſh'd and return'd ſeveral times ſo quick, that we cou'd not ſteadily fix our Eyes upon it. Thus the Order of the Colours was

I. Red, Orange Colour, Yellow, Green, Light Blue, Deep Blue, Purple.

II. Light Green, Dark Green, Purple.

III. Green, Purple.

IV. Green, faint vaniſhing Purple.

You ſee we had here four Orders of Colours, and perhaps the beginning of a fifth, for I make no queſtion but that what I call the Purple, is a Mixture of the Purple of each of the upper Series with the Red of the next below it, and the Green a Mixture of the intermediate Colours. I ſend you not this Account barely upon the Credit of my own Eyes; for there was a Clergyman and four other Gentlemen in Company, whom I deſir'd to view the Colours attentively, who all agreed, that they appear'd in the manner that I have now deſcrib'd.

There are two things, which well deſerve to be taken notice of, as they may perhaps direct us in ſome meaſure to the Solution of this curious Phænomenon.

The 1ſt is,

That the Breadth of the firſt Series ſo far exceeded that of any of the reſt, that as near as I could judge, it was equal to them all taken together.

The 2d is,

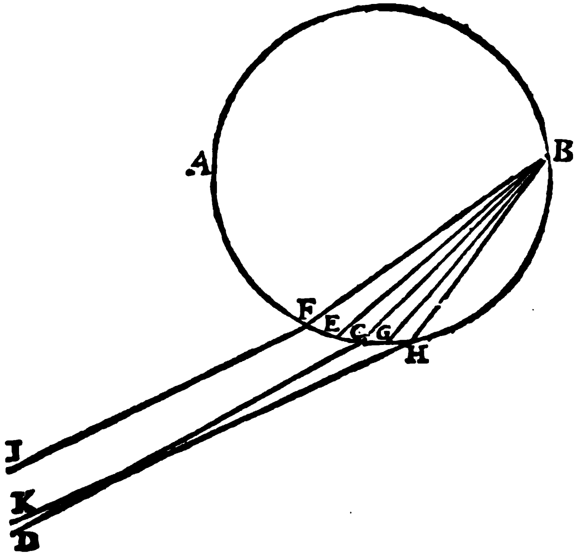
That I have never obſerv'd theſe inner Orders of Colours in the lower Parts of the Rainbow, tho' they have often been incomparably more vivid than the upper Parts, under which the Colours have appear'd. I have taken notice of this ſo very often, that I can hardly look upon it to be accidental, and if it ſhould prove true in general, it will bring the diſquiſition into a narrow compaſs; for it will ſhew that this Effect depends upon ſome Property, which the Drops retain, whilſt they are in the upper part of the Air, but loſe as they come lower, and are more mix'd with one another.

Petworth, Oct. 13.
1722.

V. *A Letter to Dr.* Jurin, *Coll. Med.* Lond. *Soc. & Secr. R. S. concerning the abovementioned Appearance in the Rainbow, with ſome other Reflections on the ſame Subject. By* Henry Pemberton, *M. D. R. S. S.*

SIR,

UPON your communicating to me the curious Obſervations, your Friend Dr. *Langwith* had made on the Rainbow, I inform'd you thoſe Appearances might, I thought, be explain'd by the Diſcoveries, the Great Sir *Iſaac Newton* had made in the Subject of Light and Colours, in his wonderful Treatiſe of *Optics*. As you ſeemed not diſpleaſed with what I mentioned to you in relation to this Matter by word of mouth, you deſired that I would ſet down in writing my Thoughts thereupon, which I have here accordingly done in the following manner.



Let AB repreſent a Drop of Rain, B the Point from whence the Rays of any determinate Species being reflected to C, and afterwards emerging in the Line CD, do proceed to the Eye, and cauſe the Appearance of that Colour in the Rainbow, which appertains to this Species. It is obſerved by Sir *Iſaac Newton* [a], that in the Reflection of Light, beſides what is reflected regularly, ſome ſmall part of it is irregularly ſcattered every way. So that from the Point B, beſides the Rays that are regularly reflected from B to C, ſome ſcattered Rays will return in other Lines, as in B E, B F, B G, B H, on each Side the Line B C. Further it muſt be noted from Sir *Iſaac Newton* [b], that the Rays of Light in their Paſſage from one Superficies of a refracting *Medium* to the other undergo alternate

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[a] Optics, *Book* II. *Part* 4. [b] Ibid. *Part* III. *Prop.* xij.

Fits of eaſy Tranſmiſſion and Reflection, ſucceeding each other at equal Intervals; inſomuch that if they reach the further Superficies in one ſort of thoſe Fits, they ſhall be tranſmitted; if in the other kind of them, they ſhall rather be reflected back. Whence the Rays that proceed from B to C, and emerge in the Line C D, being in a Fit of eaſy Tranſmiſſion, the ſcattered Rays that fall at a ſmall Diſtance without theſe on either ſide, (ſuppoſe the Rays, that paſs in the Lines B E, B G) ſhall fall on the Surface in a Fit of eaſy Reflection, and ſhall not emerge; but the ſcattered Rays, that paſs at ſome Diſtance without theſe laſt, ſhall arrive at the Surface of the Drop in a fit of eaſy Tranſmiſſion, and break through that Surface Suppoſe theſe Rays to paſs in the Lines B F, B H; the former of which Rays ſhall have had one Fit more of eaſy Tranſmiſſion, and the latter one Fit leſs, than the Rays that paſs from B to C. Now both theſe Rays, when they go out of the Drop, will proceed by the Refraction of the Water in the Lines F I, H K, that will be inclined almoſt equally to the Rays incident on the Drop, that come from the Sun, but the Angles of their Inclination will be leſs than the Angle, in which the Rays emerging in the Line C D are inclined to thoſe incident Rays. And after the ſame manner Rays ſcattered from the Point B, at a certain Diſtance without theſe, will emerge out of the Drop, while the intermediate Rays are intercepted; and theſe emergent Rays will be inclined to the Rays incident on the Drop in Angles ſtill leſs than the Angles, in which the Rays F I and H K are inclined to them; and without theſe Rays will emerge other Rays, that ſhall be inclined to the incident Rays in Angles yet leſs. Now by this means will be formed of every kind of Rays, beſides the principal Arch which goes to the Formation of the Rainbow, other Arches, within every one of the principal, of the ſame Colour, though much more faint: and this for divers Succeſſions, as long as theſe weak Lights, which in every Arch grow more and more obſcure, ſhall continue viſible. Now as the Arches produced by each Colour will be variouſly mixed together, the diverſity of Colours obſerved by Dr. *Langwith* may well ariſe from them.

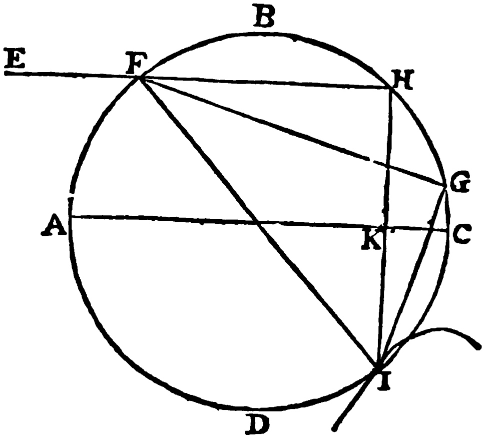
The preciſe Diſtances between the principal Arch of each reſpective Colour and theſe fainter correſpondent Arches depend on the Magnitude of the Drops of Rain. In particular, the ſmalleſt Drops will make the ſecondary Arches of each Species at the greateſt Diſtance from their reſpective principal, and from each other. Whence, as the Drops of Rain increaſe in falling, theſe Arches near the Horizon by their great Nearneſs to their reſpective principal Arches become inviſible.

AND now, Sir, we are upon the Rainbow, I ſhall here take the Freedom of ſetting down two Propoſitions, which I have formerly conſidered, relating to this Subject. For the greater Brevity I ſhall deliver them under the Form of Poriſms; as, in my Opinion, the Ancients called all Propoſitions treated by Analyſis only.

PROPOSITION I.

In a given refracting Circle, whoſe refracting Power is given, the Ray is given in Poſition, which paſſing parallel to a given Diameter of the Circle is refracted by that Circle to a Point given in the Circumference of it.

Let A B C D be the given Circle, the given Diameter A C, and given Point G; and let the Ray E F, parallel to A C, be refracted to G. I ſay E F is given in Poſition.



Produce E F to H, and draw the Diameter F I, drawing likewiſe I K H, I G. Then is H F I the Angle of Incidence, and G F I the refracted Angle; ſo that I H being perpendicular to F H and I G perpendicular to F G, I H is to I G as the Sine of the Angle of Incidence to the Sine of the refracted Angle, and the *Ratio* of I H to I G is given, as likewiſe the *Ratio* of I K to I G. Therefore I K being perpendicular to A C the Point I is in a Conic Section given in Poſition, whoſe Axis is perpendicular to A C, and one of its *Foci* is the Point G[a] Conſequently the Points I and F are given, and laſtly the Ray E F given in Poſition.

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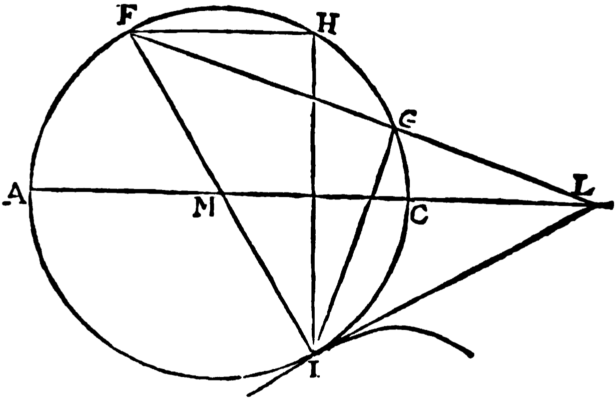
[a] *See* Papp. l. 7. prop. 238. Milnes Conic. part. 4. prop. 9.

DETER-

DETERMINATION.

IT is evident, that this conic Section, may either cut the Circle in two Points, touch it in one Point, or fall wholly without it. Therefore let the Section touch the Circle in the Point I, and let I L touch both the Section and the Circle in the ſame point I. Then G L being joined, the Angle under I G L on account of the conic Section is a right one[a], ſo that F G L is one continued right Line, and I F is to I L as F G to G I; as likewiſe, M being the Center of the Circle, M I to I L, or F H to H I, as F G to twice G I, becauſe M I is to I F as G I to twice G I. Hence by Permutation F H is to F G as H I to twice G I; that is, as the Sine of the Angle of Incidence to twice the of the refracted Angle.



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[a] De la Hire Conic. lib. 8. prop. 23.

Moreover F H being to H I as F G to twice G I, the Square of F H will be to the Square of H I, as the Square of F G to four times the Square of G I. Therefore, by Compoſition, as the Square of F H to the Square of F I or of A C, ſo is the Square of F G to the Square of F I together with three times the Square of G I, and ſo likewiſe is the Exceſs of the Square of F G above the Square of F H, which equals the Exceſs of the Square of I H above the Square of I G, to three times the Square of G I; for as one Antecedent to one Conſequent, ſo is the difference of the Antecedents to the difference of the Conſequents. Hence in the laſt place, the Square of half F H will be to the Square of A M, as the Exceſs of the Square of I H above the Square of I G to three times the Square of I G, or as the Exceſs of the Square of the Sine of Incidence above the Square of the Sine of Refraction, to three times the Square of the Sine of Refraction.

refracted Ray F G, and M K being taken to the Semidiameter of the Circle in the *Ratio* of the Sine of Incidence to the Sine of Refraction; let L M be joined, and laſtly make the Angle under K M N equal to half the given Angle under E I H. This being done, if F G be produced to O, F O ſhall be to K O as the Sine of the Angle of Incidence to the Sine of the refracted Angle, that is as M K to K L; in ſo much that K L being parallel to F O, and the Angle under M K L equal to that under F O K, the Angle under M L K ſhall be equal to that under F K O, and the Angle under K M L equal to that under K F O equal to that under F G K or half that under F G H, whence the Angle under K M N being equal to half the Angle under F I H, the reſiduary Angle under N M L will be equal to half the Angle under I F G or to half that under M K L. Therefore L C being drawn, the Angle under L M N will be equal to that under M C L; and in the laſt place, if M C be divided into two equal Parts in P, and P Q R be drawn parallel to C L, the Angle under Q M R will be equal to that under R P M, and the Triangles Q M R, M P R ſimilar, ſo that the Rectangle under P R Q ſhall be equal to the Square of M R. Whence R L being equal to M R, the Point L ſhall be in an equilateral *Hyperbola*, touching the Line M N in the Point M, and having the Point P for its Center [a]. But this *Hyperbola* is given in Poſition, and conſequently the Point L, the Angle under M L K, and the equal Angle under C K F will be given, and therefore the Ray E F is given in Poſition.

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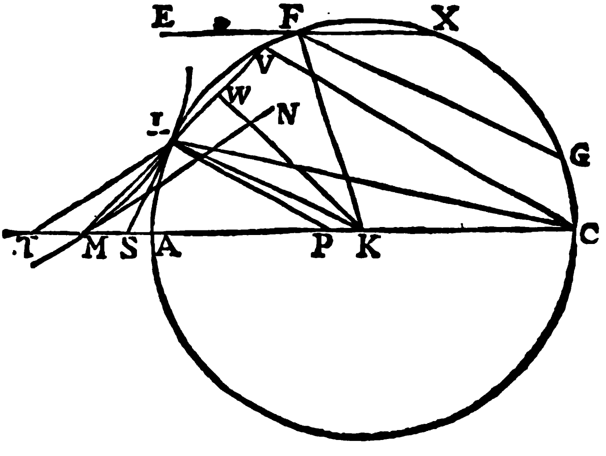
[a] Apoll. Conic. lib. 1. prop. 37. *compared with* lib. 7. prop. 23.

D E.

DETERMINATION.

Let the *Hyperbola* touch the Circle in the Point L, and let their common Tangent be L S; draw L T parallel to M N, ſo as to be ordinately applied in the *Hyperbola* to the Diameter C M. Whence L S touching the *Hyperbola* in L, P T will be to T L as T L to T S [a], and the Angle under T S L equal to that under T L P, but as the Angle under S C L is equal to that under N M L, the ſame is equal to the Angle under T L M; therefore the Angle under S L C is equal to the Angle under M L P. Farther, M L being produced to V and V C joined, the Angle under L V C is equal to that under S L C, by reaſon that L S touches the Circle in L; hence the Angles under L V C and under M L P are equal, L P, V C are parallel, and



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[a] Apoll. Conic. lib. 1. prop. 37. *compared with* lib. 7. prop. 23.

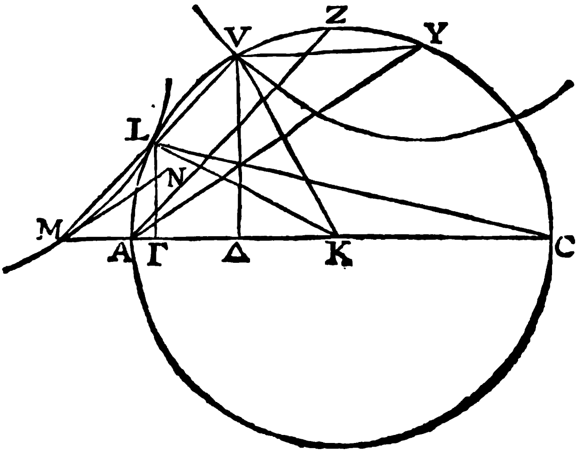
M P being equal to P C, M L is equal to L V; and K W being let fall perpendicular to LV, M W is equal to three times L W. But now if the incident Ray E F be produced to X, the Angle under M L K being equal to that under C K F, or to that under E F K, F X ſhall be equal to LV, equal to twice L W; and the Angle under K M L being equal to that under K F G; ſince K W is perpendicular to M W, F G ſhall be to twice M W as M K to K F, or as the Sine of Incidence to the Sine of Refraction: whence M W being equal to three times L W, F X ſhall be to F G as the Sine of Incidence to three times the Sine of Refraction.

Moreover, M W being equal to three times L W, the Square of M W will be equal to nine times the Square of L W, and the Rectangle under V M L, or the Rectangle under C M A, that is, the Exceſs of the Square of K M above the Square of K A, will be equal to eight times the Square of L W; therefore the Square of L W or the Square of half F X will be to the Square of K L, or of K A, as the Exceſs of the Square of K M above the Square of K A to eight times the Square of K A, that is, as the Exceſs of the Square of the Sine of Incidence, above the Sine of Refraction to eight times the Square of the Sine of Refraction.

Another

Another DETERMINATION.

Draw A Y parallel to M N, and A Z parallel to M V: then is the Angle under Y A *Z*, equal to that under L M N, which is equal to that under L C A; whence the Arches A L, Y *Z* are equal; but the Arches A L, V Z are likewiſe equal, becauſe LV, AZ are parallel, therefore Y V being joined, and L Γ drawn perpendicular to A C, the Chord V Y ſhall be the double of L Γ; but V Δ being likewiſe let fall perpendicular to A C, becauſe M V is the double of M L, V Δ ſhall be the double of L Γ; and therefore V Δ and V Y ſhall be equal; whence the point V ſhall be in a *Parabola*, whoſe *Focus* is the Point Y, its Axis perpendicular to A C, and the *Latus rectum*, belonging to that Axis, equal to twice the perpendicular let fall from Y upon A C [a]. But if K V be joined, the



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[a] Vide de la Hire Sect. Conic. lib. 8. prop. 1, 3.

Angle under L K V is equal to twice the Complement to a right Angle of the Angle under K L V, which is equal to the Angle of Incidence, and exceeds the refracted Angle by the Angle under A K L.

THE Determinations of theſe two Propoſitions, have relation to the firſt and ſecond Rainbow; thoſe of the firſt Propoſition reſpecting the interior, and thoſe of the ſecond the exterior. The firſt Determinations of theſe two Propoſitions aſſign the Angles, under which each Rainbow will appear in any given refracting Power of the tranſparent Subſtance, by which they are produced; the latter Determinations of theſe Propoſitions teach how to find the refracting Power of the Subſtance, from the Angles under which the Rainbows appear; the Angle under C M G, in the Determinations of the firſt Propoſition, being half the Angle which meaſures the Diſtance of the interior Bow from the Point oppoſite to the Sun; and in the Determinations of the ſecond Propoſition, the Angle under C M N is half the Complement to a right Angle of half the Angle that meaſures the Diſtance of the exterior Bow, from the Point oppoſite to the Sun. But whereas theſe latter Determinations require ſolid Geometry, it may not be amiſs here to ſhew how they may be reduced to Calculation, ſeeing the Obſervation of theſe Angles, as the learned Dr. *Halley* has already remark'd [a], affords no inconvenient Method of finding the refracting Power of any Fluid, or indeed of any tranſparent Subſtance, if it be formed into a ſpherical or cylindrical Figure. For this purpoſe therefore I

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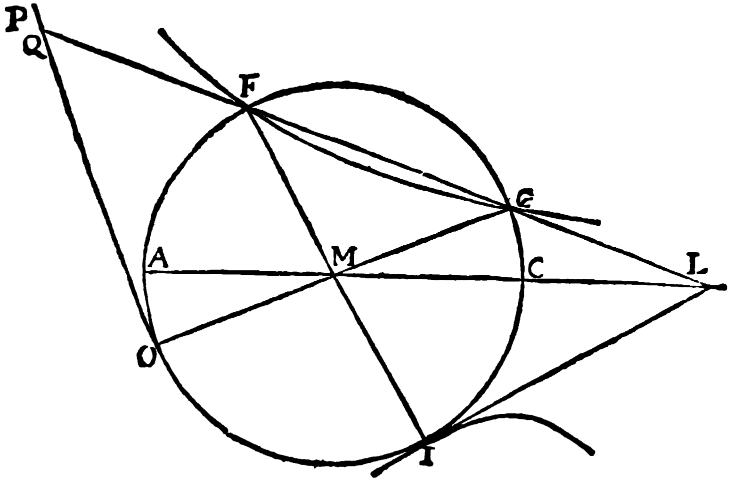
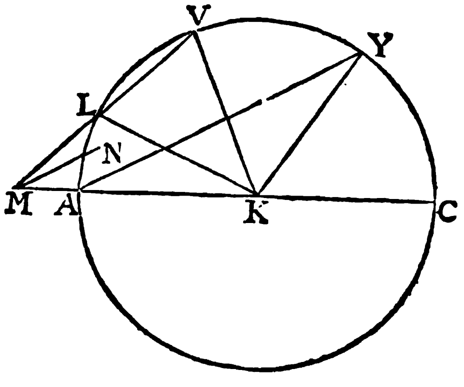
[a] *Philoſoph. Tranſact.* No. 267. *pag.* 722.



have found, that in the latter Determination of the firſt Propoſition, if the Sine of the Angle under CMG be denoted by *a*, the Tangent of the Complement of this Angle to a right one be denoted by *b*, and the Secant of this Complement by *c*; the Root of this Equation $z^3 - 3aaz = 2aa\sqrt{2c - a}$ will exceed the Sine of the Angle under FMA, that is the Sine of the Angle of Incidence, by the Sine of the Angle under CMG; and the Sine of the Angle under FMO, which is double the refracted Angle, will be the Root of this Equation $x^3 + 3aax = 4aab$; this Angle being acute, when the Tangent of the Angle under CMG is leſs than half the *Radius*, or when the Angle itſelf is leſs than 26 *degr*. 33′. 54″. 11‴, and when this Tangent is more than half the *Radius*, the Angle under OMF is obtuſe.

The Roots of theſe cubic Equations are found by ſeeking the firſt of two mean Proportionals, between each of the verſed Sines appertaining to the Arches C G, A G, and the Sine of thoſe Arches, counting from the verſed Sines; for the Sum of theſe two mean Proportionals is the Root of the former Equation, and the difference between them the Root of the latter; as may be collected from *Cardan*'s Rules.

And hence likewiſe if the firſt and laſt of the five mean Proportionals, between the Sine and Coſine of half the Angle under C M G be found, twice the Sum of the Squares of theſe mean Proportionals applied to the *Radius* exceeds the Sine of the Angle of Incidence by the Sine of the Angle under C M G; and twice the difference of the Squares of the ſame mean Proportionals applied to the *Radius* is equal to the Sine of double the refracted Angle. Moreover this double of the refracted Angle exceeds the Angle of Incidence by the Angle under C M G.



In the latter Determination of the ſecond Propoſition draw K Y, and A Y being parallel to M N, the Angle under C K Y will be equal to twice the Angle under CMN, that is equal to the Complement of half the Diſtance of the exterior Rainbow from the Point oppoſite to the Sun. Then putting a for the *Radius* AK, and b for the Sine of the Angle under CKY, the Sine of the Angle under AKV will be the Root of this Equation $y^4 + 4by^3 - 8aaby + 4aabb = 0$. But the Angle of Incidence and Refraction may alſo be found as follows.

Let two mean Proportionals between the *Radius* and the Sine of the Angle under CKY be found, then take the Angle, whoſe Coſine is the firſt of theſe mean Proportionals, counting from the *Radius*; and alſo the Angle, whoſe Sine together with the ſecond mean Proportional ſhall be to the *Radius* as the Coſine of the Angle under CKY to the Sine of the Angle before found. The Sum of theſe three Angles is double the Complement to a right one of the Angle under AKL, the Angle under KML, or the refracted Angle, being equal to half the Sum of this Angle under AKL and the Angle under CKY; as in the laſt Place the Angle under KLV, that is the Angle of Incidence, equal to the Sum of the Angles under KML and under MKL.

I need not obſerve, that the geometrical Methods of deducing theſe Angles of Incidence and Refraction from the Angle meaſuring the Diſtance of each Rainbow from the Point oppoſite to the Sun, afford very expeditious mechanical Conſtructions.

ter to that part; which by Degrees growing inflam'd, about a Week afterwards he fell into the Small Pox; and that he has ſince frequently converſed with ſuch as were ſick of that Diſtemper. He ſays alſo five or ſix more at leaſt of his Schoolfellows made the like Experiment on themſelves, at the ſame time, with the like Succeſs.

I have ſince talk'd with ſeveral more, who made the like Experiments on themſelves, ſome twenty, ſome ſixteen, *&c.* Years ago: who all poſitively affirm, they never had the Small Pox a ſecond time.

Haverford Weſt,
Nov. 23. 1722.

Perrot Williams.

VII. *Part of a Letter from the ſame Learned and Ingenious Gentleman, upon the ſame Subject, to Dr.* Jurin, *R. S. Secret.*

THE Buſineſs of my Profeſſion obliging me to go into the Country, as ſoon as I had the Favour of yours of *Jan.* 22. prevented my returning an Anſwer ſooner to the Queries you are pleas'd to propoſe.

I have little to add to what I have already mention'd to Dr. *Brady*, concerning the manner of communicating the Small Pox, more than that Mr. *Owen* was about fifteen Years of Age, when he made the Experiment on himſelf, and that he queſtionleſs had the genuine Small Pox; the Signs of 'em on his Face, and the Mark on his Hand, where he apply'd the Matter, being ſtill ſo very viſible, as to put that matter beyond Diſpute. The Phyſician, who then attended him, is dead. Tho' I can't pretend to remember how many inform'd me of their procuring the Diſtemper in this manner; yet I can aver, that within the compaſs of twenty Years laſt paſt, I have been ſo often aſſured of the Truth of it, not by Children, but grown Perſons of undoubted Credit, that I am entirely ſatisfied it has been an immemorial Cuſtom in theſe Parts; and not only practiſed by Boys when at School, but alſo by many others of both Sexes more advanced in Years, and conſequently capable of diſtinguiſhing the Small Pox from other Diſtempers. There are now living, in this Town and Neighbourhood, five or ſix Perſons, who undoubtedly had that Diſtemper after taking the foreſaid Method to infect themſelves; one of whom, a young Woman aged 23, told me (ſince I received your Letter) that, about 8 or 9 Years ago, in order to infect herſelf, ſhe held twenty pocky Scabs (taken from one towards the latter end of the Diſtemper) in the hollow of her Hand, a conſiderable time; that about ten or twelve Days afterwards ſhe ſicken'd, and had upwards of thirty large Puſtules in her Face, and other Parts; and that ſhe has ſince freely converſed with ſuch as have had the Small Pox on them.

To make it appear that Inoculation is a ſufficient Preſervative againſt receiving the Small Pox a ſecond time; about ſix Weeks ago, I cauſed my two Boys, who had been inoculated this laſt Summer, not only to ſee, but even to handle a Child, dying of a moſt malignant ſort of Small Pox; who notwithſtanding, I thank God, continue in perfect Health.

Upon a very exact Inquiry I find, that out of 227, who have had the Small Pox in the natural way, in this Town and a neighbouring Pariſh, ſince the Beginning of *June* laſt, 52 have died.

Haverford Weſt,
Feb. 2. 172$\frac{3}{4}$.

Perrot Williams.

POSTSCRIPT.

Juſt as I was concluding my Letter, being ſent for by a Gentleman about ſix Miles off, (where I ſaw two very melancholy Inſtances of the Severity of the confluent Small Pox) he took an occaſion to inform me, that a near Neighbour of his caus'd his Son, about ten Years of Age, near three Weeks ago, to buy (as he term'd it) the Small Pox, after the manner I deſcribed in my firſt Letter to Dr. *Brady*. Carefully viewing the Boy, I found him recovering from the diſtinct Kind of that Diſtemper, having not had, as I could perceive, above 40 pretty large Puſtules, which were then drying off. Had I not been ſcanted in Time, I queſtion not but I ſhould have been able to have given a great many Inſtances of the like Nature; but the hurry I have been continually in, ever ſince I receiv'd yours, muſt be my Apology, for that Deficiency, as well as the Uncorrectneſs of this Letter.

VIII. *A Letter on the ſame Subject, from Mr.* Richard Wright, *Surgeon at* Haverford Weſt, *to Mr.* Sylvanus Bevan, *Apothecary in* London.

I Received yours the 9th inſtant, and, in Anſwer to it, will readily give you all the Satisfaction I can, in relation to a very antient Cuſtom in this Country; commonly called *buying the Small Pox*, which, upon a ſtrict Inquiry, ſince I had your Letter, I find to be a common Practice, and of very long ſtanding; being aſſured by Perſons of unqueſtionable Veracity, and of advanced Age, that they have had the Small Pox commuuicated to themſelves this way, when about ſixteen or ſeventeen Years of Age, they then being very capable of diſtinguiſhing that Diſtemper from any other; and that they have parted with the Matter contain'd in the Puſtules to others, producing the ſame Effects. There are two large Villages in this County near the Harbour of *Milford*, more famous for this Cuſtom than any other, namely, *St. Iſhmael*'s and *Marloes*. The old Inhabitants of thoſe Villages, (with which they abound, being in a healthful Situation) ſay, that it has been a common Practice with them time out of mind; and what was more remarkable, one *William Allen*, of *St. Iſhmael*'s, ninety Years of Age, (who died about ſix Months ago or thereabouts) declared to ſome Perſons of good Senſe and Integrity, that this Practice was uſed all his Time; that he very well remember'd his Mother's telling him, that it was a common Practice all her Time, and that ſhe got the Small Pox that way. Theſe, together with the many other Informations, I have met with from almoſt all parts of the County, confirm me in the Belief of its being a very antient and frequent Practice, among the common People; and to prove that this Method is ſtill continued among us, I will give you the relation of an elderly Woman, a Midwife, who accidentally came into Company, when your Letter was reading, whoſe Name is *Joan Jones*, aged ſeventy Years, of good Credit, and perfect Memory. She ſolemnly declares, that about fifty four Years ago, having then the Small Pox, one *Margaret Brown* (to the beſt of her Remembrance then about twelve or thirteen Years of Age) bought the Small Pox of her; that the ſaid *Margaret Brown* was ſeized with the Small Pox a few Days afterwards; that the ſaid *Margaret Brown* had not had the Small Pox a ſecond time, a twelve-Month ago, and ſhe verily believes, that ſhe has not had them ſince. She farther ſays, that ſhe has known this way of procuring the Small Pox practiſed from time to time, above fifty Years; that it has been lately uſed in her Neighbourhood, and that ſhe knows of but one dying of the ſaid Diſtemper, when communicated after the Method aforeſaid, which Accident happened within theſe two Years laſt paſt; the Perſon who miſcarried (a young Woman about twenty Years of Age) having procured the Diſtemper from a Man then dying of a very malignant Small Pox. The above Relation I heard the old Woman declare two Days ago, and ſhe was willing to take her Oath of it before Dr. *Williams*, who is a Magiſtrate. As to what you mention concerning the manner of communicating the infectious Matter to the Blood, by ſcraping the Skin thin with a Penknife, and ſo rubbing in the Matter, that was only the Caſe of one particular Gentleman, Mr. *Owen*, a Counſellor at Law, whom I heard ſeveral times poſitively affirm, that he bought the Small Pox when at School, and of ſuch a Lady, now living, and gave her three Pence for the Matter contained in 12 Puſtules. That hundreds in this Country have had the Small Pox this way is certain; and it cannot produce one ſingle Inſtance of their ever having them a ſecond time.

Haverford Weſt,
Feb. 15. 172$\frac{2}{3}$.

Richard Wright.

IX. *An Account of ſome Experiments made to prove, that the Force of Moving Bodies is proportionable to their Velocities: (or rather that the* Momentum *of Moving Bodies is to be found by multiplying the Maſſes into the Velocities) In Anſwer to ſuch who have ſometime ago affirm'd, that that Force is proportionable to the Square of the Velocity, and to thoſe who ſtill defend the ſame Opinion. By the Reverend* John Theophilus Deſaguliers, *LL.D. F.R.S.*

AS far as I can learn, Monſieur *Leibnitz* was the firſt that oppos'd the receiv'd Opinion, concerning the Quantity of the Force of moving Bodies; by ſaying, that it was to be eſtimated by multiplying the Maſs of the Bodies, not by their Velocity, but by the Square of it. But, inſtead of ſhewing any Paralogiſm, in the mathematical Demonſtrations, which

are

are made uſe of to prove the Propoſition, or any Miſtakes in the Reaſonings from the Experiments made to confirm it, he uſes other *Mediums* to prove his Aſſertions [a]; and without any Regard to what others had ſaid on that Subject, brings new Arguments, which the Reverend and Learned Dr. *Clark* has fully anſwered in his fifth Letter to him. Meſſieurs *John Bernoulli*, *Wolfius*, *Hermannus*, and others, have follow'd and defended Monſieur *Leibnitz*'s Opinion, and in the ſame manner, ſo that what is an Anſwer to him, is ſo to them.

Polenus (Profeſſor at *Padua*) has acted after the ſame manner in the experimental Way, making ſome Experiments to defend Monſieur *Leibnitz*'s Opinion, without having ſhewn thoſe to be falſe. which are made uſe of to prove the contrary [b]; and now lately, an ingenious Profeſſor abroad (who was of the Opinion commonly receiv'd, and in his Writings had demonſtrated it in the uſual way [c], confirming it with the common Experiments made in that Caſe) happening to make ſome Experiments like thoſe of *Polenus*, has drawn Concluſions from them to ſhew the Force of moving Bodies to be proportionable to the Square of their Velocity; and being wholly come over to that Opinion, endeavours to deduce it from Phyſical Principles.

As there can hardly be ſaid any thing new, or better than has been ſaid, to ſhew the Force abovemention'd to be proportionable to the Maſs multiplied into the Velocity; I only repeat here the Subſtance of what others have ſaid, and make ſome old Experiments over again; but then I conſider ſome Circumſtances, that perhaps have been overlook'd, and at laſt, by a new Experiment, endeavour to ſhew, what has led into an Error ſome of thoſe, who defend the new Opinion.

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[a] Acta Erudit. ad ann. 1686. p. 162. [b] Polenus de Caſtellis, p. 56, 57, &c. [c] Graveſande Introductio, Vol. I. No. 132.

If a Man with a certain Force can move a Weight of fifty Pounds, through a Space of four Feet, in a determinate time; it is certain he muſt employ twice that Force to move one hundred Pounds Weight, through the ſame Space in the ſame time. But if he uſes but the ſame Force, he will move the one hundred Pounds Weight but two Feet in the ſame time. For as the one hundred Pounds Weight contains two fifty Pound Weights, if each of them has two Degrees of Velocity given to it, it will exactly require the ſame Force that would give one of them four Degrees of Velocity; hence it appears, that the Force is proportionable to the Maſs multiply'd into the Velocity.

EXPERIMENT I.

Fig. 1. Let the Balance A B, whoſe *Fulchrum*, or Center of Motion, is at C, be ſo divided, that the *Brachium* A C be but the fourth Part of the *Brachium* C B; it is known to all Mechanicians, that a Weight of one hundred Pounds at A, will keep in *Æquilibrio* a Weight of twenty five Pounds hanging at B, where it will have a Velocity four times greater than that of the Weight at A. For, not only when the Balance is horizontal, there will be an *Æquilibrium*, but when the Balance is put in Motion, it will return to an *Æquilibrium* in an horizontal Poſition; the equal and contrary Forces applied at each, deſtroying one another. Whereas, if the Forces were as the Maſs multiply'd into the Square of the Velocity, the twenty five Pound Weight ſhould have been ſuſpended at D, only twice as far from C, as the Weight at A; and in general, let the make of the Engine be what it will, let the mechanical Powers be combined in any manner, when two heavy Bodies, by means of a Machine, act upon one another in different directions, if their Velocities are reciprocally as their Maſſes, they will deſtroy each others Forces and come to reſt.

As this is true in reſpect of mechanical Powers, ſo it is in reſpect to the Shock or Blow given by falling Bodies. An heavy Body, falling with an accelerated Motion, goes through a ſpace of one Foot in a quarter of a Second, and acquires a Velocity, which would carry it two Foot in the ſame Time with an uniform Motion; the ſame Body falls through a ſpace of four Foot in half a Second, and acquires a Velocity, that would with an uniform Motion carry it eight Foot in half a Second. Therefore, as the Time of the fall through a ſpace of four Foot is twice the Time of a fall through one Foot, the Velocity in the latter Caſe is double that of the firſt, and conſequently the Blow, that the Body will give, will be double.

EXPERIMENT II.

Fig. II. Let the Weight P of one Pound, be placed in the Scale ſuſpended at the end A, of the Ballance A B, which bears upon the *Gnomon*, or Iron Supporter, *k h i*. Then if the Weight C be let fall from D, or one Foot, it will by its Stroke on the end of the Beam B, raiſe up the oppoſite end A with the Weight P, ſo high, that the Spring *g h* will fly from the Button *i*, which kept it ſtreight and upright before the Shock. If the Weight P be of two Pounds, it cannot be raiſed by the fall of C from any height leſs than F or four Foot; whereas, if the force of the Shock was proportionable to the Space, without any regard to the time, as Monſieur *Leibnitz* and his followers have affirmed, P ought to be raiſed, when C falls only from E, or two Foot, which never happens; or, if the Stroke was proportionable to the Maſs multiplied into the ſquare of the Velocity, when C falls from F, then P might weigh four Pound, whereas the Experiment will never ſucceed under thoſe Circumſtances.

I know it is objected, that the Blow cannot be always direct, and that the String that goes through the hole in the falling Weight, to guide it in its Fall, cauſes a ſenſible Friction, and therefore that ſomething of the Force is thereby loſt. But we are to obſerve, that if that was all, there would be no need of raiſing up C in the ſecond Caſe quite up to F, inſtead of E, whereas in Fact, it muſt always be raiſed beyond F, to allow for the Friction, that hinders it from producing a double Effect in falling from F. For if the Experiment be repeated an hundred times, the Weight, P when of two Pounds, will never be raiſed by letting C fall from any place between E and F.

EXPERIMENT III.

If (in order to avoid Friction) inſtead of a Blow ſtruck upon the end B, by the falling Body, the ſaid Body C be faſtned to a pretty long String tied to the button *m*, as at *c*, and firſt lifted up one Foot, and then let fall; ſo that in falling one Foot, it may pull down B, and lift up A with the Weight P of one Pound; whenever P is two Pounds, C muſt fall from a height greater than *f* or four Foot, otherwiſe it will not raiſe the *Brachium* A, eſpecially if it be let fall between *e* and *f*.

EXPERIMENT IV.

I took the Weight C of ſeventeen Ounces Troy, which was a round ball of Lead with a hole through the middle of it, and having paſſed the String N-X through it, before it was faſtened to the Hook X, I placed the whole Machine in ſuch manner, that the String being ſtretched by the Weight N, went through the hole of the Weight C, and likewiſe through the hole of the *Brachium* B, upon which *C* lay, without touching the ſides of the hole either in the Weight or Ballance; then having put ſuch a Weight P in the oppoſite Scale, as C falling from the height of one Inch, was able to raiſe high enough, to let looſe the Spring *g h* from the Button *i*: I added to P another Weight equal to it, and then letting fall C along the String that guided it, from an heighth of two Inches, then of three, and then exactly of four, it would not raiſe the double Weight P to the former height, but falling from five Inches, or a little higher, it raiſed it up.

EXPERIMENT V.

Leaving every thing as it was before, I changed the Weight C for another leaden Ball of twice the Weight, which falling from one Inch, raiſed the double Weight P to the uſual height; then changing the Weight *P* in any Proportion, whatever height was requir'd for the heavieſt Ball C (or C 2) to fall from, in order to raiſe the Weight at P; more than four times the height was required for the firſt Ball C, to raiſe the ſame Weight ſo high. as to let looſe the Spring.

EXPERIMENT VI.

I tried the Experiment with the Weight C hanging at the String *m c* (as in Experiment III.) and a Fall from an height of five, or near five Inches, was required to raiſe double the Weight in the oppoſite Scale, that a fall from one Inch would raiſe; only here the heighth above four Inches was not ſo great as in the former Experiment, the Friction being ſomething leſs. Then I ſuſpended the great Ball C (or C 2) by the String *m c*, and when by falling one Inch it raiſed the Weight P, the little Weight C could not produce the ſame Effect, without falling from a greater height than four Inches.

It is here to be obſerved, that which way ſoever theſe Experiments are tried, the Objections riſing from the Friction do no way ſerve to confirm the new Opinion, becauſe they ſhew that (upon account of the Friction) the Heights muſt be ſomething more than in a duplicate Proportion of the Velocities, but never leſs, to give a Blow with the ſame Body in Proportion to the Velocity.

That the *Momentum* of Bodies is in Proportion to the Maſs multiplied into the Velocity, is alſo moſt evidently ſhewn from the Congreſs of elaſtic Bodies, as has been demonſtrated by Sir *Iſaac Newton* in his *Principia*, in the Corollaries to his Laws of Motion. I had often tried the Experiments there mentioned with Balls of Ivory and Balls of Glaſs, and ſome of them with two Balls of Steel, of two Ounces each, and found every thing anſwer, allowing for the want of perfect Elaſticity in the Bodies. But now upon this Occaſion, as the Objections to the receiv'd Opinion were renewed, I was willing to repeat the Experiments with the utmoſt Accuracy; and therefore, as Ivory Balls are not equally denſe in all their Parts, and Glaſs Balls break after two or three ſtrokes; I cauſed Balls to be nicely turned of Steel, and made as hard (as the Workmen call it) that is, as elaſtick as poſſible, and the Weights of them were preciſely as follows: Two Balls of twelve Ounces Troy each, one of ſix Ounces, one of three, one of two, and one of Eight-penny-weight. Then making Pendulums of theſe Balls, and hanging them upon the Machine contrived by *Mariotte* for the Congreſs of Bodies, and lately improved by Dr. *Graveſande*[a], I meaſured 57¼ Inches between the Center of Suſpenſion and the Center of Gravity of the Balls, and then every Degree of the Circle they deſcribed in their Oſcillation was one Inch, and the Degrees being marked upon a line of Chords on a Braſs Ruler above the Balls, by their Strings ſucceſſively covering the croſs Lines of Diviſion, the Degrees that the Balls fell from, and thoſe to which they roſe, were very diſcernable to an Eye placed at a convenient Diſtance.

EXPERIMENT VII.

I took the two Balls 12, and removing each from the loweſt Point of their equal and reſpective Circles, up to 4 Inches, or 4 Degrees, I let them fall ſo that they met at bottom, and were both reflected again to 4, the Place from whence they fell.

EXPERIMENT VIII.

Every thing elſe being as before, inſtead of one of the Balls 12, I took the Ball 6, then letting 6 go from

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[a] Introd. No. 170. Vol. 1.

8 Degrees, and 12 from 4, after Reflection 12 was driven up again to 4, as before.

EXPERIMENT IX.

The Ball 3 falling from ſixteen Degrees met the Ball 12 that fell ſtill from 4, and after Reflection 12 went up again to 4.

EXPERIMENT X.

The Ball 2 falling from 6° and 12 from 1°, 12 was reflected to 1, and when 2 fell from 12 Degrees, and the Ball 12 from 2, the 12 was reflected to 2.

EXPERIMENT XI.

The Ball of eight Penny Weight (which weigh'd but $\frac{1}{30}$ of the Ball 12) falling from fifteen Inches or Degrees, rais'd up 12 (that fell from half a Degree) to the ſame Place again.

In all theſe Experiments the Error, or want of perfect Reflection, was greater in the little Balls than in the great ones, on account of their going thro' a greater *Arc* of a Circle, whereby they deviated more from a Cycloid than the great ones; as likewiſe on Account of the Reſiſtance of the Air, which muſt be greater becauſe of the little Balls going through a greater *Arc*, moving with more Velocity, being ſuſpended by a String as thick as that of the great ones, and having more Surface in Proportion to their Weight. But all the Errors do not bring the *Phænomena* any thing near what they ought to be, if the Force of the Bodies was as the Square of their Velocities multiply'd into their Maſſes, for then the Ball 12 would have been driven to Heights very different from what it roſe up to.

In

In the eighth Experiment, the Ball 12 ſhould have riſen to near five Inches and three quarters, for the Ball 6 falling with the Velocity, 8 muſt have had its Force $= 8 \times 8 \times 6 = 384$; and then, that the Ball 12 might have the ſame Force or Quantity of Motion, it muſt riſe near to 5, 7 becauſe $5{,}7 \times 5{,}7 \times 12 = 389{,}88$.

In the ninth, 12 ſhould have riſen to 8; for the Ball 3 muſt have had its Force $= 16 \times 16 \times 3 = 768$, and if 12 receiv'd its whole Force it muſt have riſen to 8 becauſe $8 \times 8 \times 12 = 768$.

In the ſecond Part of the tenth Experiment, 12 ſhould have riſen to near 5, becauſe $12 \times 12 \times 2 = 288$, and $5 \times 5 \times 12$ is but 300.

In the eleventh, the Ball 12 (thirty times heavier than the little one) muſt have gone to 2 ¼ Inches, becauſe the *Momentum* of the little Ball being $= 15 \times 15 \times 1 = 225$, that of the Ball 12 muſt be $= 2{,}75 \times 2{,}75 \times 12 = 226$ &c.

It may be here alledg'd, that one ought to ſubtract the *Momentum*, with which the great Ball comes upon the little one; but that won't mend the Matter much, tho' indeed the Difference will be leſs. For,

In the eighth Experiment, if we ſubtract $4 \times 4 \times 12 = 192$ from 389,88 there will remain 197,88, and the Ball 12 will go but to 4; but then in Experiment 9, if we ſubtract the ſame N° 192 from 768, we ſhall have 576, which would carry 12 to near ſeven Degrees, becauſe $7 \times 7 \times 12 = 588$.

In the tenth Experiment, there is only 48 to be ſubtracted; and in the eleventh only 15; and therefore the Velocity of 12 will very much fall ſhort of what is agreeable to the new Opinion.

After the Experiments made, and what has been ſaid, till theſe Conſequences are overthrown, no notice ought to be taken of any Objections, or new Experiments.

ments. But to give the Objectors all poſſible Satisfaction, I ſhall, in another Paper, endeavour to ſhew the Fallacies of the Arguments, and ſolve the *Phænomena* of the Experiments made; ſhewing, both by Reaſon and Experiment, that the Facts ought to be as they are, in conſequence of the receiv'd Opinion and Laws of Reſiſtance.

FINIS.

ERRATUM.

PAG. 250, in the Note for, de la Hire, &c. *read* Milnes, ibid.

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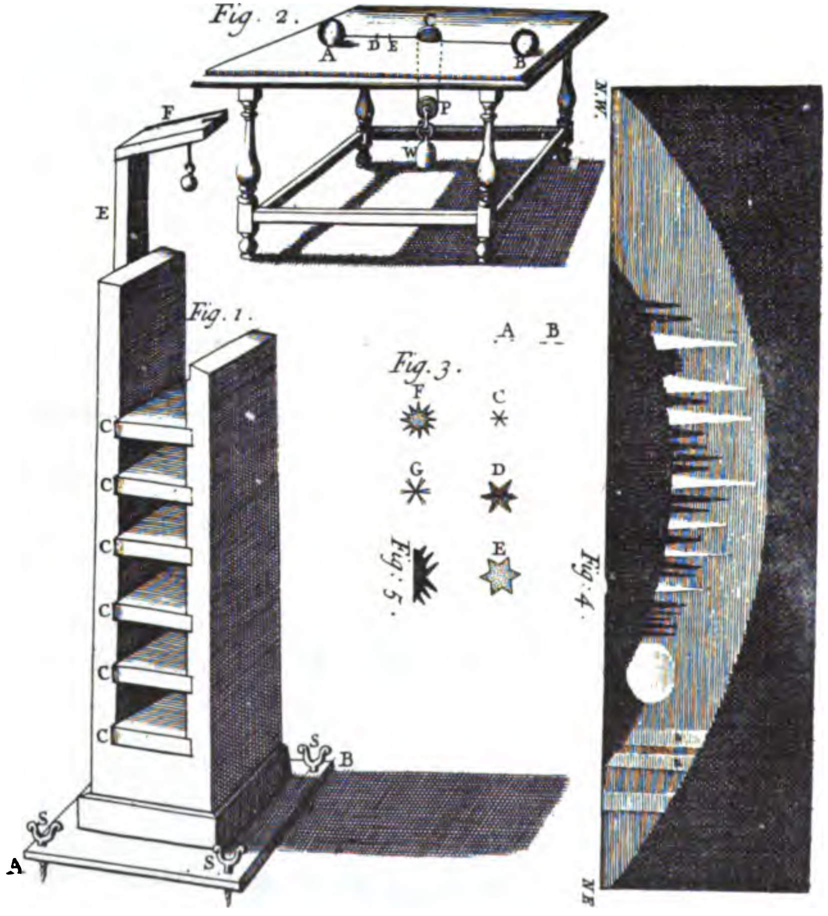

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PHILOSOPHICAL TRANSACTIONS.

For the Months of *March* and *April*. 1723.

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I. *A Catalogue of fifty Plants lately preſented to the* Royal Society, *by the Company of* Apothecaries *of* London; *purſuant to the Direction of Sir* Hans Sloane, *Bart. Preſident of the* College of Phyſicians, *and Vice-Preſident of the* Royal Society.

THE Company of *Apothecaries* of *London*, having, in the Year 1673, eſtabliſhed a Phyſick-Garden, which they have ſince furniſh'd with a great Variety of Plants, for the Improvement of their Members in the Knowledge of Botany; Sir *Hans Sloane*, Bart. in order to encourage and promote an Undertaking ſo ſerviceable to the Publick, has generouſly granted to the Company the Inheritance of the ſaid Garden, being part of his Eſtate and Manor of *Chelſea*, on condition that it be for ever kept up and maintain'd by the Company as a Phyſick-Garden; and as an Evidence of its being ſo maintain'd, he has directed and obliged the Company, in Conſideration of the ſaid Grant, to preſent yearly for ever to the *Royal Society*, at one of their weekly Meetings, fifty Specimens of Plants, that have grown in the ſaid Garden the preceding Year, which are all to be ſpecifically diſtinct from each other, until the Number of 2000 Plants be completed.

Accordingly the Company did lately preſent to the *Royal Society*, by the Hands of Mr. *Rand*, and Mr. *Meres*, the following fifty Specimens of Plants, for the laſt Year 1722. which Specimens, together with thoſe that are to follow them in ſubſequent Years, will, by order of the *Royal Society*, be carefully preſerv'd for the Satisfaction of ſuch curious Perſons, as may deſire to have recourſe to them.

The Catalogue, by Mr. *Iſaac Rand*, Apothecary, *F. R. S.*

I. *ANemone Virginiana, tertiæ Matthioli ſimilis, flore parvo,* Par. Bat. 18.

II. *Bellis Africana, capitulo aphyllo luteo, Coronopi folio, caulibus procumbentibus.* H. L. Bat.

III. *Bupleurum anguſtifolium.* Dod. 633.

IV. *Calamintha, magno flore,* C. B. 229.

V. *Carduus capite rotundo tormentoſo.* C. B. 382.

VI. *Chryſanthemum perenne, minus, Salicis folio glabro.* H. Ox. p. 3. 21.

VII. *Cirſium altiſſimum, albis maculis notatum, ſemine viſcoſo. Carduus albis maculis notatus, capitulis Cirſij.* Schol. Bot. 215.

VIII. *Clematitis cærulea erecta.* C. B. 300.

IX. *Clematitis, ſ. Flammula ſurrecta alba.* J. B. 2. 127.

Init. 255.

X. *Cnicus*

X. *Cnicus perennis cœruleus, Tingitanus.* H. L. Bat. 162.

IX. *Conyza Africana, Senecionis flore, retuſis foliis.* H. L. Bat.

XII. *Corona Solis altiſſima, alato caule.* T. Inſt. 490.

XIII. *Coronilla maritima glauco folio.* T. Inſt. 650.

XIV. *Cucubalus Plinij.* Lugd. 1429.

XV. *Daucus Fœniculi foliis tenuiſſimis.* C. B. 150.

XVI. *Daucus maritimus lucidus.* T. Inſt. 307.

XVII. *Dens Leonis, foliis Eryſimi vulgaris.* T. Cor. 35.

XVIII. *Geranium columbinum diſſectis foliis, pediculis florum longiſſimis.* Raij Syn. 218.

XIX. *Geranium columbinum perenne Pyrenaicum maximum.* T. Inſt. 268.

XX. *Geranium puſillum maritimum ſupinum, Althææ aut Betonicæ folio, noſtras.* Raij Syn. 216.

XXI. *Gnaphalodes Luſitanica.* T. Inſt. 439.

XXII. *Horminum Sylveſtre latifolium, verticillatum.* C. B. 238.

XXIII. *Hypericum perfoliatum & perforatum.* T. Inſt. 255.

XXIV. *Jacea altiſſima, folio diſſecto, capite Ovarii nudo.* D. Vaillant.

XXV. *Jacea Luſitanica, ſemper virens.* H. Reg. Par.

XXVI. *Jacobæa perennis Aſplenii folio.* D. Vaillant.

XXVII. *Lotus pentaphyllos minor hirſutus, ſiliquâ anguſtiſſimâ.* C. B. 332.

XXVIII. *Lunaria vaſculo ſublongo intorto.* Raij Syn. 164.

XXIX. *Lychnis linifolia toroſis & laxioribus capitulis.* Bar. Obſ. 64.

XXX. *Majorana Cretica, Origani foliis, villoſa, Satureiæ odore, corymbis majoribus albis.* T. Cor. 13. *Origanum Smyrnæum* Wheeleri.

XXXI. *Marrubium Hiſpanicum ſupinum, foliis ſericeis argenteis.* T: Inſt. 192.

XXXII. *Pentaphylloides paluſtre rubrum.* T. Inſt. 298.

XXXIII. *Pentaphylloides fruticoſa.* Raij Syn. 142.

XXXIV. *Phlomis fruticoſa, Salviæ folio latiore & rotundiore.* T. Inſt. 177.

XXXV. *Phlomis fruticoſa, Salviæ folio longiore & anguſtiore.* T. Inſt.

XXXVI. *Phlomis orientalis lutea, anguſtifolia cymis fulveſcentibus.* D. Sherard.

XXXVII. *Phlomis Samia herbacea, Lunariæ foliis.* T. Cor. 10.

XXXVIII. *Ptarmica orientalis pinnulis criſtatis.* T. Cor. *Abſinthium Santonicum Ægyptiacum.* C. B. 319.

XXXIX. *Quinquefolium album majus alterum.* C. B. 325.

XL. *Ruta Chalepenſis, latifolia, florum petalis villis ſcatentibus.* H. L. Bat. 431.

XLI. *Rubus odoratus.* Cornut. 150.

XLII. *Sclarea orientalis, Betonicæ folio acutiſſimo, comâ purpuraſcente.* T. Cor. 10.

XLIII. *Securidaca lutea major.* C. B. 348.

XLIV. *Thlaſpi Halimi folio, ſemper virens.* H. L. Bat. *Alyſſon.* T. 217.

XLV. *Trifolium peltatum Creticum.* C. B. Prod. 142.

XLVII. *Veronica multicaulis Pannonica.* T. Inſt. 145.

XLVIII. *Virga aurea floribus fiſtuloſis, Senecionis inſtar, foliis anguſtioribus non ſerratis.* H. Ox. p. 3. 125.

XLIX. *Virga aurea anguſtifolia, paniculâ ſpecioſâ, Canadenſis.* H. Reg. Par.

L. *Virga aurea altiſſima, ſerotina, paniculâ ſpecioſâ, patulâ.*

II. *Animadverſions upon ſome Experiments relating to the Force of moving Bodies; with two new Experiments on the ſame Subject. By the Reverend Dr.* Deſaguliers, *F. R. S.*

IN a former Paper * I demonſtrated, by Reaſon and Experiments, that the *Momentum*, or Force of moving Bodies, is always proportionable to their Maſs multiplied into their Velocity; as it is the Opinion of the greateſt Part of the Mathematicians and Philoſophers. Now I come to conſider the Experiments, that have led ſome ingenious Men into an Error, in relation to this Propoſition. *Polenus* in his Book † gives an Account of his Experiments relating to this Matter, in theſe Words: " I took a Veſſel, that had in it con-
" geal'd Tallow ſix Inches deep, and fix'd it to a
" level Floor, in ſuch manner, that the Surface of
" the Tallow, which was flat, ſhould every where be
" equally diſtant from the Floor. I had cauſed to be
" made two Balls of equal Bigneſs, the one of Lead,
" the other of Braſs, the laſt of which was a little
" hollow in the middle, that it might weigh but one
" Pound, whilſt the other weigh'd two. Suſpending
" theſe Balls from the Ceiling by Threads, in ſuch
" manner, that the lighter Ball hung over the Surface
" of the Tallow, from twice the Height that the hea-
" vier Ball did; I cut the Threads, and the Balls fal-
" ling perpendicularly upon the Tallow, by their

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* *Philoſoph. Tranſ.* No. 375. † De Caſtellis, pag. 56. No. 118.

" Fall made Pits in the Tallow, that were preciſely
" equal: the Ball of one Pound, from the Beginning
" of its Fall, till it came to reſt, going through a Space
" expreſs'd by the Number two, produc'd an Effect
" equal to that, which the two Pound Ball did pro-
" duce, in falling thro' a Space expreſs'd by the Num-
" ber one. It follows therefore, that we may look
" upon it as a ſettled Truth, That the active Forces
" (*vires vivas*) of falling Bodies are then equal, when
" their proper Weights are in a reciprocal *Ratio* of
" the Spaces, which the ſaid Bodies deſcribe by their
" Fall. And becauſe theſe Spaces are in the ſame *Ra-*
" *tio*, as the Squares of the Numbers expreſſing the Ve-
" locities; it appears by the Experiment, that the
" active Force (*vim vivam*) of the falling Body, is
" that which is made up of the Body itſelf, multiplied
" into the Space deſcribed in the Fall, or into the
" Square of the Number, that expreſſes the Velocity of
" the Body, at the end of the Motion. This Experi-
" ment I did not only make once, but ſeveral times,
" changing the Balls, the Diſtances, and the Body on
" which they fell; as for example, making uſe of Clay,
" or of ſoft Wax: and notwithſtanding theſe various
" ways of trying the Experiments, the Effects were
" conſtantly the ſame; which made me eaſily con-
" clude, that there was always the ſame Reaſon in Na-
" ture for this Phenomenon.

Thus far *Polenus*, whoſe Miſtake lies in this; that he eſtimates the Force of the Stroke of the falling Balls, by the Depth of the Impreſſion made in the Tallow, Clay, Wax, or any yielding Subſtance. But we muſt conſider, that when two Bodies move with equal Forces, but different Velocities, that, which moves the ſwifteſt, muſt make the deepeſt Impreſſion, whilſt the ſloweſt

Body communicates its Motion to the Clay round about, and therefore does not ſtrike in ſo deep as the ſwifter Body, which puts in Motion few Parts of the Clay, beſides thoſe that are before it, and which Parts have ſo much leſs Time to oppoſe this Body's Motion, as its Velocity is greater than the other's. To make this plainer, Let us ſuppoſe a Door half open, and moving very freely on its Hinges; if a Piſtol be fir'd againſt it, the Ball will go thro' the Door without moving it out of its Place; but if we take a large Weight of Lead, and throw it againſt the ſame Door, with the ſame Force as the Piſtol Bullet mov'd, the Door will be remov'd from its Poſition, and carried out of the Place on its Hinges by the Stroke; becauſe in the firſt Caſe, the Motion of the Ball is communicated but to a few Parts of the Door, and in the laſt it is diffus'd all over it. Nay, the Door will be mov'd by the Stroke, even tho' there ſhould be a prominent Part in the Lead, that ſhould be no bigger than a Piſtol-Bullet, in order to ſtrike the Door upon no more of its Surface, than the Bullet had done.

For illuſtrating this farther I contriv'd the following Experiment.

I caus'd a Machine to be made, as repreſented in *Pl.* I. *Fig.* 1. conſiſting of a Baſe of Wood AB, which could be ſet horizontal by means of three Screws, ſuch as SS: Upon this Board, or Baſe, there ſtood upright two parallel Boards, about four Inches wide, and four Inches aſunder, with the Elbow-piece EF ſliding behind one of them, ſo as to raiſe its upper End F to any Height deſired. Between theſe Boards, ſquare Frames of Wood GG *&c.* with Paper extended upon them, could ſlide in, to the Number of Six, in an horizontal

Poſition. Theſe Paper Diaphragms being thus plac'd, I ſuſpended an Ivory Ball of about one Inch and an half Diameter, weighing ſomething more than an Ounce and an half, by a ſhort Thread, under F, ſo that its Center of Gravity hung four foot over the firſt Diaphragm; then cutting the Thread, the Ball fell upon the Paper, and by its perpendicular Stroke broke thro' that Diaphragm, and the three next under it. Then putting ſo much Lead into the Ball abovemention'd, (which was made hollow for that Purpoſe) as to make it weigh twice as much as it did before; and bringing down F, to let it fall but from one foot, it broke thro' only two Diaphragms by its Fall. Making the Experiment ſeveral times with different Heights, but ſtill keeping the Proportion in Height of four to one, when the Balls were as one and two, the heavy and floweſt Ball broke thro' but half the Number of Papers. It happen'd indeed ſometimes, that there was ſome little difference, when the Papers were not equally ſtrong, or equally ſtretch'd, but the ſwifteſt Ball always broke through more Papers than the ſlow one.

Now tho' this Experiment does at firſt ſeem to confirm *Polenus*'s Theory; yet, when duly weigh'd, it proves no ſuch thing. For the lighter Ball does not break thro' more Papers, becauſe it has more Force, or a greater Quantity of Motion, but becauſe each Diaphragm has but half the time to reſiſt the Ball, that falls with a double Velocity, and therefore their Reſiſtance being as the time, as many more of them muſt be broken by the ſwift Ball, as by the ſlow one.

P. S. To all the Objectors, that allow the Force of moving Bodies, and their Quantity of Motion to be the ſame, what has been ſaid in this and my former Paper, ſeems to be a full Anſwer; but as there are now ſome Philoſophers, who diſtinguiſh that Force from the

Quantity of Motion, I am oblig'd to ſay ſomething more for the clearing up of that Point.

If I underſtand them right, they call *vis viva* a Force, whoſe Effect is ſenſible, as the Force of Gravity, when it accelerates Bodies in their Fall; and *vis mortua* a Force, which being deſtroy'd, produces no ſenſible Effect, as the Force of Gravity acting upon a Weight in one Scale of a Balance, when the Weight cannot deſcend by reaſon of a Counterpoiſe in the other Scale. But certainly no Man, that conſiders the thing attentively, would make that Diſtinction. However, ſince *Polenus* allows, that the Quantity of Motion in Bodies is as the Maſs multiplied into the Velocity (or MV); but ſays, that the Force, with which they act, which he diſtinguiſhes by the Name of *vis viva*, is as the Maſs multiplied into the Square of the Velocity, or MVV: I have made the following Experiment to ſhew his Notion to be inconſiſtent; tho all the *Phænomena* of unequal Weights applied to a *Statera*, ſo as to make an *Æquilibrium*, might ſerve for that Purpoſe, if it had not been objected, that the particular Conſtruction of the Machine hinder'd it from agreeing with the ſuppos'd Theorem, that the Force is as the Matter multiply'd into the Square of the Velocity.

EXPERIMENT.

Let two Balls, A and B, (*Pl.* I. *Fig.* 2.) be joyn'd by a String, which going thro' the ſmooth Hole C of an even Table, and under the Pulley P, ſuſpends a Weight W. It is plain, that upon letting go the Balls A and B, from the Places A and B, they will move towards C with the ſame Force, becauſe each of them will be drawn towards C by half the Force of the Weight W, whether the Balls be equal, or unequal.

1. The Balls being of two Ounces each (of Ivory), were, at the ſame inſtant of Time, let looſe from A and B, each diſtant twelve Inches from C, and both came to C at the ſame Time. Here the equal Forces will agree with the Product of the Maſſes into the Velocities, or into the Squares of the Velocities; becauſe A × 12 = B × 12, as well as A × 144 is equal to B × 144.

2. If A be taken of four Ounces Weight, and let go from D, or ſix Inches, whilſt B, ſtill equal to two, moves from 12 Inches; both Bodies will again meet at C: therefore here the equal Forces muſt be expreſs'd by the Maſſes into the Velocities, and not into their Squares; for tho' A × 6 be equal to B × 12 (4 × 6 = 2 × 12), A × 6 × 6, or 144 is but half of B × 12 × 12, or 288. Whereas if the Forces had been as *Polenus* affirms, B ſhould have been let looſe only from 8.4 Inches.

3. When A is ſix Ounces, it is let looſe only from E, or 4 Inches, to meet at C with B let looſe from 12; for then A × 4 = B × 12, whilſt A × 4 × 4, or 96, is three times leſs than B × 12 × 12, or 288. So that according to *Polenus*, B muſt have been let looſe from 7; but in that Caſe it comes ſooner to C than A.

N. B. The Weight W muſt be greater than the Weight of both Balls, leaſt the Friction of the Table ſhould ſpoil the Experiment.

III. *An Account of an Experiment, made to aſcertain the Proportion of the Expanſion of the Liquor in the Thermometer, with Regard to the Degrees of Heat. By* Brook Taylor, *LL.D. R.S.S.*

IT has, I believe, been generally ſuppoſed, tho' not proved, that the Expanſion of the Liquor in the Thermometer, is proportional to the Increaſe of Heat. To determine this Matter with Certainty, I made the following Experiment.

I provided a good Linſeed Oil Thermometer, which I marked with ſmall Diviſions, not equal in Length, but equal according to the Capacity of the Tube in the ſeveral Parts of it, as all Thermometers ought to be graduated. I likewiſe provided two Veſſels of thin Tin, of the ſame Shape, and equal in Capacity, containing each about a Gallon. Then (obſerving in every Trial, that the Veſſels were cold, before the Water was put in them, as alſo that the Veſſel I meaſured the hot Water with, was well heated with it) I ſucceſſively fill'd the Veſſels with one, two, three, *&c.* Parts of hot boiling Water, and the reſt cold; and at laſt with all the Water boiling hot; and in every Caſe I immerſed the Thermometer into the Water, and obſerved to what Mark it roſe, making each Trial in both Veſſels for the greater Accuracy. And having firſt obſerved where the Thermometer ſtood in cold Water, I found that its riſing from that Mark, or the Expanſion of the Oil, was accurately proportional to the Quantity of hot Water in the Mixture, that is, to the Degree of Heat.

IV. *An*

IV. *An Account of the Rattleſnake. By the Honourable* Paul Dudley, *Eſq;* F. R. S.

THE Rattleſnake is reckoned by the *Ab-origines*, to be the moſt terrible of all Snakes, and the Maſter of the Serpent-kind; that which cauſes their Terror, without doubt, is their mortal Venom, and the Enſign of it is their Rattle; and it is moſt certain, that both Men and Beaſts are more afraid of them, than of other Snakes; and while the common Snake avoids a Man, this will never turn out of the Way.

There are three Sorts, or Kinds, of this Snake, and diſtinguiſhed by their Colour, *viz.* a yellowiſh Green, a deep Aſh Colour, and a black Sattin.

The Eye of this Creature has ſomething ſo ſingular and terrible, that there is no looking ſtedfaſtly on him; one is apt, almoſt, to think they are poſſeſt by ſome Demon.

A Rattleſnake creeps with his Head cloſe to the Ground, and is very ſlow in moving, ſo that a Man may eaſily get out of his Way: This ought to be remarked as an Inſtance of the Goodneſs of God, who preſerves Man and Beaſt. His leaping and jumping to do Miſchief, is no more than extending, or uncoiling himſelf; for they don't remove their whole Body, as other Creatures do, when they leap; ſo that a Man is in no Danger of them, if his Diſtance be more than their Length; neither can they do any Harm when they are in their ordinary Motion, until they firſt coil and then extend, or uncoil themſelves, but they both are done in a Moment's Time.

When a Rattleſnake reſts, or ſleeps, he is coiled, and they are obſerved to be exceeding ſleepy.

Our People at firſt took the Noiſe this Creature makes, to be owing to ſome little Bones, or hard looſe Kernels lodged in their Tails; but ſoon diſcovered their Miſtake, and found the Tail to be compos'd of Joints, that lap over one another, ſomewhat like a Lobſter's Tail; and the ſtriking them one upon another, forms that Noiſe, which is ſo terrible to Man and Beaſt. The fierceſt Noiſe is obſerv'd to be in clear fair Weather, for when 'tis rainy, they make none at all; for which Reaſon, the *Indians* don't care to travel in the Woods, in a Time of Rain, for fear of being among theſe Snakes before they are aware. One other Circumſtance of their rattling has been obſerv'd, to wit, that if a ſingle Snake be ſurprized and rattles, and there happen to be others near him, they all take the Alarm, and rattle in like manner.

I dare not anſwer for the Truth of every Story I have heard, of their charming, or Power of Faſcination; but yet I am abundantly ſatisfied from many Witneſſes, both *Engliſh* and *Indian*, that a Rattleſnake will charm both Squirrels and Birds from a Tree into his Mouth. A Man of undoubted Probity ſometime ſince told me, that as he was in the Woods, he obſerv'd a Squirrel in great Diſtreſs, dancing from one Bough to another, and making a lamentable Noiſe, till at laſt he came down the Tree, and ran behind a Log: The Perſon going to ſee what was become of him, ſpied a great Snake, that had ſwallow'd him.

And I am the rather confirmed in this Relation, becauſe my own Brother, being in the Woods, opened one of theſe Snakes, and found two ſtrip'd Squirrels in

his

his Belly, and both of them Head foremoſt. When they charm, they make a hoarſe Noiſe with their Mouths, and a ſoft Rattle with their Tails, the Eye at the ſame time fixed on the Prey.

Their general Food conſiſts of Toads, Frogs, Crickets, Graſshoppers, and other Inſects, but principally of Ground Mice; and the Rattleſnake again ſerves for Food to Bears, and even our Hogs will eat them without Harm.

They are viviparous, and bring forth generally about twelve, and in the Month of *June*. A Friend of mine in the Country, being deſirous to diſcover the Nature and Manner of the Generation of the Rattleſnake, gave me the following Account, *viz.* About the middle of *May*, the Time when the Rattleſnakes firſt come abroad, he took and opened one of them, and in the *Matrix* found twelve ſmall Globes, as big as a common Marble, in Colour like the Yolk of an Egg; in three or four Days more, he took and opened another, and then plainly perceived a white Speck in the Centre of the yellow Globe; in three or four Days more, he diſſected a third, and diſcovered the Head of a Snake; and in a few Days after that, three Quarters of a Snake was formed, and lying round in a Coil. In the latter End of *June*, he kill'd an old one, and took out perfect live Snakes of ſix Inches long. In *September*, when the old ones take their Young in, and carry them to their Dens, they are not quite a Foot long. They couple in *Auguſt*, and are then moſt dangerous.

I cannot ſay, what other Serpents, or poyſonous Creatures, may do, but I am ſatisfied the Rattleſnake does not traject his Poyſon; and that unleſs the Skin be firſt broke, or an Inciſion made with his Teeth, his Venom can do no Harm; for my Friend aſſured me, that he had made an Experiment of it in this manner: He took the Breech of his Gun, and ſet it upon four or five of them, and after they had bit it, and left ſeveral Drops of their Poyſon, he with his Hand wiped it off without any Harm.

Our People have ſeveral Remedies for the Sting of a Rattleſnake; among others, that which is much made uſe of, is a Root they call Blood-root, I ſuppoſe ſo named, from the Colour of the Root, and the Juice, which is red like Blood. It grows in great Abundance in our Woods; they bruiſe the Root, and bind it above the Place that is bit, to prevent the Poiſon's going farther, at the ſame Time ſcarifying the Place affected; ſome of the Root is alſo boiled, and the Perſon poiſoned drinks the Water.

They are generally from three to five Feet long, and do not commonly exceed twenty Rattles; and yet I have it atteſted by a Man of Credit, that he killed a Rattleſnake, ſome Years ſince, that had between ſeventy and eighty Rattles, with a ſprinkling of grey Hairs, like Briſtles, over his Body; he was full five Foot and an half long, and as big as the Calf of a Man's Leg.

They ſhed, or throw off their Skins every Year, ſometime in the Month of *June*, and turn it inſide out when they throw it off. It has alſo been obſerved, that the Skin covers not only the Body, but the Head and Eyes.

They generally den among the Rocks in great Numbers together; the Time of their retiring is about the middle of *September*, and they don't come abroad till the middle of *May*, when our Hunters watch them, as they come out a ſunning, and kill them by hundreds.

Roxbury, *New-England*,
Octob. 25. 1722.

Paul Dudley.

V. *Some*

V. *Some Obſervations upon Vipers; on occaſion of the foregoing Relation. By* C. J. Sprengell, *M. D. F. R. S.*

AT *Milan* I found a Viper-Catcher, who ſeldom was without 60, or more, Vipers alive, kept together in a back Room open at the Top; he had them from all Parts of *Italy*, and ſold them dead or alive, according to the Uſes they were deſigned for. He having got one day a female Viper big with young, gave me notice to ſee her manage her Prey; whereupon we catch'd ſome Mice, and throwing them in one at a Time, amongſt all that Number of Vipers, (which were rather above 60) there was none of them, that in the leaſt concern'd himſelf about the Mouſe, till the laſt mention'd pregnant female Viper and the Mouſe interchanged Eyes; whereupon the Mouſe ſtartled, but the Viper rais'd her Head, and turn'd her Neck into a perfect Bow, the Mouth open, the Tongue playing, the Eyes all on Fire, and the Tail erect. The Mouſe ſeem'd ſoon recover'd of his Fright, would take a Turn or two, and ſometimes more, pretty briſkly, round the Viper, and giving now and then a Squeak, would run with a great deal of Swiftneſs into the Chops of the Viper, where it gradually ſunk down the Gullet. All this while the Viper never ſtirr'd out of its Place, but lay in a Ring.

It is to be obſerv'd, that no Viper will feed, when confin'd, except a female Viper impregnated.

The ſame I ſaw at *Bruſſels*, where a Soldier had catch'd a large Viper big with young. The Houſe, where I and ſome of my Companions lodged, was near the Fiſh-Market, where my Landlord had a Sow, and five ſmall Pigs of nine or ten Days old. We got one of the Pigs, which we caus'd to be bit by the Viper in the Tail, and in four Minutes time chopp'd off the Tail, the Pig appearing to be ſick and dizzy, and the remaining part of the Tail being ſwell'd; but I believe the bleeding ſav'd it, for the next Morning it was well again. The ſame happen'd to another Pig, which we had got bit in the fore Foot, and ſtaying ſeven Minutes after the Bite, cut off his Leg about two Inches above the Bite. After theſe two, we took the other three, and had them bit in ſeveral Places, whereof two died that Night, and the third recover'd, we having given it, about five or ſix Minutes afterwards, ten Grains of Emetic Tartar.

This I try'd afterwards upon Dogs bit by Vipers, and I found that they all recover'd upon the Emetic Tartar.

VI. *Obſervations on the Figures of Snow. By the Reverend* Benj. Langwith, *D. D. Rector of* Petworth *in* Suſſex. *In a Letter to Dr.* Jurin, *R. S. Secr.*

ON the 30th of *January* laſt, ſomething paſt Nine in the Morning, Weather cold, Wind South-weſterly, but not very high, Barometer above thirty Inches, I ſaw that pretty Phenomenon of the Star-like Snow, and tho', upon comparing my Obſervations afterwards with thoſe of *Deſcartes*, Dr. *Grew*, and Mr. *Morton*, I find I have but little to add upon the Subject; yet, as I obſerv'd the Progreſs of Nature in this ſort of Cryſtallization, with a great deal of Pleaſure, I hope it will not be diſagreeable to you to receive an Account of it.

I ſhall begin with the moſt ſimple Figures A and B, (*Pl.* I. *Fig.* 3.) of which the former is a roundiſh Pellet of Ice; the ſecond, a ſmall oblong Body, with parallel Sides, which is often as fine as a Hair. Of this latter kind the Flakes of Snow chiefly conſiſt; and tho' they look white to the Eye, yet when view'd with a ſmall Magnifier of a Microſcope, they appear like ſo many tranſparent Needles of Ice thrown together, without any Manner of Order.

The next Figure is C, in which the Pellet has ſhot out ſix of thoſe ſmall Bodies of equal Length, and ſet at equal Angles: Of this kind I ſaw a conſiderable Number.

The next Step in the Cryſtallization is D, in which thoſe Bodies are lengthen'd, and have ſhot out a great many more from their Sides, at equal Angles, but unequal Lengths, as growing continually ſhorter and ſhorter, till they terminate in a Point: I meaſur'd ſome of theſe, and found them to be about one quarter of an Inch in Breadth. I ſaw but very few of them in Perfection, for the collateral Shoots were ſo exquiſitely fine, as to be liable to be broken in their Fall, or confounded together by the leaſt Degree of Heat.

Of the next kind, E, I ſaw a very great number, which being examin'd by the Microſcope, plainly appear'd to be nothing but the former in Diſorder. The Edges of theſe were in general very irregular, but ſome of them happened to be ſo indented, as to look like the jagged Leaves of Plants.

The next Kind, F, had twelve points regularly diſpoſed, and probably might conſiſt of two of the former ſo join'd together, as to cut their Angles equally.

Perhaps alſo thoſe Mr. *Morton* deſcribes, as conſiſting of *Radii*, which, inſtead of terminating in a Point, grow bigger, as they advance from the Centre, might be formed from two of the Kind, C, ſo join'd at the Centre, as to cut each other's Angles unequally; for in the Progreſs of the Cryſtallization, theſe *Radii* would quickly unite.

Laſtly, that Sort, which *Deſcartes* compares to Roſes, and of which he has given a Figure in his Treatiſe of Meteors, may be nothing but the Kind E, when the Points are rounded off, by being gently thaw'd.

I propoſe theſe things only by way of Conjecture; becauſe, as the ſmall Drops of Water may be impregnated with very different Particles in the Air, it is not eaſy to determine, whether theſe Figures may not be the

Reſult of a Cryſtallization quite different from the former.

I had almoſt forgot to tell you, that I ſaw but very few Figures of twelve Points, and thoſe moſtly imperfect in one reſpect or other.

Petworth, Feb. 13.
172$\frac{2}{3}$.

VII. *Obſervationes Auroræ Borealis per quadriennium factæ,* Lennæ Regis *in* Norfolciâ. *Ex epiſtolâ doctiſſimi Obſervatoris ad* Martinum Folkes, *Arm. Reg. Soc. Vice-Præſ.*

NON ingratum fore arbitror, Vir Doctiſſime, obſervationes quaſdam de Aurorâ Boreali, apud Lennam Regis in Norfolciâ, quatuor ſuperioribus annis a me habitas tibi impertire. Mirum illud Phænomenon ſexto Martii 171$\frac{5}{6}$. non licuit mihi inſcienti videre, quod infortunium haud exiguum in me peperit ſtudium ſubſequentibus Phænomenis invigilandi. Priorem obſervationem, quam hic ſubjeci, ſummâ curâ delineavi, ſtatim atque vidi, ideoque, ni fallor, a vero parum diſcrepat. Reliquas etiam deſcriptiones pro certo habeas accuratas eſſe. Quænam ſit cauſa harum coruſcationum, nondum, ut opinor, ſatis exploratum eſt.

Die Veneris Sept. 5. 1718. circa hor. x. Phænomenon hoc, delineatum *Pl.* I. *Fig.* 4. obſervatum fuit Lennæ Regis, in puncto boreali.

Die Saturni Sept. 6. circa horas VIII & X. perplures Luminis columnæ, ſimiles ſupradeſcriptis (*aa*) obſervabantur, non æque lucidæ ac pyramides nocte præcedente obſervatæ, quæ ferebantur verſus orientem, hæ vero ad occidentem.

Die Jovis Sept. 11. perplures iſtiuſmodi columnæ iterum conſpiciuntur cum motu occidentali.

Die Saturni Sept. 13. circa horam XI. Aurora Borealis lucidior ac altior fuit, longioribuſque pyramidibus adornata, quam adhuc obſervata fuerat. Adeo lucebat hâc nocte, ut characteres in libro legerentur.

Die Saturni vero Octob. 11. circa horas X & XI. Aurora Borealis lucidior quam unquam fuit, ac coruſcationibus pyramidalibus interſperſis, haud multum abſimilis Phænomeno prius obſervato, Sept. 5. Lumine iterum ita ſplendente, ut legere liceret.

Die Veneris Dec. 19. circa horas VIII & IX. Hâc nocte viſi ſunt cum Aurorâ Boreali perplures lucis radii, e nube veluti nigrâ exorientes; revera autem non fuiſſe nubem exinde patet, quia ſtellæ per illam clare videbantur. Quid vero aliud fuerit, haud facile eſt dictu. Sed quod præcipue notavimus, fuit motus horum luminum ſane mirabilis. Hos radios obſervavimus in ſtatu ſemper mobili, poſitiones ſuas, ſeu loca, perpetuo mutantes; curſum ſuum modo hâc, modo illâc, rurſum, prorſum dirigebant, & interdum alii ex aliis eâdem viâ ſingulatim progreſſi ſunt, & aliquandiu ſibi invicem colliſione mutuâ impingebant cum tremulo ac vibrante motu, & celeritate fere incredibili. Radii quidam obſervati ſunt uſque ad Zenith ſe porrigentes. Viſa eſt ſæpe lux ſe in acervum collegiſſe, atque ita mirum exhibuiſſe fulgorem coloribus Iridis tinctum, & iterum viſa eſt ſe dilatâſſe. Luna hâc nocte lucide ſplendebat.

Die Jovis Martii 12. 17$\frac{18}{19}$. circa horas X & XI. Aurora Borealis rurſus obſervabatur.

Die Veneris Martii 27. iterum viſa eſt Aurora Borealis cum radiationibus variis obliquis, ſcundùm hanc Figuram. *Pl.* I. *Fig.* 5.

Die Lunæ Octob. 26. 1719. hâc nocte etiam viſa eſt circa horas VII & VIII.

Item die Lunæ Nov. 9. 1719.

Die Domin. Jan. ult. $17\frac{19}{20}$. Hâc nocte, ab horâ ſeptimâ uſque ad decimam, Aurora Borealis viſa eſt altior, quam unquam antehac obſervata fuit, per dimidium cœli ab oriente uſque ad occidentem, fere obducta coruſcationibus variis interſperſis, adeoque lucebat, ut characteres in libro perquam diſtincte videri poſſent.

Die Saturni Sept. 17. 1720.

Die Lunæ Jan. 6. $17\frac{20}{21}$. circa horas VII & VIII. viſa eſt iterum Aurora Borealis cum coruſcationibus pyramidalibus, undique a Zenith, veluti centro, obductis, imaginem exhibentibus pæne inſtar Umbellæ.

Die Domin. Jan. 12. $172\frac{0}{1}$.

Die Lunæ Sept. 11. 1721. circa hor. IX.

Die Mercurii Sept. 5. 1722. ab horâ decimâ ad ſeſquidecimam.

Die Mercurii Octob. 3. circa hor. IX.

Die Jovis Octob. 4. circa hor. X.

Die Domin. Dec. 23. 1722. circa hor. VIII.

VIII. *An*

VIII. *An Account of a Catadioptrick Teleſcope, made by* John Hadley, *Eſq; F. R. S. With the Deſcription of a Machine contriv'd by him for the applying it to uſe.*

THE Inſtrument conſiſts of a metalline *Speculum*, about ſix Inches in Diameter. The *Radius* of the Sphere, on which its concave Surface was ground, is ten Feet, five Inches and one quarter, and conſequently its focal Length is 62 $\frac{5}{8}$ Inches. The Back has a hollow Screw made at its Centre, to receive the End of a Handle, which is ſcrew'd on, whenever the Metal is to be moved, in order to avoid ſullying its poliſh'd Surface by handling.

This Object-Metal A, *Pl.* II. *Fig.* 1. is placed in one End of an octangular Tube, B B, about ſix Feet long, and ſomething wider than what is ſufficient to receive the Metal, dyed black on the Inſide. About ſix or ſeven Inches in Length of the three uppermoſt Sides of the Tube C, (toward that End, at which the Metal is plac'd) are ſeparated from the reſt, and open with two Hinges, to make room for the Metal to be put in and taken out. The End of the Tube is cloſed by an octangular Piece of Board D, which has an opening *d*, about $\frac{2}{3}$ of an Inch broad, from the Top down to a little below the Centre, to give room for the beforemention'd Handle, when the Object-Metal is lifted into or out of the Tube; at other times it is cloſed with a ſliding Shutter. The

Metal is placed ſo, as to have its *Axis* coincide with that of the Tube, by the means of three ſmall Buttons fix'd to the Inſide of the Tube, having their hinder Ends all in the ſame Plane, to which this *Axis* is perpendicular. Two of theſe appear at *a a*, the third being at the middle of the Bottom of the Tube, is not ſeen. The foreſide of the Metal reſts againſt theſe Buttons in three Points of its Circumference, nearly equidiſtant from each other, and is held to them by three Screws, (one of which appears at *b*) which run through the octangular Board at the End of the Tube, and bear againſt the Back of the Metal, (in three Points, which directly anſwer thoſe three on the foreſide) with juſt ſo much Force, as is requiſite to keep it ſteady in its Place. They muſt not be ſcrew'd harder againſt the Metal for Fear of bending it, which (tho' it is half an Inch in Thickneſs) a very little Force is ſufficient to do. When the Inſtrument is not uſed, theſe Screws are looſen'd; and the Object-Metal is taken out and laid by, to prevent its tarniſhing.

The oval Plane is compos'd of a Plate of the ſame Metal with the great *Speculum*, about $\frac{1}{15}$ or $\frac{1}{16}$ of an Inch in Thickneſs, ſolder'd on the Back to another of Braſs. Its Breadth is ſomething leſs than half an Inch, and is in Proportion to its Length as 1 to $\sqrt{2}$. At one End of the Oval, the Braſs Plate projects a little beyond the other, and has a Screw cut through it in that Part, as likewiſe another directly againſt the Centre of the foreſide. The other End is cypher'd away on the Backſide, that it may intercept as few of the Rays, in their Paſſage towards the Object-Metal, as is poſſible. The two Screw-holes in the Back ſerve to fix this Oval A, *Fig.* 2. to a Braſs Arm, B, which is faſtened at the other End into a Slider E E. *Fig.* 1. and 2. This

Slider is of an equal Thickneſs with the Side of the Tube, and has a Groove, G G, *Fig.* 1. cut for it in that Side, parallel to the *Axis*, and long enough to give room for its Motion, to ſet the two *Specula* at the different Diſtances, which the ſeveral Eye-Glaſſes require. It reſts on the Inſide againſt two thin Ledges, faſtened within the Tube along the Sides of the Groove. On the Outſide it is kept in its Place by a ſliding Shutter, not expreſſed in the Figure. In the Middle it has a Cylindrick Cavity, D, *Fig.* 2. whoſe *Axis* is exactly perpendicular to its inner and outer Surfaces. Each of the Boxes, in which the Eye-Glaſſes are contained, is fitted to this Cavity. The beformention'd Braſs Arm is fix'd into the Inſide of this Slider, towards the End fartheſt from the Object-Metal; it riſes perpendicular for about two Inches, and is made flat, ſo as to turn one Edge to the Rays, which come from the Object. About *b*, it is bent forwards and flatted the other Way, ſo that when the Back of the oval Plane is held flat to it, by the two Screws *c c*, the Axis of the Cylindrick Cavity may fall on the Centre of its foreſide, inclin'd to its Surface in an Angle of ſomething leſs than fourty five Degrees. This Angle is brought to be exact by two very ſmall Screws, *i i*, whoſe Threads take hold in the flatted End of the Braſs Arm, and their Points bearing againſt the Back of the Oval, raiſe one End of it a little from the flat of the Arm. The *Specula* are ſet at their due Diſtance, by turning of a long Screw, C C, for which there is a Nut lodged in the Slider at *g*; the Screw is kept from moving backward or forward, when 'tis turned, by a Braſs Plate, F, which is to be fix'd to the flat End of the Side of the Tube, and taken off at Pleaſure. Each of the Eye-Glaſs Boxes, H, has a Screw on the outward End, to faſten to it a Bowl, or

Diſh, I, to receive the Ball of the Eye, and guard it from external Light.

On the Top of the Tube is fix'd, on two ſmall Pedeſtals, a common Dioptrick Teleſcope, H, *Fig.* 1. about eighteeen Inches long, its *Axis* parallel to that of the Tube; and having two Hairs plac'd in the common *Focus* of its Object and Eye-Glaſſes, croſſing one another in its *Axis*.

There are three convex Eye-Glaſſes belonging to the Inſtrument. The firſt, or ſhalloweſt, has its focal Diſtance of about $\frac{1}{3}$ of an Inch; the ſecond, of $\frac{3}{10}$; and the deepeſt, of $\frac{11}{40}$, or ſomething leſs. When the firſt of theſe is uſed with the Inſtrument, it magnifies about 188, or 190 times, in Diameter; with the ſecond, about 208; and with the third, 228 or 230. Each of theſe Glaſſes has placed, in that *Focus* neareſt the Oval, a Circle to determine the Part of the Object ſeen at one View; and in the other *Focus* toward the Eye, a Braſs Plate with a little Hole in the middle, to let no Light paſs to the Eye from the Inſide of the Tube, but what comes from the Oval. Beſides theſe three convex, there are two concave Eye-Glaſſes, with which it magnifies about 200 and 220 times; and alſo a Sett of three Convex, which turn it into a Day Teleſcope, magnifying about 125 times. The Aperture is limited by a Circle of Card, or Paſtboard, placed before the Object-Metal in the Tube. To vary the Aperture there are three of theſe Circles, and the Apertures allowed by them are five Inches and an half, five Inches, and four and an half, tho' for ſome Objects the whole Metal may be left open.

The Engine made uſe of to direct the Tube to any Object, conſiſts of a ſtrong Plank, FF, *Fig.* 1. and 3. about fourteen Inches wide, and two Feet and an half, or three Feet long, which ſerves as a Foundation for the whole. Near one End of this Plank is placed an upright fourſided Box, III, *Fig.* 1. and 3. about two Feet high, narrower at the Back next the End of the Plank than before: Its two Sides are mortiſed both into the Plank below, *a a*, *Fig.* 3. and into the Top of the Box above, *d d*; the back and fore Part are faſten'd to the Edges of the Sides with Wood-Screws. The Top has a circular Hole cut in it, ſomething above three Inches in Diameter, whoſe Centre is about three Inches diſtant from the outſide of the Back, and at an equal Diſtance from the two Sides. This Hole gives Paſſage to a turning Pillar B, in the Bottom of which there is fix'd an Iron Pivot *c*, to turn in a thick Braſs Plate lodged in the Plank, *b*. The upper End of the Pillar riſes about an Inch and an half above the Top of the Box, and is mortiſed into a ſtrong Head, K, *Fig.* 1. and 3. about eight Inches in Length, and four or five in Breadth and Thickneſs. This Head carries two Cheeks, L L, about thirteen or fourteen Inches in Height, their hinder Edges, towards the lower End, extending five Inches beyond the *Axis* of the Pillar backward. Along the Back of theſe Cheeks, at equal Diſtances above one another, there are Notches, tending obliquely downwards, and anſwering one another in each Cheek, to receive the Pivots of a crooked Iron *Axis*, C, *Fig.* 3. on which the Tube is plac'd. The Notches are made at different Heights, to keep the Eye-Glaſs at a proper Height for the Eye, in different Elevations of the Object above the Horizon. The Figure of the *Axis* anſwers that of the three under Sides of the Tube. The *Axis* of the Tube lies about two Inches and an half higher than the *Axis* of the Motion upon theſe Pivots, and the Centre of Gravity, when the Object-Metal is in, is about three Inches backwarder. To keep the Tube from ſlipping back, when its fore End is raiſed, it has two Buttons fixed to it, which reſt againſt the fore Part of the *Axis*.

To keep the Pillar from touching any of the Sides of the round Hole, in which it turns, a Cylindrick Sector, containing about 65° or 70°, and about an Inch in Height, is cut out on the back Part of the Pillar, near the upper End D. In the ſquare Angle of this Cavity is fix'd a thin Steel Plate *oo*, bent croſs the middle to the ſame Angle. The internal angular Edge, between the two Parts of this Plate, lies in the *Axis* of the Pillar, and turns upon the harden'd Edge of a Wedgelike Iron, *f*, whoſe Baſe, or Board Part, is faſten'd with two ſtrong Screws on the Top of the Box, directly behind the round Hole beforemention'd.

The upper Parts of the Cheeks are ſtrengthened by two Brackets, GG, leaving Room between them for the Bottom of the Tube to touch the upper Edge of the fore Part of the Head. The hinder Part of the Head is alſo hollow'd, in the Manner repreſented in the third Figure.

The Head on its fore Part carries a flat Arm, M, *Fig.* 1. about twenty ſeven Inches long, a little taper towards the farther End, where it is four Inches broad. This is ſtrengthened by a narrow Slip, glew'd edgewiſe along the middle underneath, O, and alſo by a Brace or Stay, N, reaching from the turning Pillar to within nine Inches of the End of the Arm. The Stay paſſes through a tranſverſe opening cut in the fore Part of the Box, P, which is long enough to allow room for a ſufficient Motion of the Pillar round its *Axis*.

On the other End of the Bottom Plank, tranſverſely to its Length, is erected a Board about twelve Inches wide, and twenty ſix or twenty ſeven high, Q, the Top of it reaching within an Inch and an half of the under Side of the Arm. This Board is held firm in its

Poſition by a Spur, R, part of its upper End on the outſide is pared off toward the Edges, to form it into the Segment of a Cylinder, whoſe *Axis* coincides with that of the Pillar. Its Uſe is to ſupport a Reſt, SS, on which the End of the flat Arm moves backward and forward. This Reſt being apply'd tranſverſely to the outer Part of the upright Board, where it is made Cylindrick, is bent into the ſame Figure, by the means of four Screw-Pins, two of which paſſing through each End of this, and of another Piece of the ſame Length, T, (but ſomething narrower) placed over againſt it on the inſide of the Board, by their Nuts, draw them together, ſo as to graſp the End of the upright Board between them ; the upper Edge of the Reſt being firſt ſhot with a Plane very ſtrait and ſmooth. To render the Motion of the Arm along the Reſt ſmooth and eaſy, it has two Rollers lodged in a Box fix'd near to the End, on its underſide, V, to roll upon the Edge of the Reſt, when the End of the Arm is moved along it. One of the Rollers is placed near each Edge of the Arm, and their *Axes* lye in Lines paſſing through the *Axis* of the turning Pillar. The Reſt is kept up to them, with a proper Degree of Force, by two Screws, W W, which run in two Plugs, XX, faſtened on the Sides of the upright Board, and bear againſt the under Sides of two Pieces fix'd on the Inſide of the Reſt.

The Motion of the Tube is governed by two Braſs Pegs, Y and Z. The firſt of theſe, Y, is plac'd about 10 or 11 Inches from the End of the Arm, and has a Line wound round it, which paſſing under a ſmall Pulley, *f*, fix'd in a vertical Poſition near the End of the Arm, is faſtened to a Staple on the under ſide of the Tube *g*. This Line, by the turning of the Peg, brings the fore End of the Tube to its due Elevation, being acted againſt by the Exceſs of Weight in the hinder End of the Tube, when the Metal is in it, which is equivalent to about two Pound at *g*, where the Line is faſtened. In great Elevations of the Object above the Horizon, the Line is not carried ſo far as the Point *g*; but is faſten'd a little above the Pulley, to a light ſquare Stick, *b*, having at one End a Hook, by which it takes hold of the Staple *g*. This is done, that the Springyneſs of the Line may not continue a vibrating Motion in the Tube, (when any thing happens to ſhake the Inſtrument) and make the Object appear to tremble. The lower Part of the Stick reſts againſt the end of the Arm, and by its ſlight Friction contributes to the ſame Effect.

The other Peg, *z*, is ſo plac'd, that it may be conveniently reached by one Hand of the Obſerver, while the other is employed about the Peg Y: It regulates the Horizontal Motion of the Tube, by means of a Line, which being wound about the Peg at one End, paſſes by another ſmall Pulley placed cloſe by the Side of the aforementioned one in an Horizontal Poſture (not to be ſeen in the Figure) and is hung on a Pin driven into the little Head K. It is acted againſt by two Springs, *m* and *n* *Fig*. 3. placed in the Box, I I I, one on each Side of the turning Pillar; that on the right Hand, *m*, draws the right Side of the Pillar forward, by a very ſtrong Line, which being faſtened to the Head of the Spring, paſſes round the back Part of the Pillar to a Pin, at P, by which it is ſtrain'd to its due Strength. The Spring on the Left Hand *n*, draws the Left Side of the Pillar backwards in the ſame manner. Theſe Pins are plac'd on the Pillar a little higher than the Tops of the Springs, that being drawn a little downwards, as well as turn'd round its *Axis*, the Pivot in its Bottom may not be raiſed out of the Hole in the Braſs Plate, when the Reſt bears hard againſt the Rollers at the End of the Arm. Each of theſe Springs draws with a Force equal to about

18 or 20 Pounds Weight, when the End of the Arm is carried cloſe to the ſmall Head *k*, Fig. 1. and conſequently (the Semidiameter of the Pillar being an Inch and Half, and the Diſtance of that Head from the *Axis* about 28 or 29 Inches) the End of the Arm will be carried by the united Forces of both the Springs, towards the other End of the Reſt, with a Force equivalent to the Weight of about two Pounds. Each of the Pegs, Y and Z, turns in a Hole made in a Piece of Wood *l*, faſtned to the under Side of the Arm; and the Pieces being ſlit with a Saw from one End through the Hole, and about half an Inch beyond it, the ſeparated Parts are drawn together by a Skrew *m*, till the End of the Peg is griped between them, with a due Degree of Force. By theſe Pegs, with the help of the Teleſcope H, the Tube is eaſily directed to any Object, and made to accompany a Celeſtial one in its Diurnal Motion, while the End of the Arm moves the whole Length of the reſt.

If it be deſired, that when the Object is found, the turning of one Peg ſhould carry the Tube along with the Motion of the Heavens, ſo as to keep the Object always in ſight; this may eaſily be effected in various Manners.

The concave Surface of the Object-Metal has many little Spots in it, which could not be brought to take a Poliſh. In one, or two Places, the Metal itſelf ſeems to have ſome ſmall Parts, ſomething harder or ſofter than the reſt, occaſioning an irregularity in the Figure of the Metal about them. But theſe Parts being ſmall, in Proportion to the whole, do not ſeem conſiderably to affect the Diſtinctneſs of the Appearance.

The open Air has commonly an undulating Motion in its Parts, eſpecially in the day time, which occaſions the Rays of Light to deflect a little from the ſtrait Lines, in which they ought to move, in order to render the

Species perfectly diſtinct. The Effect of this, though inſenſible to the naked Eye, or even through a ſmall Teleſcope, becomes conſiderable, when the Object is very much magnified. The Inſtrument, when try'd at an Object encloſed, ſo as to ſecure it from this Inconvenience, ſeems to bear an Aperture of five Inches and an half, with the deepeſt of the forementioned Eye-Glaſſes, as well as the common Teleſcopes do the uſual Charge and Aperture given to them, except that in theſe the Objects appear a little brighter.

Fig. 1. Repreſents the Inſtrument placed on the Machine, in order to be apply'd to Uſe.
Fig. 2. Repreſents the Inſide of the Slider, with the reſt of the *Apparatus* belonging to the oval Plane and Eye-Glaſs.
Fig. 3. Repreſents the hinder Part of the Machine, the Back, and one ſide of the Box, being taken away, to ſhew the turning Pillar and Springs on the Inſide.
Fig. 4. Repreſents *Saturn*, as it appear'd in *June*, 1720, by this Teleſcope.

IX. *Anatomia ſenis decrepiti, annorum* 109. *facta* Tiguri, *die* 2. Feb. 1723. *A* Joh. Jacobo Scheuchzero, *Tigurino, Med. Doct. Matheſeos Prof. & Regiæ Societatis Socio.*

SUOS habent characteres varii ætatis gradus. A fluiditate aſcendit homo ſenſim per ſoliditatis varias mutationes, donec cum rigiditate & fibrarum veluti immobilitate amittat vitam. Nec contemnendi eſt usûs hæc in medicinâ conſideratio.

Accidit ante menſes aliquot, ut ob decrepitam ſuam ætatem & raram, in Noſocomium fuerit receptus *Johannes Leonhardus Vopper*, Rhœtus, Sharanienſis ex cœtu Glantino Fœderis ſuperioris. Is juxta tenorem teſtimoniorum lucem primum vidit, 1. *Maij* 1614. A. 1634. Metalli foſſor in venâ ſepultus, per horas 33. ſub terrâ jacuit: Mortuo, quam vivo, ſimilior inde protractus, propter Abdominis validam, quam paſſus eſt, preſſionem, inde ab illo tempore incontinentiâ urinæ laboraverat. A. 1637. iter fecerat per *Hungariam*, *Turciam*, *Terram Sanctam*, reverſus *Venetias.* Poſt A. 1639. militem egit ſup Capitaneo *Caſparo de Monte*, in ſervitiis *Caroli* Ducis *Lotharingiæ*, in ſtatu *Mediolanenſi*, & A. 1663. in *Luſitaniâ*, ſub Colonello *F. Planta.* A. 1682. interfuit obſidioni *Viennenſi*, & in ultimo bello obſidioni *Landaviæ* & Cladi *Hockſtettenſi.* Ita vitam, per varios caſus & mille rerum diſcrimina, protraxit ſenex noſter ad annos 109. menſes 3.

In cadavere obſervavimus, curſoriè veluti, (non equidem licuit per temporis anguſtiam ſectioni diu immorari) ſequentia.

In abdominis cavo erat, parvâ quidem copiâ, ſerum ſanguinolentum extravaſatum.

Inteſtina tenuia fuere omnia inflammata, rubro colore tincta, & Duodenum præ primis integrum admodum dilatatum, internâ ſui parte fracidum prorſus & gangrænoſum; Omentum prorſus emaciatum, ut vix agnoſci potuerit: Pancreas contractum: Hepar ſanum: Veſicula fellis bile repleta: Ductus Choledochi eâdem turgidi, & tota vicinia Inteſtinorum & Meſenterii virore infecta, ut alicubi extravaſata videretur bilis, cujus egreſſum in Duodenum invenire haud potuimus. Prope Pylorum, in Ventriculi ſuperiore parte, aderat expanſio veluti flatulenta, nuce juglande major. Renes ſani, ut & Lien, cujus exterior membrana, jucundo admodum ſpectaculo, obſeſſa erat hinc inde maculis nivei candoris, magis minuſque latis, quæ primo intuitu referebant puſtulas Variolarum maturas, ſed cartilagineæ prorſus erant duritiei, & ultra ſuperficiem reliquæ membranæ aliquantum elevatæ. Obſervo heic, antequam progrediar ulterius, non duntaxat rigeſcere fibras, & contrahi ſupremâ ætate, ſed tantum non inſenſiles fieri; nemo quippe juvenilis vel conſiſtentis ætatis pateretur inflammationem Inteſtinorum, qualis hæc fuit, abſque acutiſſimo dolore, de quo tamen æger poſt ſenile delirium, quod aliquamdiu eſt perpeſſus, ad ſe ultimis vitæ diebus rediens, nihil eſt conqueſtus. Cartilaginea illa Lienis cruſta manifeſto eſt indicio, poſſe ita rigeſcere membranas, ut tandem indurentur.

Thoracis apertio non parum faceſſebat laboris. Fuerunt quippe non ſolum Sterni illæ coſtoſæ prominentiæ, aliàs cartilagineæ, heic planè oſſeæ, ſed & cum ipſis Coſtis tantum non continuæ, indicio manifeſto, Cartilagines fieri ſucceſſu temporis oſſeas, quemadmodum membranæ & arteriæ fiunt cartilagineæ, & ex cartilagineis non ſemel oſſeas narrant Hiſtoriæ. Pulmones viridantibus paſſim maculis obſeſſi, in utroque Thoracis latere, poſticâ ſui parte affixi erant Coſtis: Pericardium amplum & multo ſero refertum, cujus & aliqua portio in cavo fluitabat Thorace. Cor ipſum vidimus ſatis amplum, imprimis Auriculas dilatatas præter modum, & tum haſce, tum Ventriculos ſanguine concreto refertos, unde patet ſubſtitiſſe tandem ſanguinis motum, propter defectum contractionis elaſticæ in tubulis arterioſis, & Cordis machinam ſanguini ad concretionem diſpoſito expellendo non amplius fuiſſe ſufficientem, imo propter nimiam reſiſtentiam obrutam. Præſtabat Tendinem illum, quo Cordi inſeruntur arteriæ, vel oſſeum plane, vel cartilagineum ſaltem: Oſſeus ſane, quod novimus, fit in Cervis adultioribus, unde Os de Corde Cervi. Et ſanè aliquid tale offendimus, nimirum Valvulas ſemilunares, præprimis Aortæ, cartilagineas penè, præcipuè mediâ ſui parte. Aortam ipſam deſcendentem obſervavimus valdè amplam, diametri duplo majoris, quam eſt Gula, Pariſini circiter pollicis. Gulæ inſtratæ paſſim erant Glandulæ lentiformes induratæ. Amplitudo illa Aortæ indicat tardiorem ultimis fortè vitæ annis progreſſivum motum ſanguinis, unde arteriæ, quæ propiores ſunt Cordi, fuere ſenſim dilatatæ, ſicuti ſæpe obſervamus dilatari adeo, ubi nimia eſt molis reſiſtentia, ut in Aneuriſma abeat egrediens è Corde Arteria magna.

Pergimus ad Caput, ubi prædura Cranii ſubſtantia rurſum ſolitò majorem poſcebat laborem: Suturæ, imprimis Sagittalis & Lambdoidea, erant fermè obliteratæ, nec prorſus penetrabant utramque Tabulam, ut nulla fuerit per illas è cavitate illâ magnâ Capitis perſpiratio, & ſuccus ille Oſſium nutritius, inter fiſſuras Suturarum extravaſatus, factus quoque fuerit oſſeus. Foramina quidem tria quatuorve ſatis ampla, in ſupremo vertice, ad latas utrumque Sinus Sagittalis, pollicis fermè diſtantiâ ab invicem viſuntur, internam Laminam ad externam uſque abſque quod ullum illorum utramque perforet Tabulam. Dura Meninx triplo ferè craſſior fuit, quam eſt ordinariè, coriacea penè: Pia ſeparabatur facile, nimio utpotè irrigata ſero, à Cerebri ſubſtantiâ: Ventriculi omnes ſero erant referti, quod ſat magnâ copiâ quoque fluitabat in Cerebri baſi, imprimis ad Spinalis Medullæ egreſſum. Plexus Choroides Glandulis piſalibus, lymphâ coagulatâ refertis, erat obſeſſus: Septum pellucidum egregiè poterat conſpici: Cæterum Cerebri ipſa ſubſtantia, imprimis interioris, flaccidior ſolito.

Innotuit mihi, inde ex quo obiit decrepitus noſter, retuliſſe eum perſonis fide dignis, quod pater ejus, ſeculum jam ſupergreſſus, tres genuerit liberos, & ex majoribus pauci infra ſeculi ætatem obierint. Hoc certum eſt, noſtro natam filiam *Mariam Magdalenam* ex uxore *Urſulâ Hirzeliâ* de *Naniken* pagi *Tigurini*, quæ S. Baptiſmatis charactere ſignata fuit *Dieſſenhovæ*, 18 Aug. 1707.

FINIS.

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PHILOSOPHICAL TRANSACTIONS.

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I. *Ob-*

I. *A Letter from Dr.* George Mackenzie, *to Sir* Hans Sloane, *Bar. Coll. Med. Lond. Præſ. concerning the* Coati Mondi *of* Braſil.

S I R,

YOU know that the *Coati Mondi* of *Braſil* is ſeldom or never brought alive into *Europe*; yet there were two of them found in Capt. *Green*'s Ship, who was ſome Years ago try'd and condemn'd here for Piracy, one of which being given in Compliment to my Lady *Seaforth*, dy'd in my Cuſtody of a Wound it had received in the Thigh, which I cauſed to be diſſected, and have ſent you the Account of it, compared with that which the *Pariſian* Academiſts publiſh'd of one of them; and which differs from theirs in ſeveral Particulars, moſt of which, as I believe, may proceed from the Difference of Sex, theirs being a Male, and ours a Female.

Theirs was ſix Inches and an half from the end of the Snout to the hinder part of the Head, ours was only four; theirs was ſixteen Inches from the *Occiput* to the Beginning of the Tail, ours was ten; theirs from the Inſertion of the Tail to the End was thirteen Inches, ours twelve; theirs from the Top of the Back to the Extremity of the fore Feet was ten Inches, ours was ſeven; theirs from the Top of the Back to the Extremity of the hinder feet was twelve Inches, ours eight; the Snout of theirs was very long and moveable, like that of a Hog, but ſtreighter and longer in Proportion, but ours was only two Inches; the four Paws had each five Toes, the Claws of which were black, long and hollow, like thoſe of the Caſtor; the Toes of the Fore-paws were a little longer than thoſe of the Hind-paws; the Soles without Hair; the Palms and Soles of theſe Fore-paws were covered with a ſoft and tender Skin; the Sole of the hinder Paw was long, having a Heel, at the Extremity of which there were ſeveral Scales a Line broad, and five or ſix long, in all which they perfectly agreed.

The Ears were round, like thoſe of Rats, and covered at the Top with very ſhort Hairs, and in this they likewiſe both agreed, as they did in the Eyes, which were extreamly ſmall and beautiful, but there was ſome Difference in the Hair, for theirs was ſhort, rough and knotty, blackiſh on the Back and Head; and the reſt of the Body mixt with black and red; but in ours the Hair was long, in Proportion to the Animal, eſpecially on the Tail, and the whole was beautified with white and black Circles, which made it have a moſt lovely Aſpect: But from the Snout down all the Throat and Belly to the top of the Tail and the inſide of the Legs, was of a reddiſh Colour. The Tongue of both theirs and ours was chopt with ſeveral Fiſſures or Strokes, which made it rough to the Touch. The *Inciſores* were ſix in each Jaw: The *Canini* were very large, eſpecially thoſe of the lower Jaw; but they did not turn up like Tuſks as theirs did; their Figure was not round, blunt, or white, like thoſe of a Dog, Wolf, or Lyon, but ſharp, by the means of three Angles, which at the Extremity form'd a Point ſharp like an Awl: As to their Colour, they were greyiſh and ſomewhat tranſparent: The *Gula*

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was large, and cleft like a Hog's; and the lower Jaw, as in a Hog, very much ſhorter than the upper.

We ſhall now proceed to the internal Parts. By the Diſſection we found in ours, as the *Pariſian* Academiſts did in theirs, that under the Skin, and between the Muſcles, there was a great deal of Fat, white and hard, like Tallow. Theirs being a Male had a *Penis* provided with a Bone, whoſe Length did in proportion exceedingly ſurpaſs that of the Bones which are found in the *Penis* of other Animals; ſo we in ours, being a Female, obſerved, that it had an exceeding large *Matrix*, and that the Inſertion of the *Urethra* was upon the right Side of the *Vagina*. The *Epiploon* in ours, as in theirs, was very ſmall; it had little Fat, and was a Complication of Fibres and Fillets, rather than a Membrane; it was not laid upon the Inteſtines, but touched upon the Ventricle. In theirs they obſerved a very large Spleen, but in ours we could diſcern none. We did not obſerve more than they any Veſſels in the external Membrane of the Ventricle, but the *Coronaria Stomachica*, which appear'd as in theirs towards the upper Orifice, and ſoon diſappear'd, ſhooting forth a few Branches. The Liver in ours, like theirs, was ſomewhat blackiſh, and of a Subſtance very homogeneous, without any Appearance of Glands: It had ſeven Lobes, two great ones on the left Side, and five other ſmall ones on the right Side. The *Pancreas* in ours, as in theirs, was faſtened along the *Duodenum*, inclining more towards the right Kidney than the left; but whereas it was very ſmall in theirs, it was very large in ours. The *Meſentery* in ours, as in theirs, was filled with a very hard Fat, which incloſed and almoſt concealed all its Veſſels. The *Inteſtines* in theirs were ſeven Foot long, and all of one Thickneſs, having nothing to diſtinguiſh them; but in ours they were only 42 Inches and a half. Theirs had no *Cæcum*, but we found it in ours at the upper end of the *Rectum*. The Bladder was very large; the right Kidney in ours, as in theirs, was a great deal higher than the left, and covered with the Lobes of the Liver. The Lungs in theirs had five Lobes, two on the right Side and two on the left, and the fifth in the *Mediaſtinum*, which was as thin as a Spider's Web; but in ours there was ſeven Lobes, three on the right and three on the left, and the ſeventh in the Middle. The *Heart* in ours, as in theirs, reſembled that of a Dog, having the right Auricle extreamly great, and as they found a great deal of ſlimy Matter hardened in the right Ventricle, ſo we found in ours a *Polypus*. The *Muſculus Crotophites* paſſing under the *Zygoma* was in ours, as in theirs, faſtened there, being extraordinary fleſhy even to its Inſertion, made by a very large Tendon, which was incloſed between two Pieces of Fleſh, much thicker than thoſe which are generally found in this Place, and which are thought to be put there to defend and ſtrengthen the Tendon of the Muſcle of the Temples.

The Tendons in the Articulations of the fore Feet were very big and ſtrong. In ours we obſerved two Glands on each Side of the *Anus*, with a Paſſage to each of them, full of a greyiſh fœtid Matter. The *Orbita* in ours, like theirs, was not bony throughout, but it was ſupply'd in the upper Part by a cartilaginous Ligament, which joined the *Apophyſis* of the *Os Frontis* to that of the firſt Bone in the upper Jaw. The Bone, which ſeparates the *Cerebrum* from the *Cerebellum*, was as in Dogs. The *Dura Mater* in ours did not adhere to the *Cranium*, as in theirs. The *Sinus's* of the *Os Frontis* in ours, as in theirs, were full of Matter, like a friable Fat. The *Mammillares Proceſſus*, in ours, as in theirs, were very large. In the Eye both of them agreed exactly, the Globe not exceeding four Lines and a half in Diameter, the Aperture of the Lids being much larger, and the *Pupilla* being as large as the whole Globe of the Eye; the Cryſtalline contained three Lines in Breadth, and two and an half in Thickneſs, and was more convex inwards than outwards; this Thickneſs of the Cryſtalline made the two other Humours to be leſs in Quantity. The *Choroides* was all over of the ſame Colour, *viz.* of a very brown red, without any *Tapetum*, which is hardly ever wanting in the Eyes of other Animals.

Theſe are all the remarkable Differences, that we could diſcover, betwixt ours and that of the *Pariſian* Academiſts; only I muſt inform you of ſome Things, as to the Manner of its Living and Diet, which they had not the Occaſion to obſerve in theirs.

I believe they are miſinform'd in ſaying, that they carry their Tails erected, at leaſt the Tail of this was always trailing on the Ground; neither can I be induced to believe that they eat their Tails, for there was no Part of her that ſhe could endure leſs to be handled than her Tail, the leaſt touching of which would make her cry, or rather hiſs like a Snake; ſhe could endure no manner of Cold; for in the Intervals betwixt the Times of eating, ſhe was either beneath the Bed-cloaths, or on a Cuſhion before a Fire, with the Heat of which ſhe ſeemed to be extreamly well pleaſed.

Her ordinary Meat was butter'd Eggs, Milk and Bread, all manner of roaſted Fleſh, but no Fiſh: I once try'd her with a new kill'd Partridge, which ſhe eat of moſt voraciouſly, and, for ſeveral Days after, ſhe was very wild and ungovernable, which made me never afterwards try her with raw Fleſh. I am apt likewiſe to believe, their ordinary Dens or Habitations are under Ground in ſandy Banks, like our Rabbits; for when ſhe was brought to the Fields, ſhe would dig up the Sands with her Paws, with an incredible Swiftneſs, ſo that had ſhe not been chain'd, there had been no Poſſibility of recovering her.

GEORGE MACKENZIE.

II. *Imber Calculorum inter aquarum* Pyrmontanarum *uſum, in Viro, nunquam antehac calculum experto, ac alias perfecte ſano, ſubſequente poſtea mictu cruento, tandemque veſicæ ulcere. Communicante Viro Clariſſimo* Abrahamo Vatero, *M. D. Anat. & Botan. Prof.* Wittemb. *R. S. S.*

NObilis Pomeranus, ſanus ac vegetus, uti ipſe de ſe refert, nulliuſque mali ſibi conſcius, ante hos tres annos, ſuaſu amici, converſationis cauſa, ſocium ſe ipſi adjunxit, in bibendis aquis medicatis *Pyrmontanis*. Factum autem eſt, ut, cum potando aliquot dies tranſegiſſet, calculos quoſdam minores, abſque ullo quidem dolore, excludi inter mingendum ſentiret. Effectum hunc criticum eſſe ſibi perſuadens noſter, aquas iſtas aucta doſi bibere incipit, quo facto, quatuor vel quinque

que dierum ſpatio, ultra quadraginta calculi, rurſus tamen ſine moleſtia, exierunt. Hoc viſo, ſtrenuè & alacriter potare continuat, optimum hujus curæ effectum ſibi promittens. Aſſumpta vero ita nimia aquæ quantitate, urina ſupprimitur, adeo, ut ne minimam ejus portionem per tres dies emittere potuerit, uſque dum catheteris ope via aperiretur. Commonefactus itaque à Medicis, ab uſu aquarum ulteriori abſtinuit, remita peractam eſſe credens. Non ita multo poſt, vir iſte iter faciens, continuo mingendi conatu laceſſitur, ac iterum iterumque ex rheda deſcendere cogitur, tandemque inter graviſſimos dolores ſanguinem purum effundit. Duravit mictus cruentus & doloroſus, quamdiu æger in itinere conſtitutus fuit, poſtquam vero hic, domum reverſus, quieti ſtuduit, ſtatim ceſſavit, nec quicquam doloris remanſit. Quoties vero ab illo tempore ſive rheda, ſive equo veheretur noſter, rediit dolor, & ſanguis cum urina ſeceſſit, ſed quiete reſtituta, iterum evanuit, nullo planè dolore ſuperſtitè. Contigit quidem interdum, ut calculus unus vel alter prodiret, rarius tamen hoc factum eſt, nec ullum ex eo dolorem percepit æger, quoniam hi longè minores, quam quos inter aquarum uſum reddidit, fuerunt. Symptomate hoc admodum moleſto per duos integros annos vexatur æger, tandem uſu medicamentorum liberatus eſt. Ceſſante itaque penitus mictu cruento, nil magis timuit, ſed integræ ſanitati reſtitutus ſibi viſus, medicamentis valedixit. At, quid fit? Augetur poſt illud tempus indies Strangurìa, virque continuo die nocteque ad urinam reddendam ſtimulatur, quo malo ipſe ad hunc uſque diem affligitur. Urina, quæ parciſſima quantitate, ſummoque cum dolore emittitur, craſſa exit, ob pituitam viſcidam admiſtam, quæ fundum ſtatim petit, glutini ex farina confecto ſimilis, tantæ tenacitatis, ut in illa quaſi trahi queat. Æger interea appetitu bono gaudet, & ſomno quoque ſufficienti utitur, nec ullibi præterea in corpore dolorem ſentit.

Conſideratio Caſus, ejuſque circumſtantiarum ſingularum.

Si quis hiſtoriam morbi, quoad omnes ejus conditiones, accuratè perpendat, mecum confitebitur, eandem vix parem inter obſervationes Medicas agnoſcere. Notatu digniſſimum imprimis eſt, potu aquarum iſtarum mineralium, calculos ſat inſignes, in tanta copia, atque intra tam breve temporis ſpatium excluſos eſſe, a viro, uti ipſe de ſe refert, ante aquarum uſum perfectè ſano, nec ullum calculi indicium unquam ſentiente. Non levem hoc in animo excitare poteſt ſuſpicionem, calculos non tam ab aquis propulſos, quam ex croco minerali, qualem aquæ tales medicatæ in ſinu gerunt, generatos potius in hoc ægro fuiſſe. Parum quidem huic ſententiæ favere videtur tempus admodum breve tot calculorum concretioni vix idoneum, quoniam hi, paucos poſt dies ab inchoato aquarum potu, excluſi ſunt. Non tamen exiſtimo, quenquam facilè definire poſſe, quantum temporis ſpatium ad calculi in corpore generationem requiratur; poterit hoc forte eſſe exiguum. Longè enim minus veritati conſentaneum videtur, calculos iſtos jam tum in viis urinariis latitaſſe, quoniam vir ille nunquam antehac urinæ difficultatem, aut aliud calculi ſignum percepit. Confirmant e contrario omnes hujus caſus circumſtantiæ, *concretionem iſtam ſubito factam eſſe.* Calculi enim ipſi nullatenus aſperi atque anguloſi, quales alias veſicæ calculi eſſe ſolent, ob ſucceſſivam ſpiculorum ſalinorum cohæſionem, ſed piſorum majorum minorumque inſtar, rotundi, glabri, & politi obſervantur, quod coagulationem eorum ſubitaneam

bitaneam docere videtur. Color eorum luteus convenit exactè cum ſedimento illo, coctione ex aquis *Pyrmontanis* extracto, cujus minimam portionem ex quatuor unciis aquæ hujus paratam addere volui, ut tanto melius convenientia iſta pateſcat. Fracti calculi ſtrata ejuſmodi variegata interius monſtrant, qualia in tophis thermarum Carolinarum deprehenduntur, quorum pariter particulam exiguam adjeci. Addidi propterea quoque calculos duos, cum aliquot fruſtulis contritorum, pauciſſimos enim tantum obtinui. Ultimo tandem loco attendi quoque meretur ſingularis iſta circumſtantia, quod maxima horum calculorum pars inter ipſum aquarum uſum excluſa ſit, poſt illud tempus vero rariſſimè tales, iique longe minores prodierint. Non tamen ideo culpa hujus rei in aquas iſtas medicatas conjicienda erit, quaſi hæ ad calculi generationem diſponant, cum potius debilitati viſcerum, ac pravæ digeſtioni adſcribi debeat effectus ille extraordinarius. Nolo etiam leviſſimas has meditationes atque conjecturas pro veritate indubitata venditare, rem totam potius Illuſtriſſimæ Societati Regali decidendam, ea, qua erga ipſam ſum, pietate ac obſervantia trado.

Dabam Wittembergæ Saxonum,
d. vi. Jan. MDCCXXIII.

III. *Obſervations*

III. *Obſervations upon diſſecting the Body of a Perſon troubled with the Stone. By Dr.* Perrot Williams, *Phyſician at* Haverford-Weſt *in* South-Wales.

MR. *William Bowen* of the Town and County of *Haverford-Weſt*, aged between 40 and 50, having been, for about the Space of ſeven Years, ſeverely afflicted with the uſual Symptoms of the Stone in the Kidneys and Bladder, *viz.* Bloody Urine after Exerciſe, Strangury, *&c.* Dy'd in *May* 1722. His Body being opened, there were found in the Bladder ſix ſmooth oval Stones, exactly of the ſame Figure, and nearly of the ſame Magnitude: There were alſo three Cells in each Kidney, the Figure of each ſuitable to that of the Stones: The *Ureters* were ſo preternaturally extended, as very eaſily to admit the largeſt of the Stones to paſs from the Kidney to the Bladder. The *Viſcera, &c.* appear'd in their natural State.

IV. *Hiſtoria Sectionis Viri ex Renum Calculo defuncti. Ex Epiſtolâ Viri Doctiſſimi* P. Hardiſway, *M. D. ad* Gulielmum Stukeley, *M. D. Coll. Med.* Lond. *& Reg. Soc. Soc.*

HOmo ſeptuaginta circiter annos natus, ventris dolore horrendo correptus eſt die Octobris 19° An. 1722. Acceſſerunt Vomitus & Urinæ difficultas, præ doloris atrociâ ſeſe erigere non poterat, ſed miſere incurvatus, dies noctesque prope focum jacebat ejulans. Sexto morbi die, urinâ prorſus ſuppreſsâ, de doloroſâ circa Hypochondria ſtricturâ, perinde ac a funibus circumjectis, querebatur, poſtea de pondere quodam in veſicâ moleſto, quaſi Rapi majoris, (ſic aiebat) inibi hoſpitantis. Omnibus in pejus ruentibus, die morbi duodecimo ad plures abiit. Cadavere aperto veſicam ſecuimus, quam ab ullis vel minimis calculi veſtigiis immunem invenimus; in utroque autem rene (multâ aquâ refertis) repertus eſt calculus, (*Fig.* 1, 2.) inſignis, ſcaber & multiplici ramo per parenchyma ſe diſpergens, quem integrum extrahere nullâ arte potuiſſes, niſi totâ prius fruſtulatim decerptâ carne. Ramorum maximus in ureteris caput ſe inſinuans, perbelle adeo id occupaverat, ut ampulla obturamento ſuo arctius occludi vix poſſet. Ulterius ſcrutari nobis non licuit, ideo unum tantum addam, nempe, quod Omentum inſtar faſciæ anguſtioris ſub hepate retractum delitefcebat. Pauper erat & corpore athletico, ſemper ſanus, niſi quod a paucis annis, aliquando ſabuloſa per urinas excerneret.

V. *An Account of the Depth of Rain fallen from* April 1, 1722, *to* April 1, 1723. *Obſerved at* Widdrington *in* Northumberland, *and communicated to the Publiſher by the Reverend Mr.* Horsley.

I Have kept an exact Account of what Rain has fallen the laſt Year in this Place, which I have here ſent you. The weighing the Water and reducing it from Weight to Depth ſeem'd pretty troubleſome, even when done in the eaſieſt Method: To remedy this Inconvenience (beſides a Funnel and proper Receptacle for the Rain) I uſe a cylindrical Meaſure and Gage. The Funnel is 30 Inches diameter, and the cylindrical Meaſure exactly 3; the Depth of the Meaſure is 10 Inches, and the Gage of the ſame Length, with each Inch divided into 10 equal Parts; or, inſtead of a Gage, the Inches and Diviſions may be mark'd on the Side of the cylindrical Meaſure. The *Apparatus* is ſimple and plain, and it is eaſy to apprehend the Deſign and Reaſon of the Contrivance; for the Diameter of the cylindrical Meaſure being juſt $\frac{1}{10}$ of that of the Funnel, and the Meaſure exactly 10 Inches deep, 'tis plain that 10 Meaſures of Rain make an Inch in Depth; one Meaſure, $\frac{1}{10}$; one Inch on the Gage, $\frac{1}{100}$; and $\frac{1}{10}$ of an Inch on the Gage, $\frac{1}{1000}$, *&c.* By this Means the Depth of any particular Quantity which falls, may be ſet down with Eaſe and Exactneſs, and the whole at the End of each Month, or every Year, may be ſumm'd up without any Trouble.

By the following Account you'll ſee, that ſome of the Summer Months, particularly *May* and *July*, were very wet, and ſome of the Winter ones very dry; ſo that, one with another, this Year's Rain, as far as can well be conjectur'd, may be look'd upon as a *Medium*. And if ſo, it differs not above two or three Inches, from the mean Quantity of Rain which falls at *Upminſter*, *Paris*, and *Liſle*, being leſs than at *Liſle*, and more than at the other two Places.

An Account of the Depth of Rain fallen from April 1, 1722, *to* April 1, 1723.

	Inch.
In *April*	1,015
In *May*	3,532
In *June*	2,570
In *July*	4,350
In *Auguſt*	2,132
In *September*	1,155
In *October*	,600
In *November*	2,205
In *December*	1,780
In *January*	1,225
In *February*	,485
In *March*	,195
In the whole Year	21,244

VI. Pappi

VI. Pappi Alexandrini *Propoſitiones duæ generales, quibus plura ex* Euclidis *Poriſmatis complexus eſt, Reſtitutæ a Viro Doctiſſimo* Rob. Simſon, *Math. Prof.* Glaſc. *Vid.* Pappi *præfationem ad Lib. 7. Coll. Math.* Apollonii *de Sectione rationis libris duobus a Clariſſ.* Hallejo *præmiſſam* pag. VIII. *&* XXXIV.

Ἐὰν ὑπτίου ἢ παρυπτίου ἢ παραλλήλου, &c.

Textus Pappi, qui in hac & ſequente Propoſitione injuria temporis truncatus eſt, hoc modo Reſtituendus videtur.

"[SI duæ rectæ lineæ in duas rectas lineas ſibi mutuo occurrentes vel inter ſe parallelas ducantur,] & dentur in una earum tria [a] puncta [vel duo, ſi recta in qua ſunt, parallela fuerit alicui ex tribus reliquis] : cætera vero puncta præter unum [b] tangant rectam poſitione datam, etiam hoc quoque tanget rectam poſitione datam." Hoc autem de quatuor tantum rectis dicitur, quarum non plures quam duæ per idem punctum tranſeunt. In quolibet vero propoſito rectarum numero ignoratur, quamvis vera ſit, hujuſmodi Propoſitio. "Si quotcunque rectæ occurrant inter ſe, nec plures quam duæ per idem punctum; data vero ſint puncta omnia in earum unâ, unumquodque

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[a] *Tria puncta*] interſectionum ſc. [b] *Tangant rectam*] i. e. unum punctum tangat unam aliquam rectam poſitione datam, & aliud tangat aliam poſitione datam, &c.

" autem punctum in aliâ tangat rectam poſitione da-
" tam; " vel generalius ſic. Si quotcunque rectæ occurrant inter ſe, neque ſint plures quam duæ per idem punctum, omnia vero puncta in earum unâ data ſint; reliquorum numerus erit numerus triangularis, cujus latus exhibet numerum punctorum rectam poſitione datam tangentium; quarum interſectionum ſi nullæ tres exiſtant ad angulos trianguli ſpatii, [nullæ quatuor ad angulos quadrilateri, nullæ quinque, &c. i. é. univerſim, ſi nullæ harum interſectionum in orbem redeant] unaquæque interſectio reliqua tanget poſitione datam. Propoſitio prima in decem dividitur caſus, monente ipſo Pappo, quorum ejus, in quo nullæ ex quatuor rectis ſunt inter ſe parallelæ, neque rectæ poſitione datæ per data puncta tranſeunt, demonſtrationem hic apponemus; hic enim Caſus inter omnes maximè eſt generalis, ejuſque demonſtratio ſecundæ propoſitionis demonſtrationi omnino eſt neceſſaria. (*Fig.* 3. *&* 4.) Sint igitur quatuor rectæ A B, A D, B E, C E. Et data ſint tria puncta interſectionum A, B, C in earum qualibet, reliquarum vero interſectionum D, E, F, una D tangat rectam G K poſitione datam, alia E tangat rectam H K poſitione datam; tanget etiam reliqua F rectam poſitione datam. Ducatur per F recta M F parallela ad A B, quæ occurrat ipſis H K, K G, C E, in M, N, O. Quoniam igitur data eſt ratio H B ad B C dabitur eidem æqualis ratio M F ad F O, & quoniam datur ratio A C ad A G, dabitur eidem æqualis ratio F O ad F N; quare datur ratio M F ad F N, igitur ſi jungatur F K, quæ occurrat ipſi A B in L, dabitur ratio H L ad L G; & datur H G poſitione & magnitudine; quare punctum L datur, & datum eſt punctum K, igitur K L poſitione datur.

Sit igitur H L ad L G in ratione compoſita ex rationibus H B ad B C & A C ad A G, & jungatur K L, erit hæc recta quam tangit punctum F, hoc eſt, ſi ducatur quævis C E, poſitione datis occurrens in D, E, & jungantur A D, B E ſibi mutuo occurrentes in F; recta erit linea quæ per K, F, L tranſit. Nam per F ducatur M F parallela ipſi A B, & quoniam ratio M F ad F N compoſita eſt ex rationibus M F ad F O & F O ad F N, hoc eſt, ex rationibus H B ad B C & A C ad A G, ex quibus etiam componitur ratio H L ad L G; erit H L ad L G ut M F ad F N, & igitur H G ad M N, hoc eſt, H K ad M K ut H L ad M F: Quare recta eſt linea quæ per K, F, L tranſit, per 14. 1 aut 32. 6 Elem.

Explicatio Secundæ Propoſitionis.

Obſervandum hic eſt, Numerum interſectionum, quæ in una recta reperiuntur in quâcunque propoſita multitudine rectarum, quarum non plures quam duæ per idem punctum tranſeunt, & quarum nullæ ſunt inter ſe parallelæ, unitate minorem eſſe ipſo numero rectarum: Nam duæ in unico puncto ſe invicem ſecant, tertia vero ducta priores in duobus, quarta priores in tribus punctis ſecat, &c. Et igitur numerus interſectionum in tribus rectis eſt unitas binario aucta, i. e. ternarius; numerus eorundem in quatuor rectis eſt ternarius ternario auctus; in quinque vero rectis eſt ultimus præcedens ſeu ſenarius quaternario auctus, &c. in infinitum; Qui numeri, ut manifeſtum eſt, triangulares ſunt, quorum cujuſque latus eſt numerus interſectionum, quæ inveniuntur in unâ qualibet recta, i. e. numerus qui unitate minor eſt numero omnium rectarum. Igitur ſi ex hoc numero omnium interſectionum dematur numerus omnium punctorum datorum, qui idem eſt cum numero interſectionum in una quâvis recta; reliquus erit adhuc triangularis, cujus latus

tus ſc. unitate deficit a latere prioris, quod exhibet numerum omnium punctorum, & proinde binario minus eſt numero rectarum propoſitarum. Et hic eſt numerus interſectionum quas tangere rectam poſitione datam *Pappus* in hac Propoſitione requirit, quarumque ſi nullæ tres ſint ad angulos trianguli; [nullæ quatuor ad angulos [c] quadrilateri & ita deinceps;] unamquamque interſectionem reliquam tangere rectam poſitione datam affirmat. Quæ autem uncis incluſa ſunt, textui neceſſitate coacti adjecimus, nam ſine iis propoſitio vera non eſſet extra caſum quinque rectarum.

Commode vero in duos caſus dividitur propoſitio; quos etiam aperte ſatis indicat *Pappus*, qui Hypotheſin caſus facilioris propoſitioni hujus generis univerſaliſſimæ ſimul & elegantiſſimæ præmittit.

CASUS PRIMUS.

Si quotcunque rectæ occurrant inter ſe nec plures quam duæ per idem punctum; data vero ſint puncta omnia in earum unâ, unumquodcunque autem punctum in alia tangat rectam poſitione datam; unaquæque interſectio reliqua tanget rectam poſitione datam. Sint enim quotcunque rectæ, (*Fig.* 5.) *ex. gr.* ſex A F, B G, C H, D K, E L, E A; & data ſint omnia puncta in earum unâ, ſc. A, B, C, D, E; omnia vero puncta in aliâ ſc. F, L, M, N, tangant rectam poſitione datam: unaquæque reliqua interſectio tanget poſitione datam.

Sumatur enim quævis ex reliquis *ex. gr.* O, & quoniam quatuor ſunt rectæ O L, O N, A N, A B, & data ſunt tria puncta in unâ earum ſc. A, B, E, reliqua vero

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[c] Intelliguntur etiam hic figuræ quarum latera ſe mutuo decuſſant Diagonalium inſtar, æque ac cæteræ.

vero præter unum O, viz. ipſa L, N tangunt rectam poſitione datam, tanget etiam O poſitione datam per *Prop.* I. Eodem modo idem de omnibus reliquis oſtendetur.

CASUS SECUNDUS.

Cæteris manentibus jam non ſint omnia puncta rectam poſitione datam tangentia (quorumque numerus binario minor eſt numero rectarum propoſitarum) in eâdem recta, ſed nulla eorum in orbem redeant; oſtendendum eſt reliqua omnia tangere rectam poſitione datam.

LEMMA I.

Si quotcunque rectæ inter ſe occurrant neque plures quam duæ per idem punctum, & ſumantur quævis rectarum, ſit vero numerus interſectionum, qui conficitur ſumendo duo puncta in unaquâque rectarum ſumptarum æqualis numero harum rectarum; puncta hæc in orbem redibunt. Nam quoniam ſunt duo puncta in unaquâque recta, erunt ad minimum tria in duabus rectis, & quatuor in tribus & ita deinceps; ſemper ſc. erit numerus punctorum ad minimum unitate major numero rectarum niſi recta ultima tranſeat per punctum primum; i. e. niſi rectæ in orbem redeant, in quo ſolo caſu æqualis erit numerus punctorum numero rectarum.

LEMMA II.

Si quotcunque rectæ inter ſe occurrant neque plures quam duæ per idem punctum, ſumantur vero quælibet ipſarum interſectiones, quarum numerus numero omnium um rectarum æqualis ſit; vel hæ interſectiones omnes, vel earum aliquæ, in orbem redibunt, ſeu invenientur ad angulos polygoni vel trianguli.

Nam tres interſectiones trium rectarum ſunt ad angulos trianguli; ſi vero ſint quatuor rectæ, & ſumantur quatuor puncta, una harum neceſſario invenietur in unâquaque recta; quod ſi in unâ aliqua ex quatuor rectis unum tantum inveniatur punctum, tria reliqua erunt in tribus reliquis rectis, & igitur ad angulos trianguli: Si vero nulla fuerit recta, in qua unum duntaxat punctum invenitur, erunt duo in unâquaque ex quatuor rectis, & ſunt quatuor puncta, ergo per *Lem.* 1. ſunt ad angulos quadrilateri. Et manifeſtum eſt ſi fuerint quatuor rectæ, & ſumantur plura quam quatuor puncta, multo magis aliqua eorum in orbem redire.

Sint jam quinque rectæ, & ſumantur quinque interſectionum puncta, &, ſi fuerit aliqua ex rectis in qua nullum invenitur punctum ex hiſce quinque, erunt omnia quinque in quatuor reliquis rectis; Si vero fuerit aliqua recta in quâ unum duntaxat invenitur punctum, erunt reliqua quatuor puncta in reliquis quatuor rectis; igitur in utroque caſu puncta aliqua erunt ad angulos trianguli, vel quadrilateri, per præcedentem caſum: Si autem nulla fuerit recta in qua vel nullum vel unicum invenitur punctum, erunt duo in unâquaque ex quinque rectis, & ſunt quinque puncta, ergo per *Lem.* 1. ſunt ad angulos quinquelateri. Eodem prorſus ratiocinio oſtendetur in ſex rectis & ita in infinitum.

In *Fig.* 6. ſunt octo rectæ, & octo ſumuntur puncta, quorum quatuor in orbem redeunt.

Hiſce præmiſſis propoſitio hoc modo demonſtratur. Primo ſint quinque rectæ (*Fig.* 7.) A D, A E, B F, C G, D H, & demptis punctis datis in una rectarum, viz. A, B, C, D, reliqua erunt ſex puncta E, F, G, H, K, L, in quatuor rectis, & tria horum (nam latus numeri triangularis 6 eſt 3) quæ non ſunt ad angulos trianguli, a tribus ſc. rectarum propoſitarum contenti, *ex. gr.* E, F, G, tangant rectam poſitione datam; oſtendendum eſt reliqua tria K, H, L etiam tangere rectam poſitione datam.

Quoniam igitur ſunt quatuor rectæ A E, B F, C G, D F, & tria interſectionum puncta in ipſis ſumantur, viz. E, F, G, erit una aliqua harum rectarum in quâ neceſſario invenietur unum tantum ex hiſce tribus punctis; nam ſecus erit vel aliqua in qua nullum eſt punctum, & proinde erunt tria puncta in tribus reliquis rectis, i. e. ad angulos trianguli contra Hypotheſin; vel erunt ad minimum duo puncta in unâquaque quatuor rectarum, & igitur quatuor eſſent ad minimum puncta; ſed ſunt tantum tria; quare neceſſe eſt eſſe aliquam rectam in qua unum tantum invenitur punctum: Sit hæc recta A E, in qua ſc. eſt punctum E, ergo reliqua duo F, G, ſunt in reliquis tribus rectis B F, C G, D F; igitur, quoniam dantur tria puncta B, C, D, reliquum punctum L in iſtis tribus rectis, tangit rectam poſitione datam per primam Propoſitionem: Sumatur nunc G E, recta ſc. ex hiſce tribus quæ tranſit per punctum E in quartâ recta, & omnia puncta in hâc recta G E tangent poſitione datam. Quare, per caſum primum hujus propoſitionis, reliqua puncta K, H tangunt rectam poſitione datam.

Sint jam ſex rectæ (*Fig.* 8.) A E, A F, B G, C H, D K, E L; & demptis quinque datis punctis A, B, C, D, E, quæ ſunt in unâ rectarum, reliqua erunt decem puncta F, G, H, K, L, M, N, O, P, Q in quinque rectis; & ex Hypotheſi quatuor horum, quæ non in orbem redeunt, tangunt rectam poſitione datam; ſint hæ, F, G, H, K; & oſtendendum eſt reliqua ſex L, M, N, O, P, Q tangere rectam poſitione datam.

Quoniam igitur ſumuntur quatuor puncta interſectionum F, G, H, K, in quinque rectis A F, B G, C H, D K, E L; erit una aliqua recta in qua unum tantum ex hiſce punctis reperitur; nam ſecus erit vel aliqua in qua nullum eſt punctum, & proinde quatuor puncta erunt in quatuor reliquis rectis, & igitur aliqua eorum in orbem redibunt per *Lem.* 2. contra hypotheſin: vel erunt duo ad minimum puncta in unaquaque quinque rectarum, & ita eſſent quinque ad minimum puncta; ſed ſunt tantum quatuor, quare neceſſe eſt eſſe aliquam rectam in quâ unum tantum invenitur punctum; ſit hæc A F in quâ ſc. eſt punctum F; ergo reliqua tria G, H, K ſunt in reliquis quatuor rectis B G, C H, D K, E L, & dantur puncta B, C, D, E; ergo per primam partem hujus demonſtrationis reliqua tria puncta in his quatuor rectis, ſc. L, P, Q, tangunt rectam poſitione datam. Sumatur nunc B F, recta ſc. ex hiſce quatuor, quæ tranſit per punctum F in quinta recta; & omnia puncta in hac recta B F tangent rectam poſitione datam: Quare per Caſum primum hujus Propoſitionis reliqua puncta M, N, O tangunt rectam poſitione datam. Eodem prorſus modo demonſtrabitur Propoſitio in ſeptem, octo, &c. rectis in infinitum, ut patet.

Quod autem conditio uncis incluſa in hac propoſitione omnino ſit neceſſaria, patet in his duobus exemplis; idem vero univerſaliter præcedentium ope demonſtrari poteſt.

His adjecit Clariſſimus Profeſſor Poriſmata duo ſequentia primi Libri Poriſmatum Euclidis *a ſe quoque reſtituta.*

Poriſma primum, Lib. 1. Poriſmatum *Euclidis*, quod ſervavit *Pappus Alexandrinus* in præfatione ad *Lib.* 7. Math. Coll. *Vid. pag.* xxxv. *Ejuſd. Præf.*

Si a duobus punctis datis inflectantur dua rectæ ad rectam poſitione datam, abſcindat autem earum una a recta poſitione data ſegmentum dato in eâ puncto adjacens, auferet etiam altera ab aliâ recta ſegmentum datam habens rationem.

Sint enim duo puncta data D, C, (*Fig.* 9.) a quibus ad poſitione datam A B inflectantur D B, C B; quarum una D B abſcindat a poſitione data E F ſegmentum K M adjacens dato puncto M: Oſtendendum eſt alteram C B auferre ab aliâ quadam recta ſegmentum datam habens rationem ad ipſum K M.

Juncta C D occurrat poſitione datis A B, E F in A, F punctis, quæ proinde data erunt. A puncto K, in quo inflexa B D occurrit ipſi E F, ducatur K H parallela ad A D, & occurrens alteri inflexæ B C in H, ipſi vero B A in N. Quoniam igitur dantur puncta A, D, C, dabitur ratio A D ad D C, & igitur ratio N K ad K H; quare ſi jungatur E H occurrens ipſi A D in G, dabitur ratio A F ad F G; ſed datur A F, quare & F G datur, & punctum G; & datum eſt E, quare E G poſitione & magnitudine datur; & datur E F, quare ratio E F ad E G datur; & ducta M O per datum punctum M parallela ipſi A D, & occurrens E G in O, dabitur M O poſitione, & ideo punctum O; & propter parallelas M O, F G, K H eſt M K ad O H, ut E F ad E G, quæ ſunt in data ratione. Igitur recta B C aufert a rectâ E G poſitione data, ſegmentum O H dato puncto O adjacens, in datâ ratione ad ſegmentum M K. *Q. E. D.*

Componetur vero ita, fiat A F ad F G ut A D ad D C, & juncta E G, per M ducatur M O parallela ad A D; oſtendendum eſt, ſi a punctis D, C inflectantur ad A B quævis D B, C B abſcindentes ex ipſis E F, E G, ſegmenta M K, O H punctis M, O adjacentia, fore ipſa in data ratione E F ad E G, ſeu, quod idem eſt, eſſe junctam H K parallelam ipſi A D; hoc vero videtur omiſſum fuiſſe ab *Euclide*, utpote quod tribus verbis indirecte demonſtrari poſſit; *Pappus* autem in *Lem.* 1°. ad Poriſmata, duas directas ejuſdem demonſtrationes affert, quarum ſecundam, quæ paululum eſt corrupta apud *Commandinum*, hic ſubjungemus integritati ſuæ reſtitutam.

Vid. Pap. *Lib.* 7. *fol.* 239. *pag. prior.*

Per Compoſitam vero proportionem hoc pacto.

Quoniam eſt ut A F ad F G ita A D ad D C (*Vid. fig.* Papp. *fol.* 238. *pag. poſt. vel fig. noſtr.* 9.) convertendo erit ut G F ad F A ita C D ad D A, & componendo, permutandoque & convertendo ut A D ad DF ita A C ad C G. Sed proportio A D ad D F compoſita eſt ex proportione AB ad BE, [& E K ad K F, & proportio A C ad C G compoſita eſt ex proportione AB ad B E] & proportione E H ad H G. Proportio igitur compoſita ex AB ad BE & E K ad K F eadem eſt, quæ componitur ex A B ad BE & E H ad H G. communis auferatur ratio AB ad B E, reliqua igitur E K ad K F eadem eſt quæ E H ad H G; quare H K ipſi A G parallela eſt.

Poriſma Secundum. Quod punctum illud tangit rectam poſitione datam. Secundum Poriſma videtur ſequenti modo explicandum eſſe.

Si a duobus punctis datis C, G (*Fig.* 10.) ducantur duæ rectæ C B, G D occurrentes duabus rectis poſitione datis A B, E D, ſitque recta D B puncta interſectionum jungens parallela ipſi C G, quæ per datum punctum ducitur, interſectio K ductarum tanget rectam poſitione datam.

Occurrant enim poſitione datæ ſibi mutuo in H, & juncta K H occurrat C G in F & B D in M. Igitur propter parallelas eſt A E ad E F (ut B D ad D M hoc eſt) ut C G ad G F; & igitur A E ad C G ut E F ad G F, datur itaque ratio E F ad G F, & datur E G, quare punctum F, & datur punctum H, quare H F poſitione. Sit itaque ut A E ad E F ita C G ad G F, & juncta H F erit recta quam tangit punctum K; hoc eſt ducta quævis G D, occurrens ipſi F M in K, erit recta linea quæ per C K B tranſit, nam eſt D B ad D M ut A C ad E F, hoc eſt ex conſtructione ut C G ad G F, quare D B ad C G ut (D M ad G F hoc eſt ut) D K ad K G, & igitur eſt C K B recta linea.

Pappus idem aliter demonſtrat in 2do *Lemmate*, quod hoc modo debet legi: ſc. Ducatur per G, (*Vid. Fig.* Pap. *fol.* 239. *pag. poſt. vel Fig. noſtr.* 10.) recta linea G L parallela D E, & juncta H K ad L producatur. Quoniam igitur eſt ut A E ad E F ita C G ad G F, & permutando [A E ad C G ut E F ad G F]; ut autem A E ad C G ita eſt E H ad G L, [d] quod duæ duabus ſunt parallelæ. Ut igitur E F ad F G ita E H ad G L, atque eſt E H parallela ipſi G L, ergo recta linea eſt quæ per H K L F tranſit.

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[d] *Quod duæ duabus ſunt parallelæ*, ſc. A E ad D B, & G L ad D E, unde eſt A E ad D B ut E H ad D H, & eſt D B ad C G (ut D K ad K G, i. e.) ut D H ad L G; ergo per 22. 5. eſt A E ad C G ut E H ad G L.

VII. De

VII. *De Globulorum Sanguineorum magnitudine, &c. ex Epiſtola D.* Antonii a Leuwenhoek *ad* Jacobum Jurin, *M. D. R. S. Secr.*

Delphis, 19 *Martii*, 1723.

EX tuis litteris intellexi inventam tibi rationem eſſe, qua diametros globulorum ſanguineorum, aliarumque minutiſſimarum rerum, certo cognoſcere poſſis, adeo quidem ut deprehenderis diametrum globuli ſanguinei cum parte $\frac{1}{1940}$ lati pollicis magnitudine convenire.

Hoc ego inventum cum legerem, ſumma voluptate affectus ſum; ſtatimque ſic mecum ratiocinabar: ſi 1940 globulorum ſanguineorum diametri lato pollici longitudine reſpondent; adhæc ſi corpora inſtar globi rotunda eam ad invicem proportionem habent, quam habet numerus cubicus ad ſuum axem, ſicut a Geometris traditur; ſequitur corpus, quod ex 7301384000 globulorum conjunctione componitur, haud majus eſſe corpore globoſo, cujus axis latum pollicem longitudine adæquet.

Et quia de examinando denuo ſanguine me rogaveras; meum ipſius & duarum præterea perſonarum ſanguinem viſu exploravi: planeque comperi 4 diametros globulorum ſanguineorum, uti ſcripſiſti, convenire cum diametro [a] fili argentei, quod mihi mittere dignatus es. Id ſolum dicere habeo, quod globulos quoſdam ſanguineos,

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[a] *Hujus fili argentei diametri* 485 *pollicem, ſive duodecimam pedis Loudinenſis partem, conficiebant; quod compertum fuerat per Methodum in Actis Philoſoph. expoſitam.* No. 355. pag. 761.

guineos, ſed perpaucos, viſu perceperim; quorum quaternæ diametri diametrum fili argentei longitudine aliquatenus excedebant. Verum conjiciebam globulos illos, quos ex pollice per acus punctionem eduxeram, & adhuc calentes vitro imponebam, a rotunditate figuræ nonnihil deſciviſſe ad figuram planam.

Hoc etiam addere habeo, præterquam quod gravis mea ſenectus viſui non incommodare non poſſit, oculum dextrum, magno incommodo meo, aliquantum mihi caligare. Id vero hinc accidere puto, quod plures globuli ſanguinei, in humore cryſtallino innatantes, ante viſum meum vagentur.: quorum alii nullo vel confuſo ordine inter ſe conjuncti, alii ſeorſim fluitantes, imaginem quandam mibeculæ in oculo meo effingunt. Cum autem plerumque utar oculo dextro, ſiniſtrum enim inter ſpeculandum facilè claudo, hinc obtuſior mihi viſus eſt quam eſſe ſolebat.

Haud ita pridem, ſcilicet Januario nuper elapſo, circa magnum illud & neceſſarium organum, quod diaphragma vocitamus, vehementi ſum motu correptus; adeo quidem ut adſtantes non parum turbarentur. Cum motus remitteret, & morbi illius nomen exquirerem, Medicus, qui aderat, cordis eſſe palpitationem reſpondit. Ego verò Medicum falli cenſebam; licet enim, durante motu, pulſum arteriæ ſæpius manu explorarem, nullam pulſus accelerationem percepi. Vehemens ille motus, per intervalla recrudeſcens, tres circiter dies duravit; quo tempore ſtomachus mihi & inteſtina in officio motuque ceſſabant; mortemque pro foribus eſſe certiſſime credebam.

Ego vero obſtructionem in diaphragmate hærere judicabam, non minorem nummo imperiali.

Addis in epiſtola tua, prænobilis Domine, quod ovarium exterminare ſtudeam; ad quod ſalva pace reſpondeo,

ſpondeo, iſtum meum conatum non eſſe nuperum vel novellum, ſed me ante 25, & amplius annos in illo evertendo laboraſſe; eo quod opinio, quæ generationem ab ovario arceſſit, mihi prorſus inſulſa videatur. Ego a permultis modo annis tum aliorum animalium, tum avium atque piſcium, maſculina ſemina examinare ſoleo: quod potiſſimum ſub iſtud tempus quotannis facio, cum piſces ova ſua emittunt: Plerumque autem comperi ſemen maſculinum innumerabili ſcatere copia exiguiſſimorum animalculorum; ex iiſque potiſſimum componi. Quare pro certo ſtatui, minutiſſima illa animalcula in iſtius generis animalia, unde exorta ſunt, creſcendo evadere. Anguillas tamen, ſquillas, piſces conchatos, & varias muſcas excipio; quæ omnes ſunt femellæ, & fructus edunt, hac in parte convenientes cum fructibus arboreis: quorum in ſeminibus Plantulas deliteſcere comperimus; quæ vice maſculinorum ſeminum fructus iterum proferent, fructibus illis congeneres, ex quibus ſemina jam dicta exorta fuerunt.

VIII. *Some Amendments and Additions to the Account of Things found under Ground in* Lincolnſhire, *in the* Tranſactions *of* May *and* June, An. Dom. 1702, Numb. 279. Sect. 4, 5. *By Mr.* Ralph Thoresby, *F. R. S.*

WHEREAS in the Paper aforeſaid, *pag.* 1157. *lin.* 35. and *pag.* 1158. *lin.* 15, 16. there is ſome Difference betwixt the Accounts of the Depth of the Things found; that in the Letter, *Sect.* 5, accounting it to be about 8 or 10 Foot, and that in the Letter, *Sect.* 4. to be 12 or 14 Foot deep; the Reader may pleaſe to underſtand, that the Depth was not meaſur'd, but conjectur'd, and accounted for according to the Relator's beſt Remembrance. That which makes the Depth the greater being that of *Sect.* 4. (firſt placed, but laſt wrote) was wrote upon Information (but by Conjecture alſo) of another that lived near the Place, and view'd it often. But the Difference may eaſily be accommodated or ſalved, by ſuppoſing (which will not be far from Truth) that when the Labourers or Dikers firſt diſcovered and encountred the Jetties and other Things there found, it might be about the Depth of 8 or 10 Foot; but the Bottom of them, when they came to be all taken up, might be at the Depth of about 12 or 14 Foot, as in the other Relation.

Take notice alſo, concerning what is ſaid *pag.* 1159. *lin.* 14. that ſome, and thoſe very judicious Perſons, ſay, that the Stones which the Spectators ſaw at the Bottom of *Hammon Beck*, were ſuch as the Dikers had firſt thrown out, (when they were taking up the old Goat) and were fallen in again. But that it was a hard and firm Soil is certain; and probably that on which the famous Steeple of *Boſton* ſtands. See the Record of the Foundation of the ſaid Steeple in the *Philoſophical Tranſactions, Numb.* 223. *pag.* 243.

The Form of the Shoe Soles found at *Spalding* was after this Manner, (*Vid. Fig.* 11.) each Foot had its proper Shoe: This was for the right Foot.

Now, by ſome Paſſages in Hiſtory, it may probably be conjectured, when thoſe Shoe Soles were left there, and how long it may be ſince that Atterration, that makes the preſent Country, began. Take an Inſtance or two.

In *Stow*'s Chronicle, *ad An.* 1465. we read of a Proclamation againſt the Beaks or Pikes of Shoone, or Boots, that they ſhould not paſs two Inches upon Penalties there mentioned. And by other Paſſages in Hiſtory it appears, that thoſe Pikes of Shoes were before that time exceeding long, and held up by Chains, that they might not hinder the Wearers going; which Chains or Ligaments were ſometimes of Silver, if not of Gold, that they might be rich, as well as ornamental. The other Inſtance is this.

In *Melchior Adamus*'s Life of *Conrad Pellican*, at the Bottom of *Page* 263, in the *Octavo* Edition, there is this Paſſage. *Illo ipſo tempore* (*viz. An.* 1484.) *milites ex* Flandria *reverſi multa nova introduxerunt, nempe variegatas caligas, obtuſos calceos, qui antea acuti geſtabantur a viris & mulieribus. ſimul quoque novum ſandaliorum genus obtuſum, quod vocabant* **Pantofflen**, *quibus ſuos etiam parentes uti conſpexit, omiſſis paulatim calopodiis* **Holzſchuh** *dictis: quæ quidem novitates interim immenſum creverunt.*

Now by this Account it appears, that it is not much above 200 Years ſince thoſe Shoes, before deſcribed. were worn; and conſequently it cannot be much longer, ſince the Earth hath been raiſed there to the Thickneſs before mentioned, *viz.* of 10 or 12 Foot; and ſince *Bicker* Haven grew up to be (as now it is) higher Land than the Country on each Side of it. By which may be conjectured, what a Change a Century or two more may make in the Out-falls of the Rivers of *Witham*, *Welland*, *Nyne* and *Ouſe*; and conſequently the Neceſſity of taking ſome other Method for preventing the impending Miſchiefs, which threaten the Navigation of the ſaid Rivers, and thoſe who have Eſtates and Intereſts in the great Level of the Fens, and are concerned about the Draining of them.

IX. *An Account of a monſtrous double Birth in* Lorrain. *Communicated to the Publiſher by Mr.* Colin Mac Laurin, *Prof. Math.* Abredon. *R. S. S.*

I The Underwritten, Surgeon in Ordinary to his Royal Highneſs the Duke of *Lorrain*, certify, that on the laſt of *December*, 1722, I went by his Orders to *Domp Remy la Pucelle*, to ſee there *Sebaſtiana Camus*, aged 44 Years, delivered on the 24th of the ſaid Month, being *Chriſtmas* Eve, about eight o'Clock in the Evening, of two Children, or Twins, joined together in the Manner as appears by the Draughts, (*Fig.* 1, 2.) *viz.* There is one Head, one Neck, one Breaſt, one *Abdomen*, and two Hands on one Side; and there are as many Parts on the other; the whole being well proportioned and plump, joining together by the Belly, which is common to both, ſo that one of the Heads is

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in the Place where the other's Feet ſhould be, and the other Head is in its natural Place; they have but two Legs for both of them, which ſeem to ariſe from the tranſverſe *Apophyſes* of the *Vertebræ* of the Loins on one Side; and from the oppoſite Region of the Loins, comes out a Leg ending with a Joint which bends forwards, and at the the Extremity forms a ſmall Stump, like a Finger, articulated by *Ginglymus*. There is but one Fundament for both, by which they void their Excrements; they have but one Navel-ſtring, and the Parts proper to the Female Sex are alſo ſingle: They eat and drink with their Mouths ſeverally, and while the Breaſt is given to one, the other cries for it: They ſleep and are awake, ſometimes both at the ſame time, ſometimes ſeparately. Each of theſe Children has been baptized: one of them is ſomewhat plumper than the other, which is more puny, and not ſo freſh-colour'd. The Head of the one, which is a little bigger than that of the other, came firſt to the Birth, the two Arms lying on the Breaſt followed next; the Legs lay on the Sides of the Breaſt of the Second; the oppoſite Leg, which is ſingle, was extricated afterwards; laſt of all, the Arms of the ſecond Child, being ranged on the Side of its Head, made it eaſy for the reſt to come out. The Bodies of both theſe Children make no more in Bulk than that of one ordinarily does.

It is obſervable, that the Mother can aſſign nothing that had had any relation to this Event, during the Time of her Pregnancy.

I am juſt now informed, that both theſe Children are equally well in Health.

NANCY, *the* 21*ſt of* January,
1723. *N. S.*

FEVRY.

By another Account communicated to the Royal Society, *we are inform'd, that theſe Children lived two Months after the Birth.*

Fig. 1. Repreſents the fore View of the Twins.
2. Their Back-part.
3, & 4. The Skeleton.
5. The Breaſts and Abdominal Muſcles, after the external Teguments were removed.
6. Part of the *Viſcera* of the *Abdomen.*
7. The Urinary and Genital Parts.

The two firſt Figures were drawn in their Life-time, and the five following were taken after their Death, by Order of his Royal Highneſs the Duke of Lorrain.

X. *Obſervations and Experiments on the* Sal Catharticum Amarum, *commonly called the* Epſom Salt. *By Mr.* John Brown, *Chymiſt, F. R. S.*

THAT the Salt, of late Years diſtinguiſhed by the Title of *Sal Catharticum,* is made from what, at the Salt Works, is called *Bittern,* is pretty commonly known; but the particular Manner how this *Bittern* is produced, and from it, theſe Salts, has not yet, as I know of, been communicated to the World, in ſuch a Manner, as to become intelligible. And the *Opprobrium* unjuſtly caſt upon the Salt, of its being a counterfeit *Epſom Salt,* or ſomething made in Imitation of it, or common Salt diſſolved and recryſtalized, has very much ſunk its Eſteem among the Learned in Phyſick.

By the Account M. *Bolduc* gives no longer ago, than in the Hiſtory of the Royal Academy of *Paris*, for the Year 1718, I find he did not ſucceed according to his Wiſh in the Experiments he try'd, to find out what this Salt was made from, juſtly grounding his Reaſons for its not being all made from the *Epſom* Waters, or other Springs that afford bitter purging Waters, becauſe of the large Quantities conſum'd, and the Cheapneſs of its Price. After all his curious Endeavours, it ſtill remain'd a Secret, until the Information he received from hence by Dr. *Mendez*, whoſe Account is as follows, though very imperfect. It comes, ſays he, from *Lemington* and *Portſea*, both in *Hampſhire*, where from Heaps of foſſile Salt, there runs a ſaltiſh, bitter, ſharp, and pungent Liquor. One would judge by its Brackiſhneſs and Bitterneſs, that it contained two Kinds of Salt, the one a Sea Salt, the other a bitter Salt. To ſeparate theſe Salts, they cauſe this Liquor to run through hollow Drains on the Ground; there it gets together, and condenſes into Salt; this they put into a large Veſſel, with a great Quantity of common Water, and boil it as long as is ſufficient to diſſolve it; then they let it cool and ſettle for ſeveral Days. The Water, impregnated with the Sea Salt, which is the heavieſt, ſinks to the Bottom of the Veſſel with the earthy Parts, and the Water impregnated with the bitter Salt, which is the lighteſt, ſwims at Top. They take off this upper Liquor, as long as it retains its bitter Taſte, without any Pungency; afterwards they boil it in one or two Waters, then evaporate it, and it yields white and tranſparent Cryſtals, which are the counterfeit *Epſom Salt*.

What Experiments M. *Bolduc* made with the *Bittern*, ſent him by that Gentleman, I do not remember to have met with, in any of the ſucceeding Memoirs.

Dr. *Seipp*, in his Deſcription of the *Pyrmont* Waters, *Page* 127, ſays, that the common *Engliſh* purging Salt, which is ſold in great Quantities in *Germany*, under the Name of *Epſom Salt*, is not prepared from the *Epſom* Waters, but is made in *London* from common Sea Salt, and Oil of Vitriol.

In the ſame *Page* he ſays, that the Salt obtain'd from the *Pyrmont* Waters will part with its own acid Spirit, upon pouring on the *Ol. Vitriol.* which the *Sal Mirabile* and the *Engliſh* purging Salt will not. By this Means he diſtinguiſhes the firſt Salt from the two laſt.

Dr. *Quincey*, in his *Prælectiones Pharmaceuticæ*, publiſh'd ſince his Death, ſays, there hath lately been contriv'd a Salt from the Mineral Purging Waters, made by Evaporation, Filtration, and Cryſtalization. It was firſt entitled *Sal Mirabile*, or *Sal Catharticum Amarum*; but it is now ſo ſcandalouſly counterfeited, that it is little elſe than common Salt diſſolv'd and recryſtalized.

Before I enter into the Account how this Salt is made, it may not be amiſs, firſt, to ſay ſomething of the genuine Salt that has been made from the bitter purging Waters, of which the learned Dr. *Grew* was the firſt who attempted the making it at *Epſom*. Some Years after, ſeveral other bitter purging Springs were found in different Counties, and Salts in ſmall Quantities were boyl'd up from them, but from no Place, nor all the Places put together, in ſuch large Quantities, as from the Springs on one ſide of *Shooters-Hill* in *Kent*, about the Year 1700, which were then in the Poſſeſſion of thoſe two ingenious Chymiſts, Mr. *George* and Mr. *Francis Moult*;

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and where they made ſuch large *Apparatus* for evaporating the Water, that they have ſometimes boyl'd down 200 Barrels in a Week, from which, in a dry Seaſon, and when the Land Waters did not get into their Drains, they have obtain'd 224 Pound of Salt.

After theſe Works had gone on ſome time, Dr. *Hoy* found out a more expeditious Way of making a purging Salt ſo nearly reſembling that from the purging Springs, in all its Properties, that it ſoon paſs'd on the World for the other, and continued ſo to do.

The great Conſumption of theſe Salts (which then went only by the Name of *Epſom Salts*) as well at Home as Abroad, engaged ſome of our Phyſicians, (many Years before M. *Bolduc* took Notice of it) to ſuſpect, that even what was made at *Shooters-Hill* was ſpurious, and received an Addition of ſomething to encreaſe the Quantity. But theſe Suſpicions, I dare poſitively affirm, were entirely groundleſs, as to the Salts made there, and readily believe the ſame of any other Place, where the Spring Waters were boyled down for Salt. But upon a Conſideration, that there were greater Quantities of this Salt conſumed than all the Places where the Waters were boyl'd could produce, which was the real Fact at that Time of Day, there was ſufficient Room to ſuſpect, that ſome of them were not genuine, as appear'd to be true ſometime after.

For the Secret, which was then in a few Hands, of making theſe Salts cheap, gave thoſe, who had it, an Opportunity of under-ſelling thoſe who made it from the Waters, and, in a Year or two, render'd them incapable of making it to any Advantage, conſidering the Price it was ſold for by the others: So that the Work on *Shooters-Hill* was thrown up, and I believe there has not been 100 Pound of Salt made from the Waters ſince that Time, in any Part of the Kingdom.

Some time before this Work at *Shooters-Hill* was broke up, ſome Pains was taken to diſcover the Secret thoſe had, who ſold the Salt ſo cheap; and upon examining the ſeveral Salts, that were ſold about Town, thoſe diſpos'd of by Mr. *George* and Mr. *Francis Moult*, were certainly genuine, and were therefore a proper Standard to judge of the reſt by. But from all the Experiments then made, there could no material Difference be found between the Salt made from the Waters, and that made by them who were in the Secret. There was indeed a Salt ſold by ſome, which, in the Courſe of thoſe Tryals, was found to be a *Sal Mirabile*, made from the *Ol. Vitrioli* and common Salt, but ſhot into ſuch ſmall Cryſtals, as not at firſt Sight to be diſtinguiſhed from the other.

Neceſſity being the Mother of Invention, it was not long before it was diſcovered, and the Experiment was try'd at the Lady *Carrington*'s Salt-Works near *Portſmouth*; where it was found the ſame Thing could be done, as at another Work, not far from it, and in which Dr. *Hoy* had been concerned.

It was ſome Years after this Salt had been made at *Portſmouth*, before the Saltmakers at *Lemington* attempted, or indeed knew the Method of making it; who are now the greateſt Traders in it, and have ſent ſeveral Ton in a Year to *London*, beſides what has been directly exported from thence.

I remember it to have been the Opinion of the Proprietors of the Salterns near *Portſmouth*, that this purging Salt could not be made at any other Salt-Works except theirs, and that the bitter Taſte in the Salt was communicated from the Earth to the Sea Water, whilſt it ſtood expoſed in their Sun Pans. But Time has prov'd this Opinion falſe; for beſides what has been ſaid of its being made at *Lemington*, it was about 4 or 5 Years ago begun to be made near *Newcaſtle*, where it is ſtill continued to be made; and doubtleſs may be made at any other Salt-Works, where the common Salt is made from Sea Water by Evaporation. Whether any Thing of this Kind has been attempted at any of our Inland Salt Springs, either in *Cheſhire*, or *Worceſterſhire*, I am not yet ſatisfied.

There is ſome Difference in the making the common Salt in *Hampſhire*, from that about *Newcaſtle:* At the firſt of theſe Places, in the Beginning of the Summer, at Spring Tides, or at New and Full Moon, the Sea Water is let into their feeding Ponds, which are their *Reſervoirs* for their Summer's Working, and from hence is conveyed into ſmall ſquare Pans, and again, after ſome time, from theſe it is convey'd into larger Pans, or Beds, which they call Brine, or Sun Pans; all which are made of Sea Mud and Earth. In theſe laſt Pans, or Beds, it lies expos'd to the Sun and Wind, in order to exhale the weakeſt Waters; and it is in theſe Beds, if the Weather prove very favourable, that they can make as good Bay Salt as any we have from *France*, and at ſuch a time they never bring their Brine to the Boilers. But if the Weather is not hot enough for that Purpoſe, their Brine is expos'd ſo long in theſe Pans, till it becomes of ſuch a Strength as to ſupport their Eggs, made of Glaſs or Wax, to a certain Height above the Surface of the Brine, which from thence is convey'd into large Store Ciſterns, and then into Boiling Pans made of Iron, where it is boyl'd down (after having been frequently ſcumm'd) to a Sea Salt. 'Tis obſervable, that whilſt the Brine is boyling, there precipitates a hard cruſty Matter, which is partly taken out by Veſſels placed in proper Parts of the Pan for that Purpoſe, and Part of it fixes on the Bottom of the Iron Pan ſo hard, as to be afterwards dug off; and this the Workmen call *Scratch*, and is what Dr. *Collins*, in a former Tranſaction concerning the Sea Water boyl'd at *Sheilds*, calls a Stone Powder. When the Operation for the Sea Salt is finiſh'd, it is taken out hot, and put into wooden Troughs with Holes at the Bottom, through which runs the ſuperfluous Liquor: Under theſe Troughs are ſet other Veſſels (with Sticks fixed in them in a perpendicular Poſture) to receive what runs through. In theſe Veſſels the Liquor is ſuffer'd to continue ſome time, and according to the Quantity of Sea Salt ſtill left in it, will cryſtalize to the Sticks, ſomething like Sugar-candy, but in much larger Shoots; and this they call *Cat-Salt*, or *Salt-Cats*, and it holds ſome ſhare of the bitter Salt. When this Salt is broke ſmall, or rather powder'd, it is ſo white, that ſome Gentlemen chooſe it for their Tables; but the greateſt Conſumption of it is among the Cake Soap-Boylers. The Liquor that will not ſhoot to theſe Sticks, is what at theſe Works, they call the *Bittern*, fit for making the *Sal Catharticum*.

The Remainder of this curious Paper ſhall be communicated in our next.

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PHILOSOPHICAL TRANSACTIONS.

For the Months of *July* and *Auguſt*. 1723.

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I. *Florum Geometricorum Manipulus Regiæ Societati exhibitus à* D. Guidone Grandi *Abbate* Camaldulenſi, Piſani *Lycæi Mathematico, R. S. S.*

SUOS Geometria hortos habet, in quibus æmula (an potius magiſtra?) naturæ ludere ſolet, ſua ipſius manu flores elegantiſſimos ſerens, irrigans, enutriens, quorum contemplatione cultores ſuos quandoque recreat, ac ſumma voluptate perfundit. Hos inter bonis avibus & ipſe quondam admiſſus, nonnullos decerpſi flores, vario frondium numero coronatos, quandoque & infinitis foliis ſibi per innumeros gyros circumpoſitis elegantiſſimè compactos, quorum exiguum hunc ſaltem manipulum vobis, Viri Clariſſimi, offere ſtatui, ut meum vobis obſequium aliquo argumento teſtatum facerem. An naturæ induſtria ſimili fortaſſe artificio florum, fruticum, arborum folia conſtruere ſatagat, tali proportione ſucci nutritii motum temperans, ac dirigens, ut eadem frondium figura dimanet, quales in variis ejuſmodi florum geometricorum foliis, juxta varias leges, quibus deſcribuntur. obſervare licet, Philoſophis diſcutiendum, ac decidendum relinquo, præcipuè verò ſolertiſſimis naturæ indagatoribus, qui magni *Neutoni* exemplo naturales leges ex profundioris Geometriæ principiis repetendas ſibi meritò perſuadent, quibus utique illuſtriſſimus cætus veſter, præ aliis maximè abundat. Valete.

DEFINITIONES.

I. *Flores Geometricos* generatim appello quaſlibet figuras curvâ quadam per aliquot foliorum, ſeſe ab uno centro expandentium, perimetrum recurrente circumſcriptas, quales exhibent Figuræ 1, 2, 3, 4, 5; quos quidem flores, pro numero foliorum, bifolios, trifolios, tetrafolios, pentafolios, exafolios, &c. licebit nuncupare.

II. Cùm porrò innumeris modis ejuſmodi flores generari poſſint, eam geneſim hic ſpeciatim conſideramus, quæ per ramos a centro floris prodeuntes, æquales verò ſinubus angulorum, iis angulis, quos cum data poſitione linea rami comprehendunt, in data aliqua ratione proportionalium, procedit: cujuſmodi curvas *Rhodoneas* dudum appellavimus, eamque proportionem *Rhodoneæ* cuilibet propriam dicimus.

III. *Rhodoneam ſimplicem* appellamus, quæ una circulatione perficitur, *duplicem* quæ duplici, *triplicem* quæ triplici, & ſic deinceps pro numero circulationum.

Itaque ad Rhodonearum deſcriptionem aſſumpto quolibet circulo, cujus centrum C (*Vid. Fig.* 6, *&* 7.) & ducto ubilibet radio C D ad radium poſitione datum C A utcunque inclinato, ſit angulus A C D ad angulum A C G (ſive arcus A D ad arcum A G) in data ratione *a* ad *b*, ductoque ſinu G H, fiat C I æqualis G H; erit punctum I ad Rhodoneam ſupra definitam.

Ejuſmodi Rhodonearum proprietates præcipuas enucleabimus, nec non ſpatia, & perimetros breviter dimetiemur ſequentibus propoſitionibus.

PRO-

PROPOSITIO I.

Si fuerit arcus E A ad quadrantem A F (ſive angulus E C A ad rectum) ut *a* ad *b*, erit E C unus e maximis ramis Rhodoneæ, ſive erit E apex unius ex ejus foliis. (*Vid. Fig.* 6, *&* 7.)

Nam ex deſcriptione patet, ponendum eſſe ramum C E æqualem F C ſinui quadrantis A F, qui omnium ſinuum eſt maximus.

PROPOSITIO II.

Quodlibet folium Rhodoneæ circa axem C E hinc inde æquali, uniformi, & ſimili expanſione ſpargitur.

Factis enim hinc inde æqualibus angulis E C M, E C D, ob arcus æquales interceptos E M, E D, ſi fuerit arcus A M ad A N, ut A E ad A F, ut A D ad A G, nempe in data ratione *a* ad *b*, etiam reſidua E M, F N, itemque E D, F G in eadem ratione erunt, adeoque cùm antecedentia E M, E D æqualia ſint, etiam conſequentia F N, F G invicem æquabuntur, uti & reſidua ad quadrantes N K, G A, quorum ſinubus cùm æquari debeant rami Rhodoneæ C L, C I, & ipſi æquales erunt; quare ab axe C E hinc inde æquali, & uniformi expanſione ſpargitur quodlibet folium Rhodoneæ. Quod erat, &c.

COROLLARIA.

I. Ob æquales arcus E M, E D fit A E medius Arithmeticus inter A M, A D, qui intercipiunt æquales ramos Rhodoneæ; ideoque horum ſumma illius duplum adæquat, ſive æquatur toti A E P arcui ſectoris circumſcribentis unum Rhodoneæ folium.

II. Hinc etiam arcus M P æquatur A D.

III. Et eorundem arcuum A M, A D ſumma ad ſemiperipheriam A N K eſt in data ratione a ad b, quam habet A E ad quadrantem A F.

IV. Et ſector A P C Rhodoneæ circumſcriptus, eſt ad ſemicirculum in eadem data ratione a ad b, quam habet arcus A P, ſive ſumma duorum A M, A D ad ſemiperipheriam A N K.

PROPOSITIO III.

Numerus foliorum, quibus integra Rhodonea ſimplex compingitur, eſt ad unitatem, ut 2 b ad a.

Tot enim folia emergunt ex hac deſcriptione, quot ſectores unicuique folio circumſcripti, intra circulum diſponi poſſunt; ſed quilibet ſector eſt ad ſemicirculum, ex *Coroll.* 3. *præced.* ut a ad b, adeoque ad circulum ut a ad 2 b, quare numerus foliorum in una circulatione eſt ad unitatem ut 2 b ad a. Quod erat, &c.

COROLLARIA.

I. Hinc Rhodoneam ſimplicem deſcribere poſſumus, quæ datum foliorum numerum m, puta ſex, complectatur, ſi nempe pro ratione a ad b aſſumatur ratio 1 ad $\frac{m}{2}$ (in caſu propoſito 1 ad 3) quomodo erit 2 b ad a, ut m ad 1 (in propoſito ut 6 ad 1) adeoque prodibit datus foliorum numerus m.

II. Sed & Rhodoneam duplicem, triplicem, quadruplicem, &c. eadem arte componemus, dato foliorum numero in ſe recurrentem, ſi nimirum pro Rhodonea duplici ſumatur ratio 1 ad $\frac{m}{4}$, exiſtente dato numero m impari, alias prodiret Rhodonea ſimplex ſubduplo foliorum numero, quæ in ſecunda circulatione ſibimet ſuperponeretur, per eadem foliorum veſtigia recurrens. Pro Rhodonea triplici ratio 1 ad $\frac{m}{6}$, dummodo numerus m non ſit per 3 diviſibilis, alias iterum ſimplex Rhodonea prodiret ſubtriplo foliorum numero contenta. Similiter pro quadruplici Rhodonea ratio 1 ad $\frac{m}{8}$ inſerviet, dumodo numerus m ſit impar, alias Rhodonea ſimplex, aut duplex, ut antea oriretur; oportet enim in prima circulatione reſpectu Rhodoneæ duplicis haberi integrum aliquem foliorum numerum cum $\frac{1}{2}$ alterius folii, reſpectu triplicis cum $\frac{1}{3}$, vel $\frac{2}{3}$ folii, reſpectu quadruplicis cum $\frac{1}{4}$, vel $\frac{3}{4}$ alterius folii atque ita pariformiter in aliis.

PROPOSITIO IV.

Si ratio a ad b non ſit numeris effabilis, ſed arcus D A, G A ſint incommenſurabiles, innumera folia ſibimet per infinitas circulationes advoluta circumponentur.

Quælibet enim circulatio, præter certum foliorum integrorum numerum, partem folii ſuo toti incommenſurabilem comprehendet, nec unquam ad idem punctum deſcriptio revertetur, adeò ut æquatio ejuſmodi curvæ infinitorum ſit graduum. (*Vid. Fig.* 5.)

PROPOSITIO V.

At ſi ratio a ad b fuerit dupla, prodibit Rhodonea unifolia.

Nam ex *Prop.* 4. multitudo foliorum eſt ad unitatem ut 2 *b* ad *a*; ſed in hoc caſu *a* eſt 2, & *b* eſt 1, quare multitudo foliorum eſt ad unitatem ut 2 ad 2, ſive ut 1 ad 1; adeoque numerus foliorum eſt unitas. Et ſane arcus E A, qui ſit ad quadrantem A F ut *a* ad *b*, nempe in ratione dupla, eſt ſemiperipheria, adeoque ſemicirculus eſt ſector A F E circumſcriptus ſemifolio, cujus axis E C ex *Prop. prima*, ideoque integro folio circulus integer circumſcribitur.

COROLLARIA.

I. Facilis eſt hujuſmodi Rhodoneæ unifoliæ deſcriptio, ſi ſuper radio E C deſcribatur ſemicirculus, & ducta chorda E S D, in radio C D ponatur C I æqualis intervallo C S; nam cum C S ſit ſinus anguli C E S ad radium C E computatus, ejuſque anguli duplus ſit A C D, erit ramus C I ad Rhodoneam rationis duplæ, juxta geneſim præmiſſam.

II. Unde etiam, ſi centro C, quolibet intervallo C S, in dicto ſemicirculo arcus P S deſcribatur, & tantundem extendatur in I, ut ſint arcus P S, S I æquales, erit punctum I ad Rhodoneam; quippe C S perpendicularis chordæ E D bifariam ſecat in præcedenti deſcriptione angulum E C D; cumque ſit C M æqualis C S, punctum I eſt in arcu circulari, centro C per I, & S tranſeunte, qui continuatus in P remanet bifariam ſectus in S.

III. Et hinc patet, hanc Rhodoneam duplam eſſe circuli ſuper diametro E C deſcripti, ob quoslibet arcus I S P duplos ipſorum S P, indeque dimidiam circumſcripti circuli, cujus diameter E A; id, quod conſonat infra generaliter demonſtrandis *Prop. octava*.

PROPOSITIO VI.

Ubi ratio *a* ad *b* eſt æqualitatis, efficitur Rhodonea bifolia, quæ nihil aliud eſt, quàm duplex circulus ſubduplæ diametri ad diametrum circuli, qui Rhodoneæ circumſcribitur. (*Vid. Fig.* 9.)

Nam ratio 2 *b* ad *a* erit ratio dupla, ergo ex *Prop. quarta* multitudo foliorum dupla erit unitatis: & ſane deſcripto circa radium F C, velut diametrum, ſemicirculo, quoniam ramus Rhodoneæ C I debet in hoc caſu æquari ſinui ipſiuſmet arcus A D, utique punctum I ad peripheriam dicti ſemircirculi pertinget, adeoque duplex circulus, circa radios F C, C V, velut diametros, deſcriptus, erit locus talium ramorum, id eſt, Rhodoneam ipſam bifoliam conſtituet.

COROLLARIUM.

Etiam hic conſtat Rhodoneam bifoliam dimidiam eſſe circuli circumſcripti, atque adeo æqualem unifoliæ Rhodoneæ præcedentis propoſitionis.

PROPOSITIO VII.

Quodlibet folium Rhodoneæ eſt ad quadrantem circularem ut *a* ad *b*.

Ductis enim radiis infinitè proximis C I D, C *i d*, & ductis ſinubus G H, *g h* correſpondentibus, nempe æquantibus (*Vid. Fig.* 10, *&* 11.) ramos interceptos C I, C *i*, deſcriptoque concentrico arcu I R, patet fore elementum C I *i* ſemifolii Rhodoneæ ad elementum G H *h g* quadrantis, ut $\frac{1}{2}$ arcus I R ad H *h*, eo quod baſes C *i*, *g h* trianguli elementaris C *i* I, & rectanguli elementaris *g h* H G æquentur; ergo duplum ipſius C I *i* ad G H

bg eſt ut integra R I ad H b, nempe in ratione compoſita ex R I ad D d, & D d ad G g, & G g ad H b; ſed quia G g ad H b (ex theoria infinitè parvorum) eſt ut radius C g ad ſinum g b, nempe ut C D ad C I, vel D d ad R I, ratio G g ad H b elidit æqualem ſibi reciprocam R I ad D d; quare ſupereſt, ut ratio R I ad H b eadem ſit, quæ D d ad G g; ſed hæc eadem eſt quæ a ad b, cum in tali ratione ſint, tam A D ad A G, quam A d ad A g, adeoque & reſidua eandem rationem ſervent; ergo R I ad H b, ſive duplum elementare ſpatium C I i ad elementum quadrantis G H b g, eſt in dicta ratione a ad b, & hoc ſemper; igitur duplum ſemifolii C I E, nempe integrum folium Rhodoneæ, eſt ad quadrantem, ut a ad b; Quod erat, *&c.*

COROLLARIA.

I. Hinc ſemifolium C I E ad quadrantem eſt ut $\frac{1}{2}$ a ad b, (ſive ut a ad 2 b.

Item ſegmentum Rhodoneæ C I i ad ſemiſegmentum circuli A g b eſt in eadem ratione a ad 2 b.

PROPOSITIO VIII.

Quodlibet folium Rhodoneæ medietas eſt ſectoris circularis ſibi circumſcripti, & integra Rhodonea ſimplex medietas circuli, duplex duorum, triplex trium circulorum, &c.

Nam ex *præcedente* quodlibet folium eſt ad quadrantem ut a ad b, ideoque ad ſemicirculum ut a ad 2 b, ſed ex *Coroll.* 4. *Prop.* 2. ſemicirculus ad ſectorem folio circumſcriptum eſt ut b ad a; ergo ex æquo perturbatè quodlibet folium eſt ad circumſcriptum ſectorem, ut b ad 2 b, ſcilicet in ratione ſubdupla; quare & omnia folia Rhodoneæ ad omnes circumſcriptos ſectores, id eſt Rhodonea ſimplex ad circulum, duplex ad duos circulos, triplex ad tres, &c. in eadem ſubdupla ratione erit.

Aliter. Numerus foliorum ex *Prop.* 3, eſt ad unitatem, ideoque Rhodonea ipſa ad unum folium (ſi eſt ſimplex) ut 2 *b* ad *a*; ſed folium eſt ad quadrantem circuli, *ex præc.* ut *a* ad *b*, ergo Rhodonea ſimplex eſt ad quadrantem circuli ut 2 *b* ad *b*, ſcilicet in ratione dupla; quare ſimplex Rhodonea æquatur ſemicirculo. Similis diſcurſus Rhodoneis duplicibus, & triplicibus applicari poteſt; nam in illis numerus foliorum eſt ad unitatem ut 4 *b* ad *a*, in his verò ut 6 *b* ad *a*, &c.

COROLLARIA.

I. Quælibet Rhodonea ſimplex cuilibet ſimplici Rhodoneæ eidem circulo inſcriptæ æqualis eſt, quocunque foliorum numero conſtet, ſemper enim æqualis eſt ſpatio ejuſdem ſemicirculi.

II. Item quælibet Rhodonea duplex cuilibet duplici, & quælibet triplex cuivis triplici æqualis eſt, ob eandem rationem; quippe illa ſpecies eſt ſemper circulo æqualis, hæc ſeſquicirculo; & ſic de aliis. Oportet autem in duplici, aut triplici Rhodonea computare ſpatia foliorum, quæ ſibi ſuperponuntur, tanquam diſtincta eſſent.

PROPOSITIO IX.

Bifariam ſecto angulo E C A, quem axis folii Rhodoneæ cum tangente C A continet, per rectam C D, & ramo C I deſcripto arcu circulari I S T, erit lunula T E I quadrabilis, nempe ad quadratum radii, ut *a* ad 4 *b*. (*Vid. Fig.* 12.)

Cùm ſit enim quadrans F A ad A E, ut A G ad A D, qui eſt ipſius A E ſemiſſis, erit A G medietas quadrantis, ergo quadratum radii C G, vel C D, duplum eſt quadrati ſinus G H, ſive rami C I; ideoque ſector S C I ad ſectorem E C D ſimilem, ut 1 ad 2; ſector vero E C D ad F C G eſt ut a ad b; hæc enim eſt ratio arcuum E D, G F, ut eadem eſt integrorum E A, F A, & ablatorum A D, A G; ergo ex æquo ſector S C I ad ſectorem F C G erit ut a ad $2b$, nempe ut ſemifolium C I E ad quadrantem F G A C, vel ut ſegmentum Rhodoneæ C I ad ſegmentum A G H, vel ut reſiduum C E I C ad reſiduum F G H C, quare etiam reliquum ſemifolii S E I eſt ad reliquum triangulum C H G, aut tota lunula ad quadratum C H G P, in eadem ratione a ad $2b$, & ad quadratum radii C G, quod prædicti quadrati eſt duplum, erit ut a ad $4b$. Quod erat, &c.

COROLLARIA.

I. Cùm numerus foliorum Rhodoneæ ſimplicis ſit ad unitatem, adeoque etiam ſumma omnium lunularum, quas integra peripheria radio C T deſcripta abſcindit, ad unam lunulam T E I, ut $2b$ ad a; ipſa vero lunula ad quadratum radii ut a ad $4b$, patet eſſe ſummam dictarum lunularum ad quadratum radii ut $2b$ ad $4b$, nempe ſubduplam; hoc eſt ſummam talium lunularum æquare quadratum ipſum G H C P quadranti inſcriptum.

II. Unde ſumma lunularum, ex una Rhodonea per dictam peripheriam abſciſſarum, æquatur ſummæ lunularum ex qualibet alia Rhodonea, quotcunque foliorum fuerit, eidem circulo inſcripta ſimiliter determinatarum.

III. Cum

III. Cùm ejuſdem ſectoris E C A medietas ſit tam ſemifolium E I C, quàm ſector E C D, vel E D A, nec non ſector C S V, fiunt ſegmentum C I æquale trilineo E I D, & ſemilunula E S I trilineo C I V æqualis, quod propterea erit pariter quadrabile, utpote ad triangulum C G H in data ratione *a* ad 2 *b*.

IV. Et ſumma horum trilineorum in qualibet Rhodonea pariter ejuſdem erit quantitatis, utpote ſummæ lunularum ejuſdem, vel cujuſcunque alterius Rhodoneæ ſimplicis eidem circulo inſcriptæ ſemper æqualis.

V. Adeoque ſi illa triangularia foliorum Rhodoneæ interſtitia pro foliis computentur, flos inde totidem foliorum perfectè quadrabilis exurget, ut in *Fig.* 13.

PROPOSITIO X.

Ad quodlibet Rhodoneæ punctum I tangentem ducere.

Factum jam ſit; ductaque ramo I C (*Fig.* 14, 15.) perpendicularis C M, conveniat cum tangente I M in M; & radio C I arcus I R infinitè parvus deſcribatur uſque ad alium ramum C *i* infinitè proximum, ſintque ramis C I, C *i* æquales ſinus G H, *g b*, & circuli tangens G L occurrat diametro in L. Erit ergo I C ad C M ut *i* R ad R I, nempe in ratione compoſita ex *i* R, ſeu *g* O, ad O G (hoc eſt *g b*, vel *i* C, ad *b* L) & O G, ſive H *b*, ad R I (quæ ex *Prop.* 7. eſt eadem rationi *b* ad *a*) quare *i* C ad C M erit in ratione compoſita ex *i* C ad *b* L, & ex *b* ad *a*; ſed eadem ratio *i* C ad C M componitur quoque ex *i* C ad *b* L, & *b* L ad C M; ergo oportet rationem *b* L, ſive H L, ad C M eſſe datam, ſcilicet eam, quæ *b* ad *a*, ideoque ſi fiat, ut *b* ad *a*, ita ſubtangens circuli H L ad

CM ramo CI perpendicularem, juncta MI erit tangens Rhodoneæ in puncto I; Quod erat faciendum.

COROLLARIA.

I. Si fiat ut *a* ad *b*, ita CH ad CN ramo perpendicularem, juncta NI erit curvæ Rhodoneæ normalis; nam quia HL ad CM eſt ut *b* ad *a*, & CH ad CN ut *a* ad *b*, erit HL ad CM ut reciprocè CN ad CH; & ideo rectangulum M·CN æquabitur rectangulo LHC, id eſt, quadrato GH, vel quadrato rami CI; ergo juncta NI eſt tangenti MI, ſeu curvæ Rhodoneæ in puncto I, perpendicularis.

II. Patet, tangentes angulorum CIM, & LGH, vel GCA ſemper eſſe in data ratione *a* ad *b*.

PROPOSITIO XI.

Si fiat ut *b* ad *a*, ita radius AC ad CQ, & ſemiaxibus FC, CQ deſcribatur quadrans ellipſis FVQ, erit ejus perimetur æqualis perimetro ſemifolii Rhodoneæ ECI, & partes partibus correſpondentibus. (*Vid. Fig.* 16, *&* 17.)

Erit enim ubique etiam GP ad VP, vel *gp* ad *up* in eadem ratione, quæ eſt AC ad CQ, id eſt, *b* ad *a*; quare & reſidua GO, VX in eadem ratione erunt. Quod ſi infinitè proximæ ſint PG, *pg*, GH, *gh*, & correſpondentes CI, C*i* cum arcu infinitè parvo IR, quoniam IR ad H*h*, vel GO ex *Prop.* 7. eſt ut *a* ad *b*, in qua etiam ratione erit VX ad candem GO, patet ipſas IR, VX æquales fore; cum ergo & ſint æquales R*i*, VX (ob æqualitatem quarumvis CI, GH, vel TV, nec non C*i*, *gh*, *tu*) patet ſubtenſas quoque I*i*, V*u* æquales futuras. Singula igitur elementa, tum curvæ Rhodoneæ E I C, tum ellipticæ F V Q invicem æquantur; quare & perimeter ſemifolii Rhodoneæ erit quadranti curvæ ellipticæ æqualis, & duo quælibet folia perimetrum habebunt integræ curvæ ellipſeos æqualem; Quod erat, &c.

COROLLARIA.

I. Patet, Rhodoneam eſſe ellipſim quandam contractam; nam ſi confluentibus in centrum C punctis T, *t*, ordinatæ elliptici quadrantis V T, *u t*, in ramos abeant a centro C diductos, quadrans ellipſis in ſemifolium Rhodoneæ contrahetur, eadem curvæ longitudine manente.

II. Hinc iterum patet, Rhodoneam eſſe medietatem ſectoris circularis circumſcripti; eſt enim ſemifolium E I C medietas quadrantis elliptici F V Q C, in quem expanderetur, ſi rami ab eorum centro diſſoluti fierent paralleli, & rectæ C Q perpendiculares; cumque quadrans ellipſis ſit ad quadrantem circularem, ut baſis Q C ad baſim C A, nempe ut *a* ad *b*, in qua etiam ratione eſt ſector E C A ad eundem quadrantem, ex *Prop. prima*, patet, ejuſmodi ſectorem æquari quadranti elliptico, ideoque duplum eſſe inſcripti folii Rhodoneæ.

III. Inſuper colligitur, æquales eſſe foliorum perimetros in Rhodoneis, quarum ratio ſit reciproca, & radii ſuorum circulorum in eadem reciproca ratione ſibi reſpondeant; nam ſi radius C F, vel E C *Figuræ* 17. æquaretur baſi ellipſis C Q *Figuræ* 16, & viciſſim radius C F iſtius æquaret baſim C Q ellipſis alterius *Figuræ*, patet, eandem ellipſim F V utrobique reſultare debere, quippe iiſdem ſemiaxibus deſcriptam, eamque fore utrivis folio iſoperimetram, exiſtente ibi ratione *a* ad *b*,

hic

hinc reciprocè b ad a. Exempli cauſa, ſi ratio a ad b ſit ſubdupla, ut juxta *Prop.* 3. hinc proveniat Rhodonea tetrafolia, radio autem ſubduplo (adeoque æquali baſi quadrantis ellipſis iſoperimetræ) viciſſim fiat Rhodonea juxta rationem duplam, quæ ex *Prop.* 5. unifolia evadet, erit hæc iſoperimetra uni folio illius; nam baſis quadrantis elliptici huic reſpondens baſim habebit illius radio æqualem, adeoque eadem curva elliptica utrivis folio æqualis oſtenditur.

IV. Si verò in eodem circulo duæ Rhodoneæ deſcribantur, altera juxta rationem a ad b, altera juxta reciprocam b ad a, perimetros ſuarum foliorum habebunt ipſis rationum antecedentibus a, & b proportionales; nam ſi primæ Rhodoneæ tertia quædam Rhodonea ſimilis deſcriberetur in circulo, ad cujus radium prioris radius eſſet ut a ad b, eſſet perimetur primæ ad perimetrum tertiæ ſibi ſimilis in ipſa ratione radiorum a ad b. Verùm perimeter hujus tertiæ, ex *Coroll. præced.* æquaretur perimetro ſecundæ, utpotè reciproca ratione, & juxta reciprocos radios deſcriptæ, ergo perimeter primæ ad perimetrum ſecundæ eſt in eadem ratione a ad b.

PROPOSITIO XII.

Rhodoneam datæ rationis a ad b minoris inæqualitatis ex conica ſuperficie ſecare.

Fiat ut a ad b, ita radius baſis NB ad latus NC coni recti NCK, cujus baſis diametro NK ſit perpendicularis radius BF, (*Vid. Fig.* 18.) qui ſit ad BR ut b ad a, & circa diametros BR, BF deſcribantur ſemicirculi BLR, BSF, quos ſecet quilibet radius BG in punctis L, S, ſitque GH diametro NK perpendicularis. Si ſuper circulo BLR erecta ſuperficies cylindrica intelligatur ſecare conicam in communi ſectione

C I E, erit hæc (in planum explicata) ipſamet Rhodonea propoſitæ rationis. Nam communes ſectiones cylindricæ illius ſuperficiei cum planis triangulorum C B G, C B F per axem coni C B tranſeuntium, erunt rectæ L I, R E ipſi axi parallelæ, ideoque tam C I, ad B L, quàm C E ad B R erunt ut latus coni ad radium baſis, ſcilicet ut *b* ad *a* *ex conſtructione*, ſive ut F B ad B R, ſive S B ad B L; adeoque C E æquatur B F, & C I æquatur B S, ſive ſinui G H. Explicata autem ſuperficie conica in planum ſectorem circularem ipſi æqualem, radio C N deſcriptum, ejus angulus planus N C G ſubtendetur eodem arcu N G, ſubtendente in baſi coni angulum N B G; adeoque ut B N ad N C, ſive ut *a* ad *b*, ita erit angulus N C G ad ipſum N B G, cujus ſinui G H, ut vidimus, æquatur ramus C I folii C I E, cujus maximus ramus C E æquat radium B F circuli baſis; quare folium ipſum ad Rhodoneam pertinet in data ratione *a* ad *b* deſcriptam; Quod erat, &c.

COROLLARIA.

I. Cùm ſit etiam C E ad E O, ut C F ad F B, ut *b* ad *a*, ut F B ad B R ſintque C E, F B æquales, itidem æquales erunt B R, E O, & ſemicirculus B L R quarta pars erit ſemicirculi A E P duplum diametrum habentis, ſive erit medietas quadrantis A E O; eſt verò (ex noſtra Appendice de Fornicibus conicis, quam *Vivianeis* ſubjunximus jam inde ab anno 1698) ſuperficies conica A D E C ad ſuam baſim A D E O, ut ſuperficies ſemifolii C I E ad ſuam ichnographiam B L R, nempe in eadem ratione lateris coni ad radium baſis; ergo cum A D E O dupla ſit B L R, & ſuperficies A D E C ipſius ſemifolii C I E dupla erit, ut aliunde ſupra demonſtravimus ſectorem folio circumſcriptum illius duplum eſſe.

II. Cùm oſtenſum ſit eſſe angulum A C I ad N B G, uti & A C E ad N B F, in data ratione *a* ad *b*, patet etiam in eadem ratione eſſe angulum reliquum I C E ad reliquum S B F, exiſtente (ut probavimus) ramo C I æquali ipſi B S; (*Vid. Fig.* 19.) unde ſi ſemicirculi C S E, in arcus concentricos, centro C deſcriptos, reſoluti, arcus quilibet P S, *p s* dividantur ad puncta I, *i*, ut ſit ſemper P I ad P S, *p i* ad *p s* in data illa ratione *a* ad *b*, erunt puncta I, *i* ſic inventa ad curvam Rhodoneam.

III. Imo etſi ratio *a* ad *b* majoris ſit inæqualitatis, adhuc Rhodoneas ope ſemicirculi deſcribere licebit generalius quam in *Coroll.* 2. *Prop.* 5. ſi arcus P S, *p s* producantur ad puncta I, *i*, ut ſint P I ad P S, *p i* ad *p s* in data ratione *a* ad *b*. Facto enim arcu E A R ad quadrantem E A in eadem ratione, ductoque radio C R, fiet angulus R C E ad A C E, ut angulus I C E ad angulum S C E. adeoque & reliquus *i* C R ad reliquum *s* C A, cujus ſinus æquatur C *s*, ſive C *i*, in eadem ratione erit *a* ad *b*; ideoque puncta I, *i* ſunt ad Rhodoneam datæ rationis.

IV. Et ſi arcus illi P S, *p s* in ſemicirculo deſcripti, tum dividantur in ratione *a* ad *b*, tum augeantur in reciproca ratione *b* ad *a*, curvæ interioris longitudo ad longitudinem exterioris erit ut *a* ad *b*, per *Coroll.* 4. *Prop. præcedentis.*

SCHOLION.

Verùm hæc, pro inſtituto noſtro, circa hujuſmodi curvas delibaſſe ſufficiat: quanquam alia etiam Rhodonearum ſymptomata enucleare in promptu eſſet, uti & alias florum ſpecies diverſâ geneſi efformatas exhibere facilè foret, quorum etiam folia (ut poſtremâ propoſitione folia Rhodonearum circa conicam ſuperficiem advoluta dedimus) circa aliquam conoidalem ſuperficiem convoluta deſcribere poſſemus, & quandam foliorum in calice floris latentium imaginem adumbrare, niſi jam tædio Lectorum parcendum eſſet. Unum hoc admonere non prætermittam, quod ex ultimò propoſita generali foliorum Rhodoneæ deſcriptione ſimpliciſſima ex circulo derivata, ſuſpicari quis non immeritò poſſet etiam prima naturalium foliorum ſtamina, quæ in floris, aut fruticis ſemine latent, non neceſſariò ſimilia eſſe foliis ipſis conſpicuis, & jam germinantibus, ſive adultis; ſicut enim ſi florum, & fruticum folia noſtras Rhodoneas reipſa imitarentur, poſſet quis concipere, illorum prima ſtamina ſeminibus cujuſlibet ſpeciei incluſa ſimpliciſſima circulari figurâ infinitè parvâ circumſcribi, ſed mox peculiari vi cujuſlibet ſingularis ſpeciei, dum germinant, ita determinari ſuccum nutritium, ut dum in longum eorum axis extenditur, per quaſdam undas, ſive gyros, ipſi origini ſui pedunculi, velut centro, circumpoſitos, expandatur, eoſque ſemper in determinata ratione, vel arctiores, vel ampliores, quàm ſi circularis primorum ſtaminum figura retinenda eſſet: quo poſito talis ſpecies foliorum Rhodoneæ, ac talis numerus, & forma exurgeret, qualem ratio illa determinaret. Ita etiamſi alia lege florum, & fruticum frondes natura moliatur, non neceſſe eſt earum figuram, uſque ad ipſa prima earundem ſtamina, ex quibus germinant, obſervari; ſed illa in quibuſlibet unius certæ, ac determinatæ figuræ eſſe poſſet, quæ tantum pro diverſa vi, determinante in ipſis expanſionem ſucci nutritii, in ſingulis ſpeciebus varianda foret, juxta diverſam rationem, quæ per ipſorum ſtaminum fibras dirigeretur. Sed ne extra chorum ſaltemus, hæc Philoſophis innuiſſe ſufficiat.

II. *The remaining Part of the Obſervations and Experiments on the* Sal Catharticum Amarum, *commonly called the* Epſom Salt. *By Mr.* John Brown, *Chymiſt. F. R. S.*

NEAR *Newcaſtle*, their Method is, to receive the Sea Water into their *Reſervoirs* at High-Water, at any Time of the Moon, if there be no Freſh in the River, occaſioned by Rain in the higher Country; and from theſe *Reſervoirs*, without expoſing of it in Beds, as at *Lemington*, they pump it into their boyling Pans, where, evaporating it almoſt to a Pellicle, they fill it up again 8 or 9 Times, and then waſte it with a gentle Heat for the common or Sea Salt. The Liquor, that runs from this Salt, when taken out, and put into proper Veſſels, is what they call the *Bittern*; which if it ſtands ſome time in thoſe Veſſels, a Salt will ſhoot and cryſtalize to the Sides, in Taſte pretty much like Sea Salt, but with a ſhare of Bitterneſs, and ſeems to anſwer to the *Cat-Salt* of the *Lemington* Works, and very probably would ſhoot after the ſame Manner, if they made uſe of the ſame *Apparatus.*

I could not but mention this general and looſe Account of making the common Salt, as neceſſary to introduce the Liquor call'd *Bittern*; which, before Dr. *Hoy* found out an Uſe for it, was always flung away; being ſo different in its Properties from the Brine made Uſe of to produce the Sea Salt, that it would not boil up into a Sea Salt again, and required the niceſt Skill and Attendance of the Operator, to determine the Time when to take out the Sea Salt from the Pans, before the *Bittern* incorporated with it, which would otherwiſe ſpoil the whole Making.

The *Bittern* at *Lemington* (as obſerv'd before) not ſhooting to the Sticks, is carry'd by Channels into Pits made tight with Clay, where it ſtands for ſome Months, and there will ſhoot again: What Liquor remains is boyl'd down, till ſuch Time as it is obſerved to be in a Diſpoſition to cryſtalize, and then is convey'd into wooden Coolers lined with Lead: The Liquor, which will not ſhoot there, is boyl'd down after the ſame Manner, in order for another Cryſtalization. By this Time the Liquor ſeems to have alter'd its Property, and becomes of a very pungent biting Taſte, and, if boyl'd down, will no longer ſhoot into Cryſtals as before, but precipitates, during the Boiling, a ſmall grain'd Salt; and if you, for Experiment-ſake, ſhould continue to boil down the Liquor, ſeparated from this Salt, each Quantity of Salt thus produced, will ſtill be more pungent than the other. If you boil down the whole Quantity of this Liquor, it will produce a Salt, which if expos'd to the Air, will run, *per deliquium*. But as this Salt is not the Buſineſs of our preſent Enquiry, it may probably be the Subject of another Paper. The Liquor, that produces this Salt, is always flung away, wherever the *Sal Catharticum* is made.

This is what, at preſent, I can give no other Name to, than a third Salt produc'd from the Sea Water, differing, in ſome reſpects, as much from the other two, as they differ from one another.

To return to the ſeveral Cryſtalizations, ſuch as mention'd to be ſhot from the *Bittern*; theſe will be of different Sizes, as to their Figures, and hold ſome ſhare of the third Salt but now taken Notice of, which makes them apt to give and diſſolve; nor is their Taſte come yet to that ſimple Bitter of the pure Salt. Theſe therefore are either ſeparately, or altogether, to be flung into a Copper, with as much common Water as is ſufficient to diſſolve them, and allow of a gentle Evaporation, till ſuch Time as they are again ready to be pour'd into the Coolers in order for Cryſtalization. This generally proves to be the pure *Sal Catharticum*, throughly freed (as far as the Experiments I have try'd can be convincive) from either a Sea Salt, or the third Salt. The Liquor decanted from this Shooting, may be boil'd down again in order for a ſecond Shooting, and after that a third; but as the Liquors from theſe Shootings are boyl'd away more or leſs, ſo you will ſooner or later meet again with the pungent Liquor, which contains the third Salt, as you did in the former Shootings from the *Bittern*, which the pure *Sal Catharticum* is as neceſſarily required to be freed from, as from the common Salt; a Proof of which cannot be better determined than by one of the Experiments to be taken Notice of hereafter (*viz.*) that with the *Ol. Vitriol.* which will certainly ferment with this Salt, if the Sea Salt has not been well ſeparated from it, or if it ſtill holds ſome of the third Salt. And when any of the Cryſtalizations will not ſtand the Teſt of this Experiment, they ought to be diſſolv'd and ſhot again, as before, by which means the pure Salt is to be obtain'd. I do not mention this as a Tryal made uſe of at the Salt-Works, but what I have by Experience found to be true. And the ſame Experiment will ſerve to diſtinguiſh a *Sal Mirabile* made at theſe Works, from that made with *Ol. Vitrioli* and common Salt. The Account they give of it is this. They take any Quantity of coarſer grain'd Cryſtals boyl'd from the *Bittern*, which when diſſolv'd and evaporated, more than they would otherwiſe do for making the *Sal Catharticum*, they throw into a wooden Bowl, with ſome Oil of *Vitriol*, where it ſtands for ten Days, and ſhoots into large

Cryſtals, tranſparent, and like the *Sal Mirabile*: But as this Salt, by this Method, is not ſufficiently ſatiated with the *Ol. Vitriol.* (if they uſe any) ſo it is eaſily diſcovered by the *Ol. Vitriol.* which will readily ferment with it; whereas it has no Effect on the other *Sal Mirabile* made as above.

By the Aſſiſtance of my ingenious Friend. *Robert Cay*, Eſq; at *Newcaſtle*, I have received the ſeveral Shootings of Salts from their *Bittern*, as alſo ſome of the *Bittern* it ſelf; from each of which I have obtain'd a pure *Sal Catharticum*, as alſo the like kind of third Salt, as mention'd from the *Lemington Bittern*. The Method I took in doing it is agreeable to that I have already mention'd, and many Years ago try'd at the Salt-Works near *Portſmouth*. It is by Mr. *Cay* that I am inform'd, they ſometimes boyl their *Bittern* without letting it ſtand any time to ſhoot of itſelf. The Difference is not very material.

If this Account be intelligible, what the *Sal Catharticum* is will no longer be a Myſtery. And the next Thing worth the enquiring into will be, whether this Salt deſerves the Reflections, that have diſcouraged the Preſcription of it? And why it may not paſs for a Salt as excellent in its Kind, and be of the ſame Nature, and have the ſame Properties, as that produc'd from the *Epſom*, or any other bitter purging Springs.

And in order to prove it to be ſo, I ſhall give a very ſhort Abſtract of what Dr. *Grew* ſays of his Salt, and then obſerve how nearly the two Accounts agree.

The Doctor in his Treatiſe *de Natura Salis Cathartici Amari*, *Chap.* 2. ſays, that in the Evaporation of any of the bitter Purging Waters, they yield a Cremor at Top, as alſo a Sediment, both together weighing 6, 8, or about 10 Drams, from a Gallon of Water; and that the leſſer Part of this Sediment is, in Subſtance, the ſame with this Cremor; the reſt is all Salt, but conſiſts of two ſorts, one a muriatick Salt, the other which is proper or peculiar to theſe Waters.

In the *Epſom* Water, the muriatick Salt is about a 20th Part of the ſaline Mixture; in the *Dulwich* it is in a greater Proportion, and the ſame in ſeveral others; it is, both in its acrimonious Taſte and Figure of its Cryſtals, not unlike to common Salt. The other Salt is that which he ſays is particular or proper to the purging Waters, and is made by Evaporation and Cryſtalization. In this Preparation, firſt the earthy or plaiſtery Part is to be ſeparated, next the muriatick Salt, and, laſtly, a brown and dark Liquor from the proper Salt of the Waters.

And in the 4th Chapter of the ſame Part, having ſhewn the Difference of the Figure betwixt the Cryſtals of this Salt and thoſe of Alum, he goes on, Neither is there any better Ground to account the purging Salt a Species of common Salt, from which being perfectly freed, it differs as much in Taſte as from Alum. And in the ſame Chapter, he ſays it will appear, the bitter purging Salt, although it hath ſome Qualities in common with other Salts, yet is truly, or ſpecifically different from them all. Thus far Dr. *Grew*.

Now I cannot ſee any Thing in this Account, but what will, *conſideratis conſiderandis*, very well agree with the purging Salt from the Sea Water.

For firſt, there is an earthy or plaiſtery Part contain'd in theſe Waters, and this muſt be ſeparated. The very ſame is in the Sea Water, and is precipitated in the Boiling them down, as has been obſerv'd, and by the Operators is called *Scratch*.

Next there is a muriatick Salt allow'd to be in theſe Waters; in ſome more, in ſome leſs, and this is likewiſe to be ſeparated: The very ſame is done from the Sea Water, though in a vaſtly larger Proportion.

And, laſtly, there is a black and dark Liquor to be ſeparated; tho' this is but a dark Way which the Doctor makes uſe of to expreſs himſelf, it cannot be better explain'd, than by what has been found to be Fact in boiling down the Waters at *Shooters-Hill:* That after ſeveral Shootings of Salts by repeating the Boilings of the Waters, there would, at laſt, remain a Liquor of a deep brown Colour, which would no longer yield a cryſtaliz'd Salt; but if boil'd up dry, would afford a Salt of the ſame Kind with the third Salt already mention'd: And this explaining Dr. *Grew*'s black and dark Liquor, helps at the ſame time to prove, in this Article too, that the Sea Water affords the ſame kind of third Salt.

I have try'd ſeveral of the Experiments mention'd by the Doctor, by which he diſtinguiſhes his Salt from other Salts. Such as not affecting the Colour of Syrup of Violets; curdling of Milk when boil'd; in the Figure of its Cryſtals; in its eaſy Diſſolution in the ſame Quantity of Water; in its coagulating with the *Ol. Tartar. per D. L.* in its Calcination, and in the Bitterneſs of its Taſte, as well before as after Calcination, *&c.* and find this Salt, thus ſeparated from the Sea Water, anſwer to all the Tryals. Some few Experiments, that the Doctor has not taken Notice of, I ſhall here ſubjoin, and then leave the Whole to the Opinion of better Judges; Whether there be any ſpecifical Difference between theſe two Salts?

In order to have a Standard for theſe Experiments, I purpoſely got my Friend, Mr. *Hyet*, Apothecary at *Epſom* (whoſe Fidelity I could depend on) to boil me down ſome of their Waters, which he did from the

Well in the Town, and ſent me a ſufficient Quantity of the Salts, to anſwer the Purpoſe I wanted them for.

I procured likewiſe ſome of the firſt Salts from the *Lemington Bittern:* Theſe do not hold ſo much of (what I have already diſtinguiſhed by the Name of) the third Salt, as I find the *Newcaſtle* Salts do. This *Lemington* Salt, I, for Diſtinction ſake, call the *firſt Lemington Salt.*

Part of this I diſſolv'd, and ſhot into pure *Sal Catharticum*, being freed, as well from the Sea Salt, as the third Salt; and this I call the *ſecond Lemington* Salt.

I procured likewiſe from *Newcaſtle* the firſt Salts ſhot from their *Bittern*, which I call the *firſt Newcaſtle* Salt.

Part of theſe I likewiſe diſſolv'd and ſhot, and obtain'd a pure *Sal Catharticum*, and this is what I call the *ſecond Newcaſtle* Salt.

I am obliged to make Uſe of the *Sal Mirabile*, made from the *Ol. Vitriol.* and common Salt, that having been taken for the *Sal Catharticum.*

As alſo common Salt, that having been repreſented as the principal Subſtance of the *Sal Catharticum.*

I took half an Ounce of each of theſe Salts, and diſſolv'd them in about two Ounces of Water to each half Ounce of Salt.

A ſmall Quantity of each Diſſolution I pour'd into as many Glaſſes, and dropt into them all ſome *Butyr. Antimonii.* The Precipitation that follow'd, ſeem'd to be alike in them all; and upon dropping a little *Ol. Vitriol.* into each, what was precipitated being more powerfully attracted by the Oil, the ſeveral Liquors became clear. Theſe are the two only Experiments, in which I found the Conſequences ſo much alike in them all.

In the following Experiments, the *Sal Mirabile* is ſufficiently diſtinguiſh'd from all the reſt.

Slices of Gall cut into theſe ſeveral Solutions have no manner of Effect upon any, except that of the *Sal Mirabile*, which is ſoon ting'd of the Colour of Sack, or rather deeper.

Sp. Sal Arm. c. Tart. dropt into the ſeveral Solutions turns them all milky, except that of the *Sal Mirabile*, which keeps its Tranſparency.

The *Sp. Salis Armon. c. calce.* the *Ol. Tart. p. deliq.* the *Tinctura Coccinell in Sp. Vin. fact.* do every one, uſed after the ſame Manner, ſufficiently diſtinguiſh the *Sal Mirabile* from all the reſt.

In the following Experiments, the *Epſom* Salt, the *ſecond Lemington* Salt, and *ſecond Newcaſtle* Salt, agree together, and differ from the common Salt, the *firſt Lemington* Salt, and *firſt Newcaſtle* Salt.

In the ſeveral Solutions I dropt a Solution of Silver in *Aq. Fortis*, from which follow'd theſe Conſequences. The Solution of the *Epſom* Salt, *ſecond Lemington* Salt, and *ſecond Newcaſtle* Salt, became equally milky, before the Precipitation. The Solution of the Sea Salt, and *firſt Newcaſtle* Salt, let the Precipitation paſs without receiving any milky Tinge. The *firſt Lemington* Salt, as holding leſs of the *third* Salt, than the *firſt Newcaſtle* Salt did, took a little milky Tinge. The Precipitation fell nimbly thorough the Solution of the *Sal Mirabile*, leaving it milky.

In the Condition theſe were in, I pour'd ſome *Ol. Tartar. per deliq.* to each of them, on which, after ſome time, a blueiſh Scum aroſe on the Surfaces of the *Epſom* Salt, *ſecond Lemington* Salt, and *ſecond Newcaſtle* Salt: There likewiſe appear'd a little on the *firſt Lemington* Salt, but not any on the reſt.

A So-

A Solution of corroſive Sublimate was made in Water, ten Drops of which, mixed with the ſeveral Solutions, produced little or no Alteration; but upon dropping in the *Ol. Tartari per deliq.* the following Appearances were produc'd: In the Solution of the *Epſom* Salt, *ſecond Lemington* Salt, and *ſecond Newcaſtle* Salt, the Precipitations were red; in the Solution of the common Salt, and *firſt Newcaſtle* Salt, the Precipitations were white; in the Solution of the *firſt Lemington* Salt, the Particles precipitated approach'd pretty near the Colour of the three firſt.

I took ſome of theſe ſeveral Salts in Subſtance, and to each of them pour'd a little *Ol. Vitriol.* which is one of the Experiments Dr. *Grew* try'd upon his Salt, and which he ſays cauſes a moderate Ebullition, whereby it appears to partake of an alkaline Principle: But without looking for this alkaline Principle from its fermenting with an Acid, (Terms juſtly exploded by the learned Dr. *Freind* in his *Prælectiones Chymicæ*) I am inclin'd to believe, that the Salt he try'd the Experiment on, had not, according to his own Directions, been thoroughly ſeparated from his muriatick Salt. For this Oil pour'd on the *Epſom* Salt, *ſecond Lemington* Salt, and *ſecond Newcaſtle* Salt, produc'd no ſenſible Fermentation. On the Sea Salt it acts with Violence, forcing off its acid Spirit with an inſufferable *Gas*. The ſame Effect in proportion it had on the *firſt Lemington* Salt, and *firſt Newcaſtle* Salt; none at all on the *Sal Mirabile*, as being a Sea Salt already ſatiated with the Oil.

What I have all along call'd the *third* Salt, anſwers in moſt of theſe Experiments to the Sea Salt, and yet has ſome Properties exceedingly different from it; to thoſe I have mention'd theſe may be added; it will not decrepitate like Sea Salt; it readily melts, when put in a Crucible in the Fire; and when calcin'd till red-hot, affords a *Calx* equal to, if not ſtronger than a Limeſtone, and ferments violently, as well with Water as with *Ol. Vitriol.* This *Calx*, when expos'd to a moiſt Air, will Part of it run *per deliq.* but not ſo ſoon as before Calcination. All theſe Properties differ in every Reſpect from the common Salt, and leave me ſtill in doubt what to call it, as alſo how far Experiments of this Kind may be deem'd concluſive.

III. *A Letter from the Rev. Mr.* James Pound, *Rector of* Wanſtead, *F. R. S. to Dr.* Jurin, *Secr. R. S. concerning Obſervations made with Mr.* Hadley's *Reflecting Teleſcope.*

IT were to be wiſh'd, that with the particular Deſcription given in a late Tranſaction (*Numb.* 376.) of the curious Mechaniſm of that Catadioptrick Teleſcope, which was made by Mr. *Hadley*, and by him preſented to the *Royal Society*; that moſt ingenious Gentleman would have communicated alſo a full Account of what Obſervations he had made with it, whereby the Publick might at length have been apprized of the Uſefulneſs of an Invention, (worthy of its great Author, Sir *Iſaac Newton*) which, perhaps from the vain Attempts made by ſome of putting it in Practice, hath lain neglected theſe 50 Years; for it is ſo long, ſince it was firſt publiſhed in the *Philoſophical Tranſactions*, *Numb.* 81.

Mr. *Hadley* hath ſufficiently convinced us, that this noble Invention doth not conſiſt in bare Theory; and it is to be hoped, that he, or ſome other ſuch curious and worthy Perſons, (who ſcruple not at a little Pains and Coſt) will in a ſhort Time find out a Method, either of preſerving the concave Metal from tarniſhing, or of clearing it eaſily when tarniſhed, or elſe of making a good concave *Speculum* of Glaſs quickſilver'd on the Back-part. When a Method for either of theſe ſhall be diſcovered, 'tis not to be doubted, but that the old *Dioptrick* Teleſcope will be for the moſt Part laid by, and this *Catoptrick* one will be chiefly in uſe among the practical Aſtronomers; inaſmuch as ſeveral Inconveniencies and Difficulties, which are unavoidable in the Management of the former, eſpecially when long, are in this latter wholly avoided.

It is no ſmall Convenience, that by means of one of theſe reflecting Teleſcopes, whoſe Length exceeds not five Feet, (and which may be managed at a Window within the Houſe) Cœleſtial Objects appear as much magnified, and as diſtinct, as they do through the common Teleſcope, of more than 100 Feet in Length.

Mr. *Bradley*, the *Savilian* Profeſſor of Aſtronomy, and myſelf, have compared Mr. *Hadley*'s Teleſcope (in which the focal Length of the Object Metal is not quite 5 Feet and $\frac{1}{4}$) with the *Hugenian* Teleſcope, the focal Length of whoſe Object Glaſs is 123 Feet: And we find, that the former will bear ſuch a Charge, as to make it magnify the Object as many Times as the latter with its due Charge; and that it repreſents Objects as diſtinct, though not altogether ſo clear and bright; which may be occaſioned partly from the Difference of their Apertures (that of the *Hugenian* being ſomewhat the larger) and partly from ſeveral little Spots in the concave Surface of the Object Metal, which did not admit of a good Poliſh.

Notwithſtanding this Difference in the Brightneſs of the Objects, we were able, with this reflecting Teleſcope, to ſee whatever we have hitherto diſcovered by the *Hugenian*; particularly the Tranſits of *Jupiter*'s Satellites, and their Shades, over the Diſk of *Jupiter*; the black Liſt in *Saturn*'s Ring; and the Edge of the

Shade of *Saturn* caſt on his Ring, as repreſented by *Fig.* 4. *Plate* 2. of the foremention'd *Tranſact. Numb.* 376.

We have alſo ſeen with it ſeveral Times the 5 Satellites of *Saturn*; in viewing of which this Teleſcope had the Advantage of the *Hugenian*, at that Time when we compared them; for it being in Summer, and the *Hugenian* Teleſcope being managed without a Tube, the Twilight prevented us from ſeeing in this ſome of thoſe ſmall Objects, which at the ſame Time we could diſcern with the reflecting Teleſcope.

I am, &c.

Ja. Pound.

IV. *Obſervations on the Satellites of* Jupiter *and* Saturn, *made with the ſame Teleſcope. By* John Hadley, *Eſq; F. R. S. Extracted from the Minutes of the* Royal Society, Apr. 6. 1721.

MR. *Hadley* gave the *Society* a Relation of ſome of the moſt remarkable Obſervations, which he had made with his Reflecting Teleſcope, before he preſented it to the *Society*.

In obſerving *Jupiter*'s Satellites he has ſeen diſtinctly the Shadows of the firſt and third Satellites caſt upon the Body of the Planet; Mr. *Folkes* and Dr. *Jurin*, being preſent, affirmed, that Mr. *Hadley* had likewiſe ſhewn them the Shadow of the third Satellite through the ſame Teleſcope.

In obſerving *Saturn* the laſt Spring, at a Time when that Planet was about 15 Days paſt the Oppoſition, he ſaw the Shade of the Planet caſt upon the Ring, and plainly diſcerned the Ring to be diſtinguiſhed into two Parts, by a dark Line, concentric to the Circumference of the Ring. The outer or upper Part of the Ring ſeemed to be narrower than the lower or inner Part, next the Body, and the dark Line, which ſeparated them, was ſtronger next the Body, and fainter on the outer Part towards the upper Edge of the Ring. Within the Ring he diſcerned two Belts, one of which croſs'd *Saturn* cloſe to its inner Edge, and ſeemed like the Shade of the Ring upon the Body of *Saturn*; but when he conſidered the Situation of the Sun, in reſpect to the Ring and *Saturn*, he found that Belt could not ariſe from ſuch a Cauſe.

He ſays, that at Times he has ſeen with this Teleſcope three different Satellites of *Saturn*, but could never have the Fortune to ſee all five.

Aug. 1723. Mr. *Hadley* adds, that he has ſeveral Times ſeen the Shadow of the firſt, ſecond, and third Satellites of *Jupiter* paſs over the Body of that Planet, and that he has ſeen the firſt and ſecond appear, as a bright Spot upon the Body of *Jupiter*, and has been able to keep Sight of them there for about a Quarter of an Hour, from the Time of their entring on his Limb.

Jupiter's Satellites have of late Years been ſo ſituated, with regard to the Earth and *Jupiter*, that he has not had ſufficient Opportunity of obſerving the Tranſit of the fourth Satellite, or of its Shadow.

The dark Line on the Ring of *Saturn*, parallel to its Circumference, is chiefly viſible on the *Anſæ*, or Extremities of the Elliptick Figure, in which the Ring appears; but he has ſeveral Times been able to trace it very near, if not quite round; particularly in *May*, 1722, he could diſcern it without the Northern Limb of *Saturn*, in that Part of the Ring, that appeared beyond the Globe of the Planet. The Globe of *Saturn* (at leaſt towards its Limb) reflects leſs Light than the inner Part of the Ring, and he has ſometimes diſtinguiſh'd it from the Ring by the Difference of Colour.

The duſky Line, which in 1720 he obſerved to accompany the inner Edge of the Ring croſs the Diſk, continues cloſe to the ſame, though the Breadth of the Ellipſe is conſiderably encreas'd ſince that Time.

V. *An Account of an Extra-Uterine Fœtus, taken out of a Woman after Death, that had continued five Years and an half in the Body. By* Robert Houſtoun, *M. D.*

I Was ſent for in *Auguſt* 1717, to a Woman near *Newport-Market*, who had been married eighteen Years to a Native of the *Eaſt Indies*, by whom ſhe had eight Children, beſides two Miſcarriages. At my viſiting her, now, ſhe was with Child in a ſecond Marriage, and her Husband a vigorous young Man.

She was near her full Time, and had felt Pains for ſeveral Days, which, returning by Intervals, ſhe concluded, would, as uſual, bring on her Delivery. Her Mother and her Midwife, apprehending no Difficulty, aſſured thoſe about her, that only Time was wanting.

But I found, on Examination, that her Womb was of no Bulk to contain a Child near its Time; and that its Neck, of an uncommon Hardneſs, was alſo clos'd ſo ſtraitly, as to refuſe the leaſt Admiſſion, even of a ſmall Probe or knitting Needle.

I declared upon this, that her Delivery was impoſſible; becauſe the Child was not within the Womb, but between the Womb and the Guts: That it might be remov'd by a Paſſage to be made for it, without any great Pain, and with Safety to the Mother. I offer'd to undertake it, and aſſur'd them, that this was the only Opportunity; and that if ſhe neglected it, it

would

would hereafter be out of the Power of Art to give her the Relief, ſhe muſt languiſh for till Death, unleſs favour'd by ſome unlikely and extraordinary Accident.

However confidently I affirm'd it, they liſtened with a Mixture of Disbelief and Amazement, and rejected my Aſſiſtance. At that Time, in Probability, it would have been ſucceſsful; for ſhe was a ſlender well-ſhap'd Woman, in good Habit of Body, and of a ſprightly Diſpoſition.

It was a Year after this, when Mrs. *Hexel*, a noted Midwife in St. *James*'s Pariſh, deſir'd me again to viſit her. I found her much diſorder'd by a growing Impoſthumation in her Belly: I order'd her ſome cordial Stomachicks, *Caſſia*, and ſuch gentle Lenitives; and they met a Succeſs beyond my Expectation: So that, by aid of a regular Diet, and the watchful Exactneſs of a very tender Mother (a Nurſe of above thirty Years Experience about this City) I reſtor'd her to ſuch Strength, that ſhe went chearfully Abroad, and re-apply'd herſelf to Buſineſs. I told her ſhe might frankly benefit herſelf by my Advice, or my Medicines: And ſhe call'd as ſhe found Occaſion.

But about fifteen Months from the Time when I viſited her firſt, her Mother came from her to entreat my Aſſiſtance: She complain'd of great Pain in the lower Part of her *Abdomen*; and I found a Tumour of a conick Form, projecting about an Inch beneath the *Umbilicus*: Its Inflammation, with Tenſion, and a Feveriſhneſs attending it, ſo plainly indicated Suppuratives, that I was not ſurpriz'd to hear, in a few Days, that it had broke, as I wiſh'd.

I propoſed to lay it open, both to give a free Emiſſion, and prevent its becoming fiſtulous; but ſhe was apprehenſive, that I would, as ſhe call'd it, cut open her Belly: So that not being able to prevail with her, I order'd a Pot of Unguent, and ſome Plaiſters.

The Ulcer ſoon grew fiſtulous, and ſo continued till ſhe dy'd, which was on the 23d of *March* laſt, in the 41ſt Year of her Age.

For above five Months before her Death, ſhe voided her Excrements by this Vent, and all the ſoft Parts of the *Fœtus*, with ſome ſmall Bones of its Fingers. But the reſt of the *Skeleton* remaining entire, I took it out of her Body, together with the *Vagina*, *Uterus*, *Rectum*, *&c.* wherein it had involv'd itſelf, as may be ſeen more particularly in the Figure annexed.

Explication of the Figures.

Fig. 21.

A The *Clitoris.*
B B The *Nymphæ.*
C The *Rima*, or Entry into the *Vagina.*
D The *Bladder.*
E E The *Ureters* cut off.
F The *Uterus* ſound and entire.
G The Left *Ovarium.*
H H The *Tubes.*
I Part of the *Colon* cut off.
K Part of the *Peritonæum.*
L Part of the *Ileum* cut off.
M Part of the *Cæcum.*
O Part of the *Cranium* that plainly appears.
P The Ulcer, through which the Excrements paſs'd, with ſome ſmall Bones.
Q Q Q Q The *Bulky Maſs*, wherein the *Skeleton* is contain'd, between the *Uterus*, part of the *Vagina*, and *Rectum.*
R R Part of the *Cutis.*

She was full nine Months gone in *Auguſt* 1717, and dy'd the 23d of *March* 1723, on which Day I took it out.

Fig. 22. *A Lateral View.*

A *Orificium Vaginæ.*
B *Anus.*
C *Inteſtinum Rectum.*
DDD The Maſs, wherein the *Skeleton* is contain'd.
E The *Coſtæ* plainly appearing.
F The Bladder laid aſide.
G The *Ureter* cut off.
H The *Uterus.*
I The *Ovary.*
K The *Tube.*
L Part of the *Colon.*
M The *Ulcer*, through which the Excrements paſs'd, about one Inch beneath the Navel.
N Bones in the Orifice of the *Ulcer.*

FINIS.

ERRATUM.

Numb. 376. *Pag.* 308. *l.* 7. *del.* ſquare.

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ADVERTISEMENT.

THE Practice of inoculating the SMALL POX being now extended into many Parts of the Kingdom, and it being highly requiſite that the Publick ſhould be faithfully inform'd of the Succeſs of that Method, whether Good or Bad; It is deſir'd, that all PHYSICIANS, SURGEONS, APOTHECARIES, and others therein concern'd, will be pleas'd to tranſmit to Dr. *Jurin*, Secretary to the ROYAL SOCIETY, a particular Account, ſpecifying the Name and Age of every Perſon by them inoculated, the Place where it was done, the Manner of the Operation, whether it took Effect or no, what Sort of Diſtemper it produced, on what Day from Inoculation the Eruption appear'd; and, laſtly, whether the Patient died or recover'd. They are deſired to comprehend in their Accounts all Perſons inoculated by them, from the Beginning of this Practice among us to the End of the preſent Year, and to ſend them ſome Time in *January* or *February* next.

IN caſe this be comply'd with, the ADVERTISER promiſes to give the Publick an exact Account of the whole Number of Perſons inoculated in *Great Britain*, diſtinguiſhing them into Claſſes according to their ſeveral Ages; as likewiſe of thoſe that it has had no Effect upon, and of thoſe that have died of it.

AND he farther promiſes to preſerve the Original Accounts, that ſhall be ſent him, and to give a Sight of them to any Gentleman who ſhall deſire it, that in caſe any of thoſe, who have been inoculated, ſhall afterwards have the SMALL POX in the natural Way, it may be known, whether ſuch Perſon had before received the SMALL POX by Inoculation, or not.

N. B. THE Names of the Perſons inoculated ſhall not be printed without Leave of the Parties concern'd.

Dec. 11th. 1723.

JAMES JURIN.

Numb. 379.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *September* and *October*. 1723.

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I. *An*

I. *An Account of a* Roman *Inſcription, found at* Chicheſter. *By* Roger Gale, *Eſq; F. R. S.*

THIS Inſcription, *Fig.* 1. as curious as any that has yet been diſcovered in *Britain*, was found the Beginning of laſt *April* at *Chicheſter*, in digging a Cellar under the Corner-houſe of St. *Martins-lane*, on the North Side as it comes into *North-ſtreet.* It lay about four Foot under Ground, with the Face upwards, by which it had the Misfortune to receive a great deal of Damage from the Picks of the Labourers as they endeavoured to raiſe it; for beſides the defacing of ſeveral Letters, what was here disinterred of the Stone was broke into four Pieces: The other Part of it, ſtill wanting, is, in all Probability, bury'd under the next Houſe, and will not be brought to light till that happens to be rebuilt. The Inſcription is cut upon a grey *Suſſex* Marble, the Length of which was ſix *Roman* Feet, as may be conjectured by meaſuring it from the Middle of the Word TEMPLUM to that End of it which is entire, and is not altogether three Foot *Engliſh*, from the Point mentioned. The Breadth of it is two and ¾ of the ſame Feet, the Letters beautifully and exactly drawn, thoſe in the two firſt Lines three Inches long, and the reſt 2 ½.

Being at *Chicheſter* in *September* laſt with Dr. *Stukely*, we took an accurate View of this Marble, which is now fixed in the Wall under a Window within the Houſe where it was found, and that we might be as ſure of the true Reading as poſſible, wherever the Letters were defaced, we impreſſed a Paper with a wet Sponge into them, and by that Means found thoſe in the fifth Line to have been as we have expreſs'd them above, and not as in other Copies that have been handed about of this Inſcription.

The only Letter wanting in the firſt Line is an N before EPTVNO, and ſo no Difficulty in reading that. As to the ſecond, though it was more uſual in Inſcriptions of this Nature to expreſs the Donation by the Word SACRVM only, referring to the *Temple* or *Altar* dedicated; yet we have ſo many Inſtances in *Gruter's Corpus Inſcriptionum* of TEMPLVM and ARAM alſo cut on the Stones, that there is not the leaſt Occaſion to ſay any Thing farther upon that Point.

The third Line can be no other Way fill'd up, than as I have done it by the prick'd Letters: I muſt own, however, that I have had ſome Scruple about the Phraſe of DOMVS DIVINA, the ſame Thing as DOMVS AVGVSTA, the *Imperial Family*, which I cannot ſay occurs, with any Certainty of the Time it was uſed in, before the Reign of *Antoninus Pius*, from whom, down to *Conſtantine the Great*, it is very frequently met with in Inſcriptions. This kept me ſome Time in Suſpenſe, whether this found at *Chicheſter* could be of ſo early a Date as the Time of *Claudius*: But as we find ſeveral Inſcriptions in *Gruter* with thoſe Words in them, or I. H. D. D. *In honorem Domus Divinæ*, which is much the ſame Thing, without any Mark of the Time when they were cut, they may have been before the Reign of *Antoninus Pius*, and then only came into more general Uſe; and as the Time that *Cogidunus* lived in, will not let this be of a later ſtanding, I think we may offer it as an Authority

Philos. Tranſ. N°. 379.

Fig. 1.

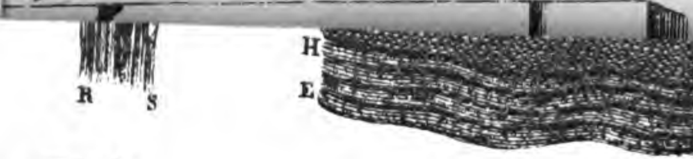


Fig. 10.

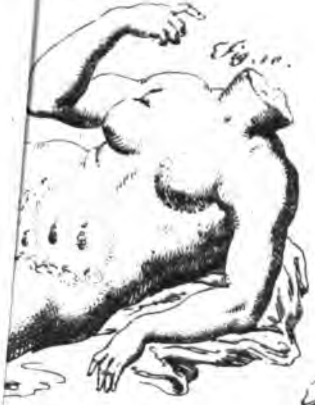
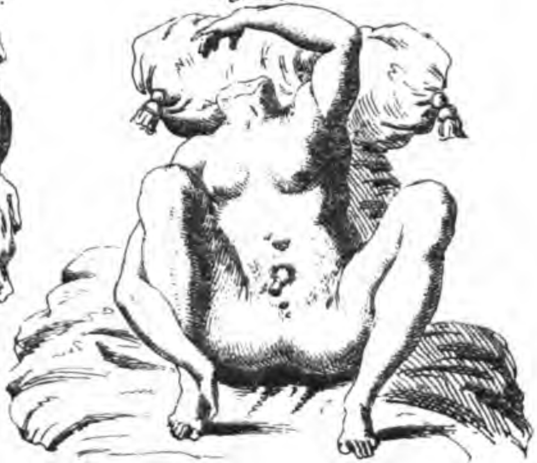


Fig. 11.



Cap. 14.

as an Authority

for the uſe of this Piece of Flattery to the Emperors long before that excellent Prince came to the Purple.

The third Line, as I believe, was EX AVCTORITATE. TIB. CLAVD. and the fourth COGIDVBNI. R. LEG. &c. that is, *Ex Auctoritate Tiberii Claudii Cogidubni Regis, Legati Auguſti in Britannia*; for the following Reaſons: We are informed by *Tacitus in Vita Agricolæ* [a], That after *Britain* had been reduced to a *Roman* Province by the ſucceſsful Arms of *Aulus Plautius*, and *Oſtorius Scapula*, under the Emperor *Claudins*, *Quædam Civitates* Cogiduno Regi *erant donatæ, is ad noſtram uſque memoriam fidiſſimus remanſit, vetere ac jam pridem recepta Populi Romani conſuetudine ut haberet inſtrumenta ſervitutis &* Reges. This *Cogidunus* ſeems to be the ſame Perſon as *Cogidubnus* in our Inſcription, the Letter B in the third Syllable making little or no Difference in the Word, eſpecially if pronounced ſoft, as it ought to be, like a V conſonant.

It is ſo well known to have been the Cuſtom of the Roman *Liberti* and *Clientes*, to take the Names of their Patrons and Benefactors, that it would be waſting of Time to prove the conſtant Uſage of that Practice. Now as this *Cogidubnus*, who, in all Probability, was a petty Prince of that Part of the *Dobuni* which had ſubmitted to *Claudius*, and one that continued many Years faithful to him and the *Romans* [b], had given him the Government of ſome Part of the Iſland by that Emperor, nothing could be more grateful in regard to *Claudius*, nor more honourable to himſelf, after he was *Romaniſed*, than to take the Names of a Benefa-

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[a] Cap. 14.

[b] V. Tacit. ut ſupra.

ctor to whom he was indebted for his Kingdom, and ſo call himſelf TIBERIVS CLAVDIVS COGIDVBNVS.

I ſuppoſe him to have been a *Regulus* of the *Dobuni*; becauſe we are told by *Dion Caſſius* [c], that *Aulus Plautius* having put to flight *Cataratacus* and *Togodumnus*, Sons of *Cunobelin*, part of the *Boduni* (the ſame People as the *Dobuni*) who were ſubject to the *Catuellani*, ſubmitted to the *Romans*; and the Name *Cogidubnus*, or *Cogiduvnus*, **Coc a Dubn**, or **Dubn** [d], ſignifying expreſsly in the *Britiſh* Language PRINCEPS DOBVNORVM, ſeems to put the Matter out of all doubt.

How far his Territories extended, it is impoſſible to define. Biſhop *Stillingfleet* [e] ſuppoſes them to have lain in *Surrey* and *Suſſex*; *Suſſex* certainly was Part of them, ſince the Temple mentioned in this Inſcription was erected in it by his Authority; and it is not unlikely, that beſides the *Regni*, who were the People of thoſe two Counties, he might have that Part of the *Dobuni* which had ſubmitted to the *Romans*, and ſeems to have been his own Principality, together with the *Ancalites*, *Bibroci*, and *Segontiaci*, whoſe Countries lay between the *Dobuni* and the *Regni*, beſtowed upon him; the Words *Civitates quædam* in *Tacitus*, not importing no more than ſome *few Towns*, but *ſeveral People*, the Word *Civitas* always ſignifying a *People* in that Hiſtorian.

Before I proceed any farther, it will not be amiſs to obſerve, that *Togodumnus* and *Cogidubnus*, tho' their Names are ſo much alike, were two diſtinct Perſons: The firſt was Son of *Cunobelin*, King of the *Trinobantes*,

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[c] In lib. LX. [d] V. Baxteri Gloſſar. in verbis COGIDVMNVS. & DOBVNI. [e] Orig. Britan. p. 63.

I

tes, vanquiſhed and killed in Battle by *Aulus Plautius*. The ſecond a Prince that ſubmitted to *Oſtorius Scapula*, and continued in his Fidelity to the *Romans*, *in noſtram uſque memoriam*, ſays *Tacitus*, who was born at the latter End of *Claudius*'s Reign; ſo that *Togodumnus* was probably dead before *Cogidubnus* had his Government conferred upon him.

I call it his Government, for tho' by the Letter ·R· ſtanding in the Inſcription with a Point both before and after it, by which it plainly denotes an entire Word of itſelf, it may ſeem that it was intended for COGIDVBNI REGIS, and I believe was ſo in reſpect of his *quondam* Dignity, yet it is evident, that he had condeſcended to take the Title of LEGATVS AVGVSTI IN BRITANNIA from *Claudius*; and that too muſt have been only over thoſe People that he had given him the Government of, *Aulus Plautius*, *Oſtorius Scapula*, *Didius Gallus*, *Avitus Veranius*, and *Suetonius Paullinus*, having the ſupreme Command ſucceſſively about this Time in this Iſland, the ſecond and laſt of which are called expreſsly *Legati* by *Tacitus*[f]. The *Legati Cæſaris* or *Auguſti* were thoſe, *qui Cæſaribus ſubditas regebant Provincias*.

The ſixth Line has loſt at the Beginning the Letters COLLE, but ſo much remains of the Word as makes it to have been indubitably, when entire, COLLEGIVM, and the following Letters are an Abbreviation of FABRORVM.

Theſe Colleges of Artificers were very ancient at *Rome*, as ancient as their ſecond King *Numa Pompilius*, if we may believe *Plutarch*[g], who tells us, that the People were divided by him into what we at this Day call *Companies of Tradeſmen*, and mentions the Τέκτονες or *Fabri* among them, tho' *Florus*[h] ſays, that *Populus Romanus a* Servio Tullio *relatus fuit in Cenſum*, *digeſtus in Claſſes*, *Curiis atque* Collegiis *diſtributus*. But as the Power of the *Romans* extended itſelf, it carry'd the Arts of that great People along with it, and improv'd the Nations that it ſubdued, by civilizing, and teaching them the Uſe of whatever was neceſſary or advantageous among their Conquerors, from which moſt wiſe and generous Diſpoſition, among other beneficial Inſtitutions, we find theſe *Collegia* to have been eſtabliſhed in every Part of the Empire, from the frequent mention of them in the Inſcriptions collected by *Gruter*, *Spon*, and other Antiquaries.

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[f] Lib. xii. Ann. cap. 32. & Vit. Agric. cap. 15.

Several Sorts of Workmen were included under the Name of *Fabri*, particularly all thoſe that were concern'd in any kind of Building, whence we meet with the *Fabri Ferrarii*, *Lignarii*, *Tignarii*, *Materiarii*, *Navales*, and others; the laſt named may have been the Authors of dedicating this Temple to *Neptune*, having ſo near a Relation to the Sea, from which the City of *Chicheſter* is at ſo ſmall a Diſtance, that perhaps that Arm of it which ſtill comes up within two Miles of its Walls, might formerly have waſh'd them. The reſt of the Fraternity might very well pay the ſame Devotion to *Minerva*, the Goddeſs of all Arts and Sciences, and Patroneſs of the *Dædalian* Profeſſion.

As no leſs than five Letters are wanting at the Beginning of the ſixth Line, there cannot be fewer loſt at the Beginning of the ſeventh, where the Stone is more broke away than above; ſo that probably there were ſix when it was perfect. What we have left of them is only the Top of an S; I will not therefore take upon me to affirm any Thing as to the reading of them, which is ſo entirely defaced; perhaps it was A. SACR. S. *a ſacris ſunt*; perhaps it was HONOR. S. *Honorati ſunt:* As to the former, we find theſe *Collegia* had their *Sacerdotes*, therefore *Qui a ſacris ſunt*, which is found in Inſcriptions [i], would be no improper Term to expreſs them; or it might have been SACER. S. *ſacerdotes ſunt*, ſince we find ſuch mentioned in the following Inſcriptions [k].

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[g] In vit. Numæ.

[h] Lib. I. cap. 6.

MAVORTI SACRVM
HOC SIGNVM
RESTIT------
COLL. FABR. ARI
CINORVM ANTIQVISS.
VETVSTATE
DILAPSVM ET
REFECER. CVR. L. LVCILIVS
LATINVS PROC. R.P. ARIC.
ET T. SEXTIVS MAGGIVS
SACER. COLL. EIVSD.

Mavorti ſacrum hoc Signum reſtituit Collegium Fabrorum Aricinorum *Antiquiſſimum, vetuſtate dilapſum, et refecerunt. Curabant Lucius Lucilius Latinus, Procurator Reipublicæ Aricinorum, et Titus Sextius Maggius* Sacerdos *Collegii ejuſdem.*

[l] L.

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[i] V. Grut. Corp. XXIX. 8. CXXI. 1. DCXXXII. 1.

[k] Spon. Miſcell. Erud. Antiquit. p. 58.

L. TERTENI AMANTI
SACER. COLL. LOTORVM
IIVIR C. SARTIVS C. F.
ITERINVS ET L. ALLIVS
PETELINVS D. D.

Lucius Tertenius Amantius Sacerdos *Collegii Lotorum, Dnumviri Caius Sartius, Caii Filius, Iterinus, et Lucius Allius Petelinus Dedicaverunt.*

As to the latter, thoſe Members of the College that had paſſed through the chief Offices of it, as that of *Præfectus*, or *Magiſter quinquennalis*, had the Title of HONORATI conferred upon them: You have ſeveral of theſe HONORATI mentioned in *Gruter*, particularly a long Catalogue of them *in Collegio Fabrorum Tignariorum*, p. CCLXVIII. 1. and in *Reineſius*'s *Syntagma* [m] there is an Inſcription.

EPAGATHO TVRANNO
HONORATO COLLEGI
FABRVM TIGNARIORVM
ROMANENSIVM &c.

So that the Vacuity in our Inſcription may very well have been filled up with one or other of theſe Words, and the three next Letters that follow them D. S. D *de ſuo dedicaverunt*, will agree with either of them, and what precedes them.

The

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[l] Ibid. p. 64.

[m] Pag. 605.

The laſt Line has been PVDENTE PVDENTINI FILio; but there muſt have been a Letter or two of the *Prænomen* at the Beginning of it, unleſs it was ſhorter than the reſt at that, as well as at the latter End of it; and from what I have ſaid, the whole may be read as follows.

Neptuno & Minervæ Templum pro Salute Domus Divinæ, ex Auctoritate Tiberii Claudii Cogidubni Regis, Legati Auguſti in Brittanniâ, Collegium Fabrorum, & Qui in eo a Sacris [or] *Honorati ſunt, De ſuo Dedicaverunt, Donante aream Pudente Pudentini Filio.*

Chicheſter, by this Inſcription found at it, muſt have been a Town of Eminence very ſoon after the *Romans* had ſettled here, and in Proceſs of Time ſeems to have been much frequented, by the *Roman* Roads, ſtill viſible, that terminate here from *Portſmouth*, *Midhurſt*, and *Arundel*, tho', what is very ſtrange, we have no *Roman* Name now known for it. I once thought it might have put in its Claim for *Anderida*, which our Antiquaries have not yet agreed to fix any where, being ſituated, very near, both to the *Sylva Anderida*, and the *Southern Coaſt* of the Iſland, the two Properties of that City[n]: But *Henry* of *Huntingdon*, who lived in the Time of *Henry* II. telling us, that the *Saxons* ſo deſtroyed *Andredeceſter*, that *Nunquam poſtea reædificata fuit, & locus tantum quaſi nobiliſſimæ urbis tranſeuntibus oſtenditur deſolatus*[o], it could not be *Chicheſter*; for that was not only rebuilt before his Time, but was a Place of ſuch Note, that when the Biſhops, ſoon after the Conqueſt[p], removed their Churches

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[n] V. Camb. *Brit. and* Sonner'*s Roman Ports and Forts.*

[o] Pag. 312. V. *Dr.* Tabor'*s Diſcourſe of* Anderida. Philoſ. Tranſ. N. 356.

[p] A. D. 1076.

from ſmall decay'd Towns, where ſeveral of them were then ſeated, *in Urbes Celebriores*, *Stigand* then Biſhop of *Selſey* ſettled his Epiſcopal Chair at that Place.

I ſhall conclude with obſerving, that when this Inſcription was dug up, there were alſo two Walls of Stone diſcovered cloſe by it, three Foot thick each, one running North, the other Eaſt, and joining in an Angle, as the *North-ſtreet* and *St. Martins-lane* now turn, which, in all Probability, were Part of the Foundations of the Temple mentioned on the Marble.

Octob. 31. 1723.

II. *De Structura Diaphragmatis. Epiſtola Domini* Antonii van Leeuwenhoek, *R. S. S. ad* Societatem Regiam.

Delphis, 31 *Maii* 1723.

IN epiſtola illa, quam ad Dominum *Jurin* nuper perſcripſi, paucis aperueram opinionem meam de morbo, qui nuperrima hyeme me invaſerat. Statuebam enim malam affectionem Diaphragmatis originem morbo præbuiſſe.

Ut autem de hac Diaphragmatis affectione omnem mihi dubitationem eximerem, Diaphragma ovis anniculæ ad me deferendum curavi; partem illius Diaphragmatis in minuta fruſtula conſectam, ope microſcopii, qua potui diligentia conſideravi; comperique Diaphragma ex parte compoſitum eſſe ex tenuiſſimis fibrillis; quæ ſine microſcopio conſpectæ latum circiter capillum inter ſe diſtant.

Cum deinde diverſa Diaphragmatis fruſta diverſis microſcopiis applicuiſſem, & viſu diligenter examinaſſem; pro certo ſtatui fibrillas illas ex partibus exoriri carnoſis, quæ Diaphragmati circa coſtas intexuntur. Eaſdem fibrillas Diaphragmati vice tendinum inſervire judicabam. Tendines iſti, ſic mihi dicti, non ſunt per totum Diaphragma æque ſibi vicini: Quin alii aliis, quamvis ſibi vicinis, ſunt aliquanto craſſiores.

Cum autem & fibrillas modo dictas, & membranam inter fibrillas exporrectam, rite conſideraſſem; animadverti Diaphragma, quantum ad maximam ſui partem, ex fibrillis iſtis, & membrana fibrillis interjecta, compoſitum eſſe: Si tamen vaſorum ſanguineorum & copioſi adipis, quæ Diaphragmati magna copia inſident, rationem modo non habeamus.

Membrana vero, inter tendines (ita mihi dictos) interjecta, tot rugulis exiguiſve plicis ſcatebat, ut pene obſtupeſcerem: iſtas autem rugas ſive plicas ad hunc uſum naturaliter deſtinatas eſſe credebam, ut Diaphragma per aëris inſpirationem extenſum rurſus contraherent; eaque ratione opem ſuam conferrent ad aërem, qui Pulmones per inſpirationem impleverat, inde expellendum.

Ut Diaphragmatis contexturam oculis ſpectandum exponerem; exiguam Diaphragmatis ovilli modo dicti portionem delineari, & in icone 2. per ABCDEFGH deſignari curavi. Ubi BG, CF; & DE, tres ſunt tendines, ea magnitudine expreſſi, quam microſcopium oculis exhibebat: qui tendines, ut jam ante monui, magnitudine non parum inter ſe differunt. Inter illos vero tendines membranam interjectam videmus; ex quibus tendinibus, & interjecta membrana, Diaphragma maximam partem compoſitum eſſe modo dicebam.

Ut autem de vera iconis 2 magnitudine judicare poſſitis, delineatorem, ut veram fruſtuli delineati magnitudinem digito indicaret, rogavi; qui mox ſpatium inter Y Z in icone 5. commonſtravit.

Adhæc membranam modo dictam permultis iiſque exiguiſſimis foraminibus pertuſam eſſe obſervavi: quæ exiguiſſima foramina ab abdomine patebant in pectus, & a pectore viciſſim in abdomen. Cum autem fieri non poſſe hactenus judicaverim, ut humor quiſpiam ex pectore delabatur in abdomen, vel ex abdomine ſubvehatur in pectus; nunc ſatius mihi videtur, indicium ſuper ea re meum ſuſpendere. Quin ſic mecum diſſero; forte etiam pulmonibus noſtris vaſcula inſunt incomprehenſibilis exilitatis. Quippe ſi cogitemus, quantum humoris ex pulmonibus noſtris per exſpirationem educatur in aërem, præ exigua humoris copia quam inſpirando ex aëre inferimus in pulmones; facile in ſententiam illam propendebimus.

Sed & hoc non ſemel obſervavi, quod fibrillas illas, quas tendines appello, vaſa quædam perrepant itinere tranſverſo.

In alio ejuſdem Diaphragmatis fruſtulo, quod microſcopio applicatum ſervabam, clare intueri licebat ſtructuram exortumque membranularum; quas inter fibrillas, ſive tendines ſic mihi dictos, interjacere dixi. Membranulas illas permagno plicarum numero, ut etiam præmonui, inſtructas videmus: quæ omnia in icone 3, exhibentur ad L K. Fibrillæ vero, ſive exigui tendines, de quibus modo diſſerebam, in eadem icone deſignantur per L M N O P Q. Verum fibrillæ iſtæ ſunt hic majores, quam uſpiam mihi occurrerint.

Præterea membranam illam, ex qua Diaphragma maximam partem conſtare dixi, quamvis tam tenuem, a ſe divelli, & exiguam avulſæ partis portionem ope microſcopii in icone 4 exprimendam curavi. Quod eo conſilio feci, ut laceros tendines atque membranulas, quantum

tum ars imitari naturam poterat, oculis ſubjicerem. Ergo hic per R S T V particula indicatur tendinis extenſi: ubi & exiliſſimæ exhibentur fibrillæ, ex quibus prædicta tendinis portio contexta eſt. In eadem icone per V T & W X portio deſignatur tendinis quieſcentis.

Iſtud vero neque explicari, neque ſatis comprehendi poteſt, quam incredibilis fibrillarum, vaſculorum, & particularum numerus hic inter ſe conglomeretur; & ad tenuiſſimæ membranæ, Diaphragmatis inquam, formationem concurrat. Sic autem ſtatuo, quoties validius inſpiramus, omnes illas plicas ſive ſinuoſas corrugationes in Diaphragmate evaneſcere atque complanari: quoties autem exſpiramus, Diaphragma rurſus in plicas rugaſve contrahi, quod & ſuperius notavi.

Dum igitur tantam adipis copiam in Diaphragmate gigni, & congregari videmus; prætereaque cogitamus ſubſtantiam Diaphragmatis per adipem illum ſic utrimque poſſe craſſeſcere, ut in tumorem extuberet; facile intelligemus Diaphragmatis expanſionem atque contractionem ea ratione poſſe impediri atque pigreſcere, atque hinc ſpirandi difficultatem conſequi: ob quam rationem homines obeſos præ aliis anhelos eſſe judicamus. Modico autem adipe Diaphragma perfundi oblinique neceſſarium ſtatuimus: cum enim Diaphragma ſingulis horis nongenties expandi & nongenties contrahi neceſſe ſit; motus illos per adipem magnopere adjuvari cenſemus.

Ceterum frequentiori indagatione explorare conatus ſum, qua ratione carneæ Diaphragmatis partes, quas coſtis vicinas eſſe dixi, tranſiiſſent in membranam; ſive qua ratione membrana Diaphragmatis ex illis eſſet exorta. Verum licet indagationem illam crebro frequentarem, ipſe mihi ſatisfacere non potui.

Jam & illa dubitatio me ſubierat, an non tendines, quos ego quidem ſic voco, vaſculis ſint inſtructi: Et licet microſcopio meo vix tantum tribuere auderem; tamen tertio poſt die indagationem iteravi, & tendines tranſverſe diſſecui: quando & illuc indagationem meam dirigebam, ut etiam in membrana Diaphragmatis, quam rugoſam eſſe dixi, ſanguinea vaſa requirerem. Tandem igitur ſucceſſit, ut ſanguinea vaſa ſecundum longitudinem tendinum protendi, imo octonos denoſque invicem adjacere viderem. Inde ad membranam, quæ tendines quaſi circumveſtit, oculos converti; in eaque tam ingentem obſervavi numerum exiliſſimorum vaſculorum, ut admiratione defixus hærerem. Complura iſtorum vaſculorum velut areolis incluſa jacebant; quæ tendinum tranſverſe perſectorum vaſcula eſſe cenſebam. Ut autem eorum omnium, quæ modo narravi, teſtem haberem ocularem; microſcopium iſtud, cui dictas Diaphragmatis particulas applicaveram, Chirurgo meo tradidi; qui dicta mea cum iis quæ videbat, examuſſim convenire reſpondit.

Hac tamen obſervatione non contentus, comparavi Diaphragma bovillum; ex quo circa partes illius carneas, quas coſtis adjacere dixi, fruſtum circiter palmarium excidi: comperique Diaphragma bovillum circiter quadruplo craſſius eſſe ovillo; & hanc craſſitudinem maxima ex parte illius adipi acceptam eſſe referendam.

Exinde ex illa Diaphragmatis portione lamellam quam potui tenuiſſimam abſcidi; quoties enim paulo craſſiorem exſcindebam, perſecabam adipem, qui verſus medium Diaphragmatis protenditur; puta, ubi Diaphragma ſatis eſt craſſum, neque adipis expers. Diaphragma vero utrimque, id eſt, ſuperius atque inferius, quatuor diſtinctis conſtat membranis, earumque tendinibus; qui tendines membranis veluti incluſi delitescunt.

Iſtorum autem tendinum ingens multitudo non poterat mihi non admirationem parere: neque ſatis credibile videbatur in corpore, cujus ſtructura functionesque tam parum ſunt cognitæ, in Diaphragmate inquam, tales tamque multiplices partes includi.

Ut Diaphragmatis bovilli ſtructuram oculis exponerem, partem illius in icone 6 ad ABCD exprimendam curavi, ubi primo quatuor exhibentur partes diſtinctorum tendinum, qui magno numero invicem adjacent, & ſupra in Diaphragmate ovillo etiam ſunt exhibiti. Verum per integrum bovis Diaphragma iſtiuſmodi diffunduntur exiguiſſimi tendines, quos ego quidem ſic appello. Et hos quidem tendines, una cum membranis, neceſſe eſt in rite conſtituto corpore ſine ulla intermiſſione alternatim extendi atque contrahi; quæ omnia in icone 6 ſatis accurate delineator expreſſit.

Tenuiſſimas illas fibrillas, uti & membranarum rugulas plicaſve, per crebram indagationem etiam ex voto meo deprehendi in Diaphragmate ovillo; nec ipſe tantum ſæpius cum voluptate conſideravi, ſed & aliis conſiderandas exhibui. Ceterum tendines illos, ita mihi dictos, & tenuiſſimas particulas tranſverſe perſecui; inquirere volens num forte ſint perviæ, & cavitatibus præditæ. Quando incredibilis exilitatis vaſcula tanto numero in oculos meos incurrerunt; ut, qui ipſe non viderit, vix mihi ſit habiturus fidem. Tum & ingentem percepi numerum exiguiſſimorum vaſculorum, quæ tranſverſo Diaphragma itinere permeabant; & quantum ego quidem ſentiebam, ad hoc officium a natura deſtinantur, ut adipem per omnes Diaphragmatis partes circumferant, & ipſi Diaphragmati continuum alimentum ſubminiſtrent.

Superius modo dixi adipem in Diaphragmate incluſum latere; & utrimque ſuper adipem quatuor exporrigi membranas, inter ſe omnino diſtinctas. Addo membranas iſtas tam arcte ſibi conjunctas eſſe,ut ſimplex eſſe membrana videatur.

Jam vero alteri microſcopio, quod paulo minus quam microſcopia modo dicta magnitudinem objectorum augebat, fruſtum Diaphragmatis mediocris craſſitudinis admoveram. Quod eo ſpectabat, ut demonſtrarem, qua ratione membranæ partes Diaphragmatis adipoſas, ſive ipſum Diaphragmatis adipem, utrimque circumdent; adeo ut adeps in membranis illis includi videatur. Id autem in icone 7 indicatur per EFGHIKLM, ubi per EFGH, item per IKLM, duæ iſtæ quadruplices deſignantur membranæ; in GH vero, & IK, adeps ſæpe jam dictus includitur.

Cum deinde tenuiſſimam Diaphragmatis portionem, quæ præ manibus erat, in orbes conſcindere conarer, Diaphragma in duas partes ſive lamellas ſeceſſit; unde ſuſpicatus ſum Diaphragma ſic a natura comparatum eſſe, ut adeps eo facilius per Diaphragmatis ſubſtantiam diſtribuatur.

In icone 7, ubi membranæ tranſverſe ſectæ exhibentur, quædam exiguæ apparent corrugationes, quas in iis plerumque percepi. Poſtea comperi membranulas, in quibus adeps coeundo producitur, exiliſſimis vaſculis annexas eſſe; quæ tamen vaſcula per contiguarum partium exſiccationem in Diaphragmate jam dicto erant diſrupta. Hanc ego membranularum & vaſculorum connexionem ex eo capite neceſſariam exiſtimo; ne Diaphragma, quoties extenditur, omnino complanetur, ſed in cavitatem pene globoſam ſeſe expandat.

Forte complures Diaphragma ſentient eſſe membranam apprimè craſſam; cum tamen ſit membrana perexilis. Ut igitur hunc illis errorem eximam; fruſtulum Diaphragmatis bovilli, ſicut ſe nudis oculis offert, delineandum curavi: Iſtud tamen verum eſt, Diaphragma pro tenuitate ſua eſſe oppido robuſtum, quod multitudini exiliſſimorum tendinum, de quibus ſæpius egi, tribuendum eſſe ſtatuo.

Igitur per N O P Q, in icone 8, fruſtulum deſignatur Diaphragmatis bovilli: ubi per fibrillam N O unum latus, ſive dimidia craſſitudo, Diaphragmatis bovilli indicatur; per P Q altera craſſitudinis illius medietas exprimitur. Reliquum Diaphragmatis, nempe quidquid inter O P & N Q interclusum eſt, adipe ſive partibus adipoſis eſt obſitum: niſi quod inter N Q nonnihil carnis interjaceat, pertinens ad carnem coſtis adjacentem.

Igitur quo morbum antedictum, qui nupera hyeme me corripuit, & quem ego Diaphragmati maxima ex parte imputabam, ſæpius in memoriam revoco; eo etiam ſæpius Medicos errare cogito, dum palpitationem, quam in pectoris regione quandoque percipimus, cordis palpitationem appellant. Certe ego tales palpitationes ex mala Diaphragmatis affectione oriri cenſeo; ſive affectionem illam producat alimenti defectus, ſive vaſorum quorundam efficiat obſtipatio, quæ Diaphragma magno numero pervagantur. Talis enim obſtipatio in ante-dictis tendinibus facile convulſivos motus excitare poteſt; & hanc morbi mei ipſiſſimam fuiſſe cauſam exiſtimo.

III. *Partium Genitalium in Muliere Structura præternaturalis. Ex Epiſtola Viri Doctiſſimi* Johannis Huxham, *M. D. ad* Jacobum Jurin, *R. S. Secr.*

Plimutho, v° *Kalend. Octob.* 1723.

A. B. De parochia *Lanteglaſs* in Comit. *Cornubiæ*, prope *Fowye* oppidum, annos nata XXIII nupta fuit cuidam Nautæ robuſto, tandemque prægnans, ſibi partium male conformatarum conſcia, opem imploravit chirurgicam. Sibi accerſitum eſſe voluit Dominum *Bonnett* de *Fowye* oppido, & artis chirurgicæ, & obſtetriciæ peritiſſimum. Ille vero perſpecto denudato corpore ſequentia obſervavit.

Loco umbilici, in medio abdomine, paululum vero inferius, prominet maſſa quædam ſpongioſa, carnis quaſi offam præ ſe ferens, abdomini tranſverſe incumbens, magnitudinis fere ovi gallinacei, longa autem tres digitos: ex hac duo erumpunt meatus urinarii exiles, qui urinam perpetim exſtillant, quam neque vel retinere vel ejaculare poteſt; hinc veſicam, (ſi quæ adſit) ſphinctere caruiſſe concludendum. Spongioſa hæc molliſque maſſa, urinæ acrimonia corroſa, tactum fere refugit molliſſimum, adeo ut incurvata obambulare coacta ſit, ad dolorem a veſtibus impreſſum evitandum, eamque tenuiſſimis linteis involvere. Hanc quidem maſſam funem fuiſſe umbilicalem male a partu abſciſſum, male dein curatum, exiſtimo; ne vel minima enim umbilici, niſi hic, apparent veſtigia; quid quod & urina per urachum pervium, & in duos

foramina

forſan tubulos diviſum, vel ſaltem per canales duos proprios, effluit. Priori potius credo opinioni; quia dantur Hiſtoriæ urinam per umbilicum in adultis etiam excretam fuiſſe atteſtantes. *Vid. Hiſt. de l'Academie Royale des Sciences, Ann.* 1701.

Hanc infra offam ſubintrat Vaginæ foramen, ab offa diſtans breviſſimo ſpatio: ex hoc effluxere Catamenia, per hoc etiam gravida facta fuit mulier, haud magnam vero tempore coitus percepit voluptatem; ſumma etiam Penis glans in hoc orificium vix fuit intromiſſa, multo minus ipſa virga. In hoc foramen digitum ægre introduxit Chirurgus, eo ſcilicet animo, ut ipſum uteri collum exploraret, quod tamen ne vel tactu percepit, plane autem deprehendit membranam craſſam hocce orificium ab altero inferiore, jam deſcribendo, ſeparantem.

Eo fere ipſo loco, ſuperius vero paululum, ubi in mulieribus rite conformatis adeſt foſſa magna, inventum fuit foramen alterum oblongum, ne vel minimi digiti apicem altius admittens, recto inteſtino, uti poſt partum obſervatum fuit, pervium (quod a ſectione fortaſſe accidit): nullus autem hic occurrit Sphincter: inferius vero rectum inteſtinum, more ſolito, cum Sphinctere circundato terminatur.

Orificium hoc oblongum ab orificio vaginæ, abdomine maxime tumente, duos ſaltem digitos tranverſos diſtitit, inter quæ membrana deſcripta intervenit ab interiore parte, hujuſce vero Fiſſuræ oblongæ quaſi labiorum coalitio exterius e ſuperiore foraminis parte.

Nulla hic Clitoris, hic nulla oſſium Pubis adfuere veſtigia, niſi, quaſi Apophyſes breviores ex utriuſque oſſis Ilii parte inferiore protuberantes, oſſium Pubis rudimenta dixiſſes. Hic fuit ante partum rerum ſtatus.

Die 18 Julii 1722, hora noctis 11ª advocatus eſt *Bonnett* Chirurgus, ut parturienti opem ferret. Perpenſis omnibus, Fœtum invenit Vaginæ orificium infra dilapſum, quem muliere decumbente ſuperiora verſus propellere fruſtra adnixus eſt, ob fortiſſimos Fœtus motus & graviſſimos Matris dolores, cui etiam jam ſupervenere Convulſiones, Syncope, &c. Vaginæ autem orificium vix ac ne vix quidem dilatatum, ita ut illi jam jam moriendum eſſe fuerit omnium expectatio.

In hoc miſerrimo rerum ſtatu, miſericordia & humanitate adductus Chirurgus, poſthabuit omnino, quid vel ignarum vulgus, vel invidus hic garriret aut hic: anceps experiendum eſſe remedium potius quam nullum apud ſe ſtatuit, morte aliter citiſſimo ingruente pede.

Parentibus periculoſiſſimum rerum ſtatum enarravit, nil niſi a ſectione expectandum, ancipitem prædixit eventum. Annuentibus hujus miſerrimæ matre, & aſtantibus, in orificium oblongum inferius ſcalpellum chirurgicum introduxit, & uno ictu coalitionem labiorum hujuſce orificii & membranam ſeparantem diviſit: hinc in unum coivere & orificium Vaginæ & oblongum inferius. Jam digitis facilis introitus, orificium Uteri internum attrectavit; dilatavit paululum, caput inde ſenſit infantis: quid plura? Fœtus ori intruſo digito, Puellulam vivam, probe formatam, mirantibus maxime aſtantibus, demum extraxit, quæ & adhuc & viget & valet: uti etiam & ipſa Mater, quanquam poſt puerperium febre graviter colluctata eſt.

Jam a partu prolapſu ipſius Uteri divexatur, quo prolabente nec per horas 8, 10ᵛᵉ reducto, eam dein ſi fortius intrudas, exiliunt e meatibus urinariis deſcriptis rivuli duo ad pedis ſaltem diſtantiam; argumentum ſane Cyſtidis cujuſdam urinam excipientis: aliter revera ſuſpicaſſem meatus iſtos duos urinarios ipſorum ureterum fuiſſe orificia, hic terminantia.

Quaerat fortaſſe curioſior aliquis, quo forte modo gravida facta fuit muliercula noſtra. Illi reſponderem, Penis intruſionem ad prolem concipiendam haud abſolute eſſe neceſſariam, ſeminis autem intra vaginam ejaculationem quam maxime. *Vid. Hiſt. de l'Acad. Royale des Sciences*, 1712. *Videatur etiam Mauriceau.*

Hujus rei narrationem tibi citius tranſmiſiſſem, ni diu, fruſtra autem hactenus, mulieris alteram graviditatem expectaſſemus, quum per ſat longum tempus a puerperio cum marito cohabitaverit.

Figura 9 repreſentat mulierem gravidam.

A A Spongioſam aſſimulat quaſi carnis maſſam, quam partem fuiſſe chordæ umbilicalis autumo, magtudinis fere ovi gallinacei, tres vero digitos longam. Ex hac duo erumpunt Fiſtulæ urinariæ *b b*, quæ urinam perpetim exſtillant, nec ex ullo alio orificio redditur urina.

c Orificium Vaginæ, cloacæ galli gallinacei quam ſimillimum, ſpiſſo quaſi ſepto circumdatum: per hoc effluxere ἔμμηνα, per hoc etiam gravida facta fuit mulier. Hoc orificium, dum perpeſſa fuit mulier graviſſimos partus dolores, nullo fere modo dilatatum fuit, quanquam anus ipſe, ob vehementiſſimos muſculorum abdominis, &c. contractiones, maxime hiaret.

d Locum indicat, ubi inventum fuit foramen oblongum, minimi digiti apicem vix admittens. In hoc, tempore puerperii, Chirurgus introduxit ſcalpellum chirurgicum

cum, idque ab hoc in orificium ſuperius adegit: ita ut jam a partu dehiſcat monſtroſum horrendum informe ingens foramen.

Orificium ſuperius tranſverſum ab inferiore oblongo (appropinquante partu) 2 fere digitos diſtitit. Ex hoc magno hiatu ipſius uteri prolapſum ſæpiſſime nunc patitur. Ex loco etiam, ubi adfuit olim orificium inferius, jam a puerperio, partim excretæ ſunt Fœces alvinæ: hoc vero ante partum minimè obſervatum fuit, ita ut a ſectione forte acciderit.

Orificium hoc oblongum paulo altius quam par eſt, in Figura deſignatur.

e Anum repræſentat paulo magis antrorſum quam in aliis poſitum: per hunc maxima fœcum pars egeritur. Pudendum hoc, quale quale, eſt non denſe crinitum.

IV. *Mulieris ejuſdem Hiſtoria, ex Epiſtola Medici Doctiſſimi,* Gulielmi Oliver, *ad* Richardum Mead, *M. D. R. S. & Coll. Med.* Lond. *Soc.*

Vir celeberrime,

TIBI omnium animalium naturas & formationes optimè callenti, acerrimo judicio quotidie perſcrutanti, hiſtoriam inuſitatæ, ac plane mirandæ cujuſdam ſtructuræ partium ingratam non fore, nullus dubito.

Mulierem, Comitatus *Cornubiæ* incolam, in hunc modum, quem jam ſum deſcripturus, a cunabulis formatam fuiſſe, matronæ vicinæ, & non paucis res & nota & examinata fuit, uno ore conſentiunt. Subſtantia quædam carnoſa, ſpongioſa, floride rubeſcens, leviſſimum tactum, ob atrociſſimum dolorem inde oriundum, fugiens umbilici, *v. Fig.* 10. quæ non ex Autopſia ſed ex Chirurgi narratione delineata eſt, naturalem locum uſurpabat, & e cavitate, quæ plerumque ibi eſt, quaſi pullulabat. Ex hac excreſcentia, quæ nec augeri, nec minui, per multos annos ſentiebatur, continuo deſtillabat urina, cui nec alia patuit e corpore via, neque hac potuit vel violentiſſimis nixibus cum impetu expelli, vel per ſaltum excerni, adeo ut paulatim exſudans veſtes interdiu, ſubſtragulaque nocte madefaceret, inquinaret. Paulo infra, ad diſtantiam pæne palmi minoris, orificium quoddam, longitudine unciam unam, latitudine digitum tranſverſum æquans,

ſic-ſeſe oſtendebat, ut indicem admitteret, & tamen perſtringeret, ſatis amplum. Hac via effluebant a pubertate menſtrua, unde ad Vaginam & Uterum hunc eſſe aditum facile patebat. Ad eandem fere diſtantiam, qua orificium hoc Vaginæ ab excreſcentia illa diſtabat, patuit & orificium alterum, ſuperiore minus, digitum minimum vix capiens. Huic autem orificio cum inteſtino rectocom municationem dari, tenuiſſimæ fæcum partes, ex eo frequenter protruſæ, ſatis demonſtrabant. Ab hoc autem Pſeudo-Ano Anus verus, per quem excrementa duriora dejiciuntur, unciis quinque vel ſex extitit, & naturalem fere pudendorum ſitum obtinuit. Pili quidem aderant, ſed rari, nec margines alicujus orificii magis quam reliquorum ornantes, ſed duas lineas deſcribentes, quæ a medio ſpatio inter excreſcentiam illam & commiſſuras femorum cum abdomine incipiebant, & ſibi mutuo appropinquantes angulum, in cujus vertice poſitus eſt verus Anus, conficiebant. Hunc in modum conformata, valetudine ſatis bona fruebatur virgo, & æterna virginitate ex neceſſite laboraturam concluſerant omnes, quibus res innotuerat. Advenit tandem Nauta quidam, cui æs triplex circa pectus erat: Illam vidit, amavit, duxit, & non multo poſt imprægnavit. Geſtationis tempore optime ſe habuit mulier, quo propemodum finito, de difficultate pariendi, non ſine ratione, aderat gravis ſuſpicio. Orificium Vaginæ a viri congreſſu non omnino dilatabatur, adeo ut de modo imprægnationis multæ inter mulierculas ortæ ſint diſputationes; & aſſeruere aliquæ non ſine dedignatione & horrore, maritum aut viri nomine non eſſe dignum, aut per aliam introiviſſe viam, quod bonus Deus vetet! Immiſſionem autem Penis in Vaginam ad imprægnationem non eſſe abſolute neceſſariam, non eſt quod tibi, Vir clariſſime, cui hæc omnia optime nota, narrarem. Exemplis turgent libri Medici. Mulier tamen de ſua vita anxia Chirurgos aliquos adivit, conſilium petiit, opem precata eſt. Unus tandem apud *Foy*, oppidum non procul a mulieris noſtræ habitaculo, caſu audito, re perſcrutata, ad promovendum partum, ſi opus fuerit, operam ſuam navare promiſit. Geſtationis tempore perfecte finito, dolores aliaque ſymptomate partum prognoſticantia illam tentaverunt. Chirurgus advocatus aderat. Refert ille infantem conatus ſuos ad regionem pubis, illa ipſa autem ad orificium Vaginæ feciſſe dicit. Quid eſt agendum? Dixit ille inciſione exitum infanti eſſe aperiendum; ex crudeli miſericordia renuunt adſtantes, ingraveſcunt dolores, ſuperveniunt convulſiones, vita deſperabatur. Tandem cum mortem præ foribus eſſe omnes judicaverunt, illam pæne expirantem in manus & poteſtatem Chirurgi tradiderunt. Ille ſtatim diſſectionem iſtius Iſthmi, qui inter orificium Vaginæ & Pſeudo-Anum erat, & haud improprie Perinæum poſſit vocari, inſtituit. Integumentis exterioribus diſſectis, membrana quædam valida & craſſa, tranſverſe locata, ſeſe in conſpectum dedit, quæ Vaginam a canaliculo illo inferiore dividebat. Hac diſſecta iter ad Uterum ſatis amplum patuit, manum introduxit, os Uteri digitis dilatavit, infantem capite prehenſo in lucem tuto eduxit, matrem ad vitam revocavit. Hanc mihi ipſe hiſtoriam retulit, illa ipſa confirmavit. Rei enim novitare percuſſus *Foy* adivi, utrum verum eſſet necne ſciendi gratia. Chirurgum de ſtructura partium originali, & operatione ſua tantum non Cæſarea, ſolicite interrogavi. Omnia, ut ſupra narravi, graphice deſcripſit. Fœminam autem ipſam videndi maxima mihi fuit cupido. Amicum quendam meum, honore ſemper colendum, ut ſub prætextu alicujus negotii mulierem ad domum ſuam accerſeret, rogavi. Accerſita venit. Deſiderium meum aperui. Illa ab ullo examine, tanquam re omnibus modeſtiæ & virtutis regulis contraria, religioſe abhorrebat. Me Medicum profeſſus, rem tam ab omni immodeſtia alienam eſſe probavi, quod cum in uſum publicum cederet, caſum ſuum communicare ejus eſſe officium demonſtravi. Hac ratiocinandi methodo, & argumenti alterius aurei ope, votum obtinui. Muliere igitur ad examen commode locata, excreſcentiam illam umbilici cavitatem uſurpantem ſuperiori deſcriptioni ad amuſſim reſpondentem inveni. Illam cum ſummo digiti leviſſime tetigerim, miſella, non aliter ac ſi gladio perfoſſa fuiſſet, diſcruciata ejulavit. Mirum ſane mihi videtur hanc mulierem cum marito ſuo poſſe congredi ; ex ſitu enim partium nihil aliud cogitare poſſis, quin vir rei Venereæ incumbens, excreſcentiam illam, aliter prorſus intactilem, abdomine ſuo duriter perfricaret. Rogata quomodo hæc perficiebantur, reſpondit ſe omnia ob amorem, quo maritum ſuum proſequebatur, poſſe pati. Sic amor omnia vincit. Sed ad Rem.

Hæc excreſcentia ex multis lobulis, propriis membranis obductis, a ſe mutuo diſtinctis, conflari videtur. In interſtitiis horum lobulorum poruli, per quos ſtillabat urina, oſcula ſua ad ſuperficiem aperiebant. Paulo infra hiatus magnus, ex unione chirurgica duorum orificiorum ad partum facilitandum confectus, ſeſe oſtendebat. Procidentia Uteri tunc temporis laborabat, qui extra labia, ſi circumferentia illa mereatur nomen, propendens, longitudinem ſex unciarum, magnitudinem ovi anſerini æquabat. Tali procidentia ſe ſemper a partu uſque laboraſſe narravit, & quidem mirum eſſet ſi aliter accidiſſet; tanta eſt enim ad orificium Vaginæ apertura, ut nihil poſſit ab extra Uteri prolapſui obſtare, & Uteri corpus multo magis, quam in ſtatu vulgari, a veſica premitur. Veriſimile enim eſt urinam ad excreſcentiam illam per urachum aſcendere, & per illum ab initio fluxiſſe, quia per meatum urinarium nullum natura præbuit exitum; impoſſibile tamen eſt, ut urina per urachum ad umbilicum aſcenderet, uſque dum veſica ſit turgide repleta; ex illa vero plenitudine neceſſe eſt, ut ſemper in Uterum non parvo pondere gravaretur, & illum foras extruderet. Ut conformatio partium melius appareret, reclinata in lectum Muliere, repoſitionem Uteri tentavi. Non citius illum introducere cæpi, quin urina a preſſione in veſicam repletam facta, per poros excreſcentiæ umbilicalis, multis rivulis, pilum craſſiorem magnitudine non ſuperantibus, cum impetu ad plures pedes proſiliebat, fontem artificialem non injucunde æmulans. Exinanita veſica, Vaginam inverſam in naturalem ſitum facile reſtitui; reſtitutis Vagina, & Utero dehiſcebat chaſma magnum, ex poſitione corporis circulare, cujus diametrum tres pollices tranverſos metiri credo, cujus fimbriæ, potius quam labia, multis ſarcomatis inæqualiter horrebant. In ſuperiore ejus margine interno meatum urinarium fruſtra quæſivi, in inferiore Pſeudo-Anum facile inveni, per quem digitum ſub pubem, quæ ad cartilagineam mollitiem accedere videtur, ad inteſtinum rectum immiſi, necnon contentis ejus turpiter infectum eduxi, unde credo rectum, ad tres pollices ſupra ſphincterem Ani, eſſe perforatum. Paulo ſubtus pubem, ſitum, quo aliarum ſinus ipſe pudoris gaudet, verus Anus hujus fœminæ obtinet, non conſueta caligine & profunditate occultus, utpote non intra clunes poſitus.

Figurarum 10 & 11 *Explicatio.*

Fig. 10.

a Excreſcentia umbilicalis.
b Orificium Vaginæ.
c Pſeudo-Anus.
d Anus.
e e Pili.

Fig. 11.

a Excreſcentia umbilicalis.
b Os Uteri.
c Anus.

G. Oliver.

V. *Fluxus ſanguinis per Penem mirabilis. Ex Epiſtola Viri Doctiſſimi Domini* Howman, *M. D. ad Dominum* Hans Sloane, *Baronettum. Coll. Med.* Lond. *Præſ. R. S. Vice Præſ.*

QUadragenarius quidam civis *Norvicenſis* pancratice valens, profluvio ſanguinis ſinceri per canalem urethræ correptus eſt, 30 die Junii, 1716. Idem rediit phænomenon, 31 Julii. Adhibitis quibuſdam medicamentis, & celebrata venæſectione in Brachio, cohibitus eſt fluxus ſanguinis prædictus, uſque ad octavum diem Septembris, ac deinceps penitus evanuit. Notatu tamen eſt dignum hæmorragias ſupra memoratas abſque omni prævio dolore, ſeu poſtea ſpirituum dejectione contigiſſe.

VI. *An*

VI. *An Account of the Pits for* Fullers-Earth *in* Bedfordſhire; *in a Letter from the Reverend Mr.* B. Holloway, *F. R. S. to Dr.* Woodward, *Pr. Med.* Greſh. *S. R. & Coll. Med.* Lond. *Soc.*

BEDFORD, 6 *July*, 1723.

I Went a few Days ago to the *Fullers-Earth* Pits at *Wavendon* near *Woburn*, where there are ſeveral Pits now open; but, as Men were then at work only in one, and I underſtood the Earth was diſpoſed in much the ſame Manner in all, I did not trouble my ſelf to go down into more than that wherein they were then digging; in which I found Things diſpos'd thus.

From the Surface, for about ſix Yards Depth, there are ſeveral Layers of Sands, all reddiſh, but ſome lighter colour'd than others, under which there is a thin *Stratum* of red Sand-ſtone, which they break through; and then for the Depth of about ſeven or eight Yards more, you have Sand again, and after that come to the *Fullers-Earth*; the upper Layer of which, being about a Foot deep, they call the *Cledge*; and this is by the Diggers thrown by as uſeleſs, by reaſon of its too great Mixture with the neighbouring Sand, which covers, and has inſinuated itſelf among it: After which they dig up Earth for Uſe, to the Depth of about eight Feet more, the Matter whereof is diſtinguiſh'd into ſeveral Layers, there being commonly about a Foot and an half between one horizontal Fiſſure and another. Of theſe Layers of *Fullers-Earth*, the upper Half, where the Earth breaks itſelf, is ting'd red, as it ſeems by the running of Water from the ſandy *Strata* above; and this Part they call the *Crop*; betwixt which and the *Cledge* above-mention'd, is a thin Layer of Matter not an Inch in Depth, in Taſte, Colour, and Conſiſtency, not unlike to *Terra Japonica.* The lower half of the Layers of *Fullers-Earth*, they call the *Wall-Earth*; this is unting'd with that red above-mention'd, and ſeems to be the more pure and fitter for *Fulling*; and underneath all is a *Stratum* of white rough Stone, of about two Foot thick, which, if they dig through, as they very ſeldom do, they find Sand again, and then is an End of their Works.

One Thing is obſervable in the Site of this Earth, which is, that it ſeems to have every where a pretty equal horizontal Level; becauſe they ſay, that when the Sand-Ridges at the Surface are higher, the *Fullers-Earth* lies proportionably deeper.

In theſe Works they ſeldom undermine the Ground, but as they dig away the Earth below, others are employ'd to dig and carry off the Surface; otherwiſe, the Matter above, being of ſo light and flitting a Nature, would fall in and endanger the Workmen: For, as was obſerv'd before, that *Stratum* of Sand-Stone, which occurs before they come to the *Fullers-Earth*, does not lie, as in Coal-Pits, immediately over the Matter they dig for, like a Cieling, but even in the midſt of the ſuperjacent *Strata* of Sand, and therefore can be no Security to them if they undermine.

The perpendicular Fiſſures are frequent, and the Earth in the *Strata*, beſides its apparent Diſtinction into Layers, like all other Kinds of Matter, by reaſon

of

of its peculiar Unctuouſneſs, or the running of the adjacent Sand imperceptibly among it, breaks itſelf into Pieces of all Angles and Sizes.

For the Geographical Situation of theſe Pits, they are digg'd in that Ridge of Sand-Hills by *Woburn*; which near *Oxford* is call'd *Shotover*; on which lies *Newmarket-Heath* by *Cambridge*, and which extends itſelf from Eaſt to Weſt, every where, at about the Diſtance of eight or ten Miles from the *Chiltern* Hills, which in *Cambridgeſhire* are called *Gog-Magog*; in *Bucks*, and *Oxon*, the *Chiltern* Hills, from the chalky Matter, of which they chiefly conſiſt: which two Ridges you always paſs, in going from *London* into the North, North-Eaſt, or North-Weſt Counties in the Manner I before-mention'd: After which you come into that vaſt Vale, which makes the greater Part of the Midland Counties of *Cambridge*, *Bedford*, *Bucks*, *Northampton*, *Oxford*, and *Glouceſter*, and in which are the Rivers *Cam*, *Ouſe*, *Nen*, *Avon*, *Iſis*, and others; which I take Notice of, becauſe it confirms what you ſay of the regular Diſpoſition of the Earth into like *Strata*, or Layers of Matter, commonly through vaſt Tracts, and from whence I make a Queſtion, whether *Fullers-Earth* may not probably be found in other Parts of the ſame Ridge of Sand-Hills, among other like Matter.

VII. *Invitatio ad Obſervationes Meteorologicas communi conſilio inſtituendas. A* Jacobo Jurin, *M. D. Soc. Reg. Secr. & Colleg. Med.* Lond. *Socio.*

COELI & Aeris, quem ſpiritu ducimus, conditiones varias, frigoris, puta, & caloris, ſudi, vel humidi commutationes & viciſſitudines, magnas præſertim atque ſubitaneas, ad Humani Generis valetudinem pertinere meritò cenſetur. Operam itaque & laborem in iisdem obſervandis minime contemnendum poſuerunt non Medici ſolum, ſed & alii quoque ab omni ævo Naturæ rerum contemplandæ ſtudioſi. Superiore tandem ſæculò Inſtrumenta etiam & Machinæ Philoſophorum ingenio & diligentiâ repertæ ſunt, quibus ponderis, caloris, humiditatis, & elateris aerii momenta & mutationes ſimul oculis repræſentantur, ſimul ad menſuram ac trutinam, & quidem ſubtilem admodum illam atque accuratam, exiguntur.

Nec hic etiam ſubſiſtendum judicarunt Eximii illi Viri, ſed ſtudio & ſciendi amore incitati ad cauſas harum mutationum, quâ licuit, indagandas contenderunt. Quem in finem Obſervationes Inſtrumentorum recens inventorum ope factas de pondere, humiditate, & calore ambientis diligenter in Diariis notabant; iiſque multa alia adjiciebant ad Tempeſtatem ac Cœli faciem, Ventos, & Pluviæ copiam pertinentia, quod in Actis Philoſophicis & alibi ſparſim videre eſt.

Methodo iſtâ & obſervandi ratione meliorem facile non reperias. Quod ſi fuiſſent Obſervatores & numero idoneo, & commodis locis per magna terrarum ſpatia diſpoſiti; ac tandem unus aliquis omnium Diaria, quid inter ſe convenirent aut diſcreparent, contuliſſet; profecto jam a multis annis eam haberemus Aeris Hiſtoriam, qualem hoc temporis vix animo & votis fas eſt concipere.

Id etenim compertum habemus, ut quod maxime, ſubitas Tempeſtatum commutationes Ventis præcipue acceptas eſſe referendas; quumque ſcire liceret per talem obſervandi rationem, qualem ſupra expoſuimus, quibus in locis orti, quem curſum, quo tempore, & per quanta terrarum ſpatia Venti tenerent; his cognitis, forſan ad Originem etiam & Cauſas Ventorum aſſequendas via patuiſſet. Unum hoc ſaltem, quod ipſum non leve momentum ad has diſquiſitiones attuliſſet, quodque jam, ut plurimum, pro Conjecturâ veriſimili habetur, potuiſſemus certiſſimis obſervationibus ſive veri, ſive falſi arguere. Opinionem dico ſagaciſſimi Viri, *Edmondi Halleii* *, qui Hydrargyrum ideo cenſet in Barometro aſcendere, quod Venti ex contrariis regionibus utrinque eodem ſpirantes Aerem cogant & quaſi in cumulum attollant; ut contra Hydrargyri deſcenſum Ventis, ex eodem loco verſus oppoſitas partes Aerem deferentibus, & quaſi exhaurientibus, attribuit.

Rogantur itaque Eruditi, qui ad excolendam hanc partem Hiſtoriæ Naturalis operam ſuam conferre voluerint, ut quotidie ſemel minimum, vel utcunque ſæpius libuerit, notare dignentur in Diario Barometri & Thermometri altitudinem, Venti Plagam cum aliquâ virium æſtimatione, Cœli faciem, & Pluviæ vel Nivis quantita-

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* V. Philoſ. Tranſact. N. 181.

quantitatem, quæ tempore poſt obſervationem ſuperiorem elapſo deciderit. Quod ſi quis Obſervationes Hygroſcopii cujuſlibet, ſive Acus Magneticæ ope factas adjicere voluerit, non erit ingratum.

Quoties ingruerit Procella vehementior, utile fuerit ortum ejuſdem, incrementum, ſummam violentiam, remiſſionem & exitum notatis temporibus accuratius deſignare, uti & altitudines Barometri, quæ dictis temporibus reſpondeant.

Monendum cenſemus, ut qui Barometri replendi & conficiendi modum callent, Barometro vulgari, ſive aperto, quod vocant, utantur. Sit autem Tubus quartam, ut minimum, vel etiam tertiam digiti partem latus; quum in Tubis anguſtioribus Hydrargyrus infra juſtam altitudinem ſubſidere deprehendatur †. Ciſternæ vero, ſive Vaſi Hydrargyrum excipienti tribuetur diameter octonis ſaltem, vel decem partibus major Tubi diametro, idque eum in finem, ut aſcendente, vel ſubſidente Hydrargyro in Tubo, altitudo Hydrargyri in Ciſternâ invariata permaneat, aut certe quam paululum immutata.

Qui vero Barometro clauſo, ſive portatili uti malunt, ejuſmodi Barometra magnâ diligentiâ fabricata comparare poterunt apud Laudatum Artificem *Franciſcum Haukſbejum*, in Areâ vulgo dictâ *Crane-Court*, *Londini* degentem; qui Thermometra etiam ſubminiſtrabit ad eam Scalam, ſive graduum notationem exacta, quæ jam per multos annos, exquiſitis ejus Thermometris inſculpta, innotuit Eruditis.

Qui Thermometro utuntur aliâ quâcunque ratione conſtructo, rogatos volumus, ut in Diario Thermometri ſitum, fabricam, diſpoſitionem graduum in Scalâ, & nomen

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† *V.* Philoſ. Tranſact. N. 363.

men etiam Opificis, ex cujus Officinâ prodiit, apponere ne graventur. Situm Thermometro commodiſſimum cenſemus in conclavi ad Septentriones obverſo, ubi focus aut nunquam accenditur, aut ſaltem quam rariſſime.

Quo facilius inter ſe conferri poſſint Diaria, commodum fuerit omnia in hujuſmodi formam diſponere.

Columna prima indicet diem & horam obſervationis; ſtylo autem ut omnes Juliano, ſive Vetere, in Diariis utantur, Obſervatores rogamus.

Secunda altitudinem exhibeat, ad quam attollitur Hydrargyrus in Tubo Barometri ſupra ſuperficiem Hydrargyri in Vaſe, per digitos, ſive partes duodecimas Pedis *Londinenſis*, & per partes decimales eorundem digitorum notatam. Habet autem Pes *Londinenſis* ad *Pariſienſem* eam rationem, quæ eſt inter 15 & 16 proxime.

Columna tertia gradum monſtret, & partes gradus decimales, quas Spiritus in Thermometro attingit.

Quarta Venti Plagam & ſpirandi vires repræſentet; quæ vires ſemper denotari poterunt per aliquem ex numeris ſequentibus, 1, 2, 3, 4: ex quibus 1 ſignificet leniſſimum Aeris motum vix arborum folia agitantem, 4 vero ſummam Venti violentiam; numeris 2 & 3 intermedias inter haſce Ventorum vires exponentibus, & denotante cyphrâ, ſive 0, perfectam Malaciam.

Quintam occupet Cœli facies, & ſuccincta Tempeſtatis hiſtoria.

Sexta & ultima altitudinem pluviæ, vel nivis in aquam reſolutæ, quæ poſt ſuperiorem obſervationem deciderit, per digitos *Londinenſes* & eorum partes decimales metiatur.

Hæc facile æſtimari poterit ope Infundibuli duos circiter, vel tres pedes ampli, Vaſis alterius aquam ex Infundibulo defluentem excipientis, & Menſuræ Cylindricæ cum Regulâ in digitos & partes decimales diviſâ.

Infundibulum ita ſitum ſit, ut, quicunque ventus flaverit, nulla tamen pluviæ pars ſive ædificii interventu, ſive quocunque alio impedimento intercipi queat. Sit autem vas aquam continens undique probe clauſum, ne quid in vapores attollatur, anguſto ſolum foramine, ad aquam deſuper ex Infundibulo excipiendam, relicto. Menſuræ porro Cylindricæ Diameter decem partibus minor Diametro Infundibuli tribuetur: quo fiet, ut aqua digitum unum alta in menſurâ ad altitudinem centeſimæ partis digiti in Infundibulum, atque adeo in reliquam terram, cecidiſſe intelligatur; & ſimiliter pro partibus digiti decimalibus.

Ad finem vero Menſis & Anni cujuſque apponatur media altitudo menſtrua, vel annua, Barometri & Thermometri, uti etiam ſumma omnium altitudinum Pluviæ, quæ Menſe, vel Anno integro deciderit. Habebitur autem dicta media altitudo, redigendo in unam ſummam, omnes Barometri altitudinum obſervationes mane factas, Thermometri vero ſive matutinas, ſive totius diei maximas, (quæ nempe circa horam tertiam, vel quartam pomeridianam contingunt) & ſummam iſtam per numerum dierum dividendo.

Omnes rogamus, qui ſupraſcriptas Obſervationes, vel univerſas, vel aliquâ ex parte volent inſtituere, ut Diariorum Exempla, ad finem anni cujuſque continuata, ad Secretarios Regiæ Societatis tranſmittere dignentur; uti cum Diario, quod *Londini* juſſu Societatis Regiæ conficitur, conferri poſſint. Conſilium vero eſt, ut quicquid ex Diariorum iſtorum collatione colligi poterit, ſingulis annis in Actis Philoſophicis cum Publico communicetur.

Diarii

Diarii Forma.

Dies & Hora 1723. Nov. St. V.	Barom. alt. dig.dec	Therm. alt. gr. dec.	Vent.	Tempeſtas.	Pluvia. dig.dec
1. 8 *a. m.*	29.75	49 . 6	S. W. 1	Cœlum nubibus obduct.	0.035
4 *p. m.*	29.56	47 . 3	S. W. 2	Imbres interrupti. Sol pervices intercurrens	0.043
2. 7½ *a. m.*	29.24	48 . 5	S. 1	Pluvia fere perpetua	0.725
3. 9 *a. m.*	29.95	49 . 7	N. 1	Cœlum ſudum	0.032
5 *p. m.*	30. 4	49 . 2	N. 1	Cœlum ſudum	0.000
4. 7 *a. m.*	29. 9	47 . 0	S. W. 1	Nubes ſparſæ	0.000
10	29. 7	46 . 2	S. W. 2	Imbres intercurrentes	0.103
12	29. 4	45 . 0	S. 3	Cœlum nubibus undique fere tectum	0.050
3 *p. m.*	28. 8	46 . 0	S. 4	Nubes ſparſæ	0.000
5	28. 6	47 . 2	S. W. 4	Eadem Cœli facies	0.000
7	28. 9	48 . 0	S. W. 2	Pluit	0.000
9	28. 9	48 . 2	0	Pluvia fere perpetua	0.305
5. 7 *a. m.*	29. 7	53 . 4	N. E. 1	Sudum. Gelu.	0.250

VIII. *Account of a Book, entitled,* Adverſariorum Anatomico-Medico-Chirurgicorum Decas tertia. *Auctore* Frederico Ruyſch, *M. D. Anatom. & Botan. Prof.* Amſtel. *R. S. Soc.*

§ I. THE learned Author, who has ſo long improv'd the World by his curious Searches into the niceſt Secrets of the Animal-Machine, and illuſtrated them by his admirable Preparations, gives an Account of preternatural Things found in Tumors, of that Sort which we call *Atheroma*, ordinarily a *Cyſt* filled with a thick Matter like Pult; ſuch are Bones, Fleſh, Hair, Teeth, and other Things very ſurprizing and unaccountable. He tells us, he firſt found out the Fabrick of the cortical Part of the Brain; that 'tis made of ſmall pulpy Canals from the Ends of the Arteries: That many Years ago he drew, by ſimple Diſtillation, from crude Vegetables, a perfect volatile Salt; and that 'tis eaſily done from freſh Seeds of the hot antiſcorbutic Herbs, ſuch as Muſtard, Rocket, Onions, *&c.*

§ II. He diſſects a Pear, which he ſays is entirely compos'd of the Pipes in the Stalk, dilated into a ſoft and moiſt Pulp, like what they antiently call'd the *Parenchyma*, that compoſes the Liver and Spleen, which he affirms to be pure Veſſels, not Glands diſtinguiſh'd by being invelop'd with one proper Membrane. You may, if you pleaſe, call the Pear, the Liver and the Spleen, taken in the whole, a Gland; but he denies any particular Glands in their Compoſition.

§ III. He tells a remarkable Story of a Girl, who had frequent Eruptions of Blood from the Skin of her Head, Ears, Mouth, Navel, and the Nipples of her

Breaſt: For fourteen Weeks ſhe eat no Meat, nor had any Excretion by Siege or Urine; and many other odd Symptoms.

§ IV. He gives us a pretty Obſervation of the *Cicatricula*, as commonly call'd, in the *Ovaria* of Females, whence the impregnated Egg is dropt into the *Falloppian* Tubes. He ſhews, that in reality it is a *Calyx*, or natural Aperture, which enlarges itſelf for that Purpoſe by Degrees, and after Excluſion cloſes again, as the *Os Uteri* afterwards; ſo that the Egg truly drops from it, as an Acorn when ripe from its Cup; and the Trace of it ſoon becomes imperceptible. He ſays farther, he has frequently ſeen the thick Part of the Male Sperm carry'd up even into the *Falloppian* Tubes upon Impregnation.

§ V. He ſpeaks of an Obſtruction, that has ſometimes happen'd in the Inteſtines of new-born Infants, ſo that they have had no Manner of Paſſage by Stools, and that gentle Lenitives have not been able to relieve them: In this caſe, he adviſes ſtrong Purgers, adapted to their tender Age.

§ VI. The Author informs us of wonderful Appearances within the very Center of human Bones; for in eight ſeveral Preparations he has, of Leg and Thigh Bones, and others, of Men and Infants, he has obſerved plain *Chryſalis's* of Inſects, and very perfect, of different Sorts, lodged in the Cavities of the Marrow: How they come there, he can no more account for, than for whole Bundles of Worms twiſted together, which he has found in the great Artery juſt by the Heart; or for ſix fat, thick *Erucæ* in a Sheep's Brain; and many of the like Nature, that have happened in the Way of his Obſervation, which are in the Czar's Repoſitory.

In § VII. he ſays, very often, upon Diſſection of the Bodies of old Women, he has found the meſenteric

Glands, that miniſter to the Conveyance of the Chyle, perfectly waſted away; ſo that he judges it not improbable, that, in ſuch Caſes, the meſaraic Veſſels abſorb the Chyle from the Guts, and carry it into the Blood, at leaſt in part, according to the Notion of the Antients, who knew not the lacteal Veſſels: This he thinks not a little confirm'd by the like waſting of the glandular Part of the Breaſts in old Women, which though formerly very large, ſhall have nothing but the Nipples left.

§ VIII. He treats of the *Epidermis*, or Scarf-Skin, with a good Method of ſeparating it for publick Demonſtrations: He takes off a Piece of the whole Skin, and nails it upon a Board with the Outſide uppermoſt, and then puts it into boiling Water, which raiſes it ſo, that with a blunt Knife it is eaſily ſeparable, which is a much better Way, than that of burning or bliſtering. He ſays, the different Colours of the Skin are owing to the *Corpus Reticulare*, which in Blackmoors is perfectly black, in Moors of a tawny Colour, in white People perfectly white. The *Epidermis* is ever found abſolutely void of Blood-Veſſels: upon which occaſion the Author offers to lay Two Guineas with Mr. *St. Andre*, who, he ſays, falſly aſſerts he has Preparations ſhewing theſe Veſſels, and has pretended to ſhow them to ſome of his Countrymen the *Dutch*. He adds, that he gueſſes the Occaſion of his Miſtake to be thus: The Skin of new-born Infants, in ſome certain Places, if it be carefully ſeparated from all Fat, is ſo fine and thin, that it becomes like the *Epidermis* in Adults. This no doubt is extremely full of Veſſels, and is what he has imagined to impoſe upon Mr. *St. Andre*.

§ IX. He treats of human Bones, in which Search he has long laboured: He obſerves the *Epiphyſes* upon the Ends of the Bones are chiefly faſtened to them by the *Perioſteum*; which being taken off, they eaſily fall aſunder. 'Tis a Miſtake, that we vulgarly think this is done by means of an intermediate Cartilage: The Truth is, both the *Epiphyſes* and Ends of the Bones, at firſt, are cartilaginous; but in Time become bony, and then are only joined by the *Perioſteum*. The Oſſification always begins in the middle of Bones. Sometimes he has found human Bones ſo void of Cavities, that he has made Knife-Handles of them: It has been thought that cartilaginous Parts were exſanguious; but falſly, as he demonſtrates by ſeveral Preparations thereof.

§ X. He conſiders the *Papillæ* of the nervoſe Tunic in the Inſide of the Stomach, and finds that the Figure of them is roundiſh, in that Part of the Stomach over which the Spleen is fixt; but longiſh in other Parts thereof, and in the Inſide of the *Jejunum*. He takes Notice, that the Fabrick of the Womb is made of much the ſame Sort of thick muſcular Fibres as the Bladder, both to facilitate their proper Excluſion; particularly theſe Fibres are more remarkable upon the *Fundus Uteri*, to which Part the *Placenta* moſt generally faſtens its ſelf, and moſt evidently for this Reaſon.

FINIS.

ERRATUM.

P. 402. *l.* 9. *pro* indicium *l.* judicium.

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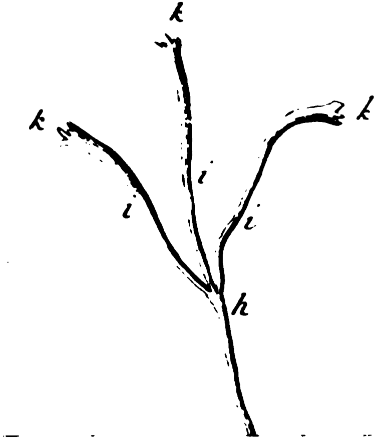

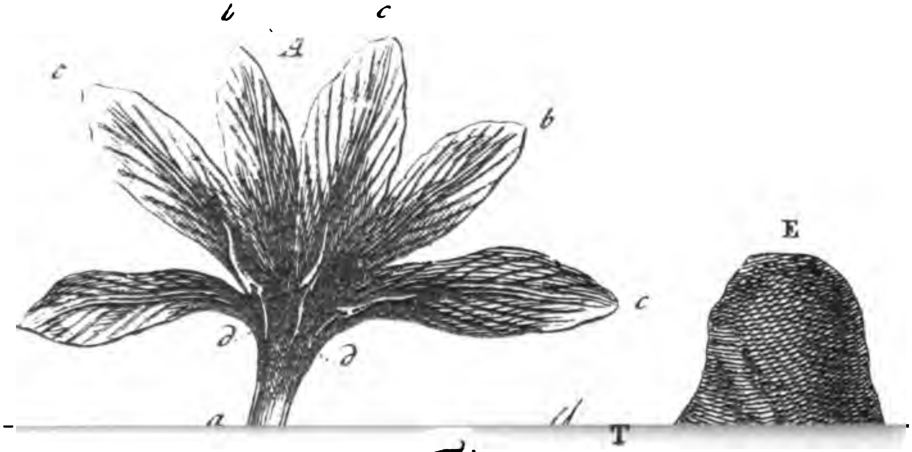

Fig. 5.

Numb. 380.

PHILOSOPHICAL TRANSACTIONS.

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I. *A Letter from Mr.* David Martineau *Surgeon in* Norwich, *to Sir* Hans Sloane, *Bart.* Coll. Med. Lond. Pr. S. R. V. Pr. *Concerning Stones voided* per Anum.

Norwich, Oct. 5th 1723.

THESE Stones (*Vid. Fig.* 1, 2, 3, 4, 5.) were voided by a poor Woman, *per Anum*, on the 26, 27, and 28th of *March* laſt paſt, who was then pregnant about the 11, or 12th Week, according to Computation. I was call'd in the Night to this poor Woman on the 23d, who in Appearance was in the Extremity of a Convulſion Fit, attended with violent Vomitings, which when over, ſhe complain'd of great Pain in her Back, from her Reins downward to the *Anus*, upon which I blooded her, order'd ſome *Anti-Emeticks*, and left her: She continued with much Pain the whole Day; on the 24th, in the Night, her Fits return'd again with double Force, her Pains alſo increaſing like unto Labour-Pains, put me upon further Inquiry, but nothing appear'd more than ordinary; the Extremity of Pain drove me to the Uſe of Glyſters, which was attempted, but none could be thrown up, although repeated by one and another of the moſt experienced Nurſes, upon which I gave her a gentle Draught, which ſhe obſerved increas'd her Pain with a ſtrong *Teneſmus*, that continued near three Hours before the largeſt Stone appear'd, which ſtopp'd at the upper Part A, D, *Fig.* 1. but was quickly remov'd by preſſing the lower Protuberance C, upon which, with a plentiful Diſcharge, ſhe had

Eaſe, ſlept ſome Hours, waking with a Deſire to ſtool, and voided with it the 2d and 3d; and the next Day, at four Hours Diſtance, the other two. She recovered perfectly, and was deliver'd of a very fine living Girl, on the 24th of *Auguſt* paſt. Upon Inquiry, it ſeems, ſhe had been frequently troubled, for fourteen Years paſt, with Pains in her Side and Stomach without Vomitings; which is all ſhe remembers.

The Largeneſs of the Stones, their Seat and Subſtance, is what to me ſeems worthy Speculation; their being all alike in Colour and Weight, according to their Dimenſions, is the Reaſon I broke but one.

Wt. of the Stones.				Their Dimenſions in Circumference.			
	oz.	*dwts.*	*gr.*		*Inch.*		*Inch.*
Fig. 1.	2	16	12	A. B. C. D.	8	A. D. the top of the Stone oval.	6 $\frac{5}{8}$
Fig. 2.		8	12	E. F. G. H.	5 $\frac{1}{8}$	E. G.	4 $\frac{5}{8}$
Fig. 3.		7	3	I. K. L. M.	5	I. L.	3 $\frac{5}{8}$
Fig. 4.		7	12	N. O. P. Q.	4 $\frac{6}{8}$	N. P.	3 $\frac{7}{8}$
Fig. 5.		5	13	R. S. T. U.	4 $\frac{3}{8}$	R. T.	4 $\frac{1}{8}$

1. A Shell taken of the Stone at the End 2. The Stone and Shell being like a Cheſnut ſoft and like Wool, cutting into the Stone at 3, I found it hard.

II. *Epiſtola Viri Doctiſſimi* Johannis Hoogvlietii *ad* Jacobum Jurin, *M. D. R. S. Secr. De Epiſtolis duabus Poſthumis Viri Celeberrimi* Antonij a Leeuwenhoek, *R. S. S.*

SEnex noſter venerandus *Leeuwenhoekius*, jam in agone Mortis verſans, ac nihilominus artis ſuæ memor, me ad ſe vocari juſſit, attollenſque oculos jam gravatos morte, verbis ſemiabruptis me rogabat, vellemne haſce binas literas ex vernaculo in Latinum Sermonem vertere, tibique, Vir Ampliſſime, mittere. Ut ergo hiſce tanti viri, quo abhinc jam aliquot annos uſus fueram familiariſſime, parerem præceptis, non poſſum, quin tibi, Vir eruditiſſime, hoc extremum, Viri mihi amiciſſimi morientis, munus mittam, ſperans fore ut hæc ultima ejus conamina tibi grata ſint futura.

Delphis in Batavis,
pridie Nonas Sept.
1723.

III. *De Globulis in Sanguine & in Vini Fœcibus. Epiſtola poſthuma Domini* Antonij a Leeuwenhoek, *Societatis Regiæ Londinenſis, dum viveret, Sodalis digniſſimi, ad* Jacobum Jurin, *R. S. Secr.*

Delphis in Batavis.

EX proximis tuis literis Septimo Kalendas *Julii* datis, innotuit mihi, nobiliſſimos D. D. Regiæ Societatis, meas tres ultimas epiſtolas perlibenter accepiſſe, quod mihi gratiſſimum erat intellectu; Tibique, Vir doctiſſime, placuiſſe, tuas obſervationes de globulorum ſanguineorum magnitudine, meis obſervationibus congruere. Porro, dicis, Vir doctiſſime, non credo hanc explorationem fore indifferentem, ſeu inutilem, quoniam non impoſſibile eſt, quin illa obſervatio de globulorum ſanguineorum magnitudine in variis animalibus, una cum ſuis parvulis vaſculis, ductura ſit nos aliquando ad detectionem ubi, & quibus mediis hi globuli formentur, & inde determinatam craſſitudinem in eodem animali conſervent.

Deinde dicis, mi doctiſſime, hoc ampliori tua exploratione bene dignum eſt.

Ad hæc mihi aſſumo tibi, Vir eruditiſſime, objicere, me ad imaginationem eam adduci, nos nunquam ad explorationem illam perventuros, quomodo ſcilicet hi globuli ex materia fluida ad magnitudinis determinatæ globulos ſint coagulaturi.

Multis abhinc annis ad me aſportari juſſi vinum fermentans, quia hoc Vinum globulos producit fere ſanguineis æquales, quos vini fœces vocamus: & quamvis tale vinum microſcopio apponerem, tamen nil aliud videbam, quam innumerabilem multitudinem parviſſimorum globulorum aëreorum, ſurſum ad ſuperficiem vini adſcendentium, & ſecum illos globulos, quos vini fœces nominamus, ducentium. Cum jam in ſuperficiem vini venerant, ſeparabantur vini fœces a globulis aëris, & fundum repetebant, mihi non apparente, quomodo globuli, quos dicimus vini fœces, formabantur: & ſimulac, ni fallit memoria, globuli illi inferius deſcendiſſent, exibat ex globulo quoque iterum globulus aëreus, & ita denuo ad ſuperficiem Vini devehebatur.

Imaginor mihi, coagulationem illam, ac determinatam globulorum ſanguineorum magnitudinem à rerum primordiis eſſe creatas; nam ſi unus globulus altero major erat, ſuſpicamur exinde inordinatam circulationem fore ſecuturam, quoniam ſanguinem per varia vaſcula ſanguinea rapidè transfluentem vidi, quæ adeo exilia erant, ut ſimplex ſaltem globulus tranſire potuerit.

IV. *Ejuſdem Viri Clariſſimi ad eundem Epiſtola poſthuma. De Generatione Animalium, & de Palpitatione Diaphragmatis.*

Delphis in Batavis.

INTER alia ex tuis proximis literis, ſeptimo Kalendas *Julii* datis, intellexi, Sententiam meam de generatione tibi videri firmiſſimam omnium, hactenus in lucem editarum; & quod mihi non incognitum erat, quoſdam Anatomicos ſupra mea fundamenta novum ſtruere Syſtema auſos eſſe, affirmantes ovum eſſe proprium nidum, in quo animalculum ſeminis maſculini hoſpitatur & quod idem ovum ex eo impraegnatum, poſtea per tubam *Fallopianam* deducitur ab ovario ad uterum.

Multos expectavi contradicentes, & doctiſſimus quidam dominus dicebat, poſtquam meum Syſtema de generatione legiſſet, recte putas, ſed vita tua convincendis omnibus non ſufficiet; alius dicebat, me mendacia de animalculis memoriæ prodere.

Paulo ante obitum Domini *Leibnitz*, ſcribebat ille Dominus mihi *Hannonia*, ſe meam ſententiam de generatione amplexiſſe, ſeque in libro quodam, a ſe edito, idem demonſtraſſe, ſed quendam dominum in *Italia* eſſe, qui contra meum Syſtema de generatione aliquid eſſet editurus; atque in *Germania* dominum eſſe, qui cum illo communicaverat totum ovarium in tuba *Fallopiana* repertum eſſe; Profeſſoreſque in *Germania* inveniri, qui animalcula ſeminis maſculini eſſe negabant. Verum enimvero, quamvis contradictiones quampluri mas paſſus ſim, & adhuc patiar, permanebo in Syſtemate meo, quoniam animalcula in omnigenis ſeminibus inveni, ſi ſaltem animalia rite tractare poſſem, ne exceptis quidem variis avium ac piſcium ſpeciebus.

Porro comperimus in omnibus arborum & plantarum ſeminibus, etiamſi parviſſimis, ſi modo tractari poſſint, plantam eſſe formatam, quam plantam antea comparavi cum animalculis ſeminis maſculini. Et farinoſa materia in ſeminibus plantarum plantis nutrimento eſſe debet tamdiu, quàm planta ſuos ejecerit radices, & ex terra ali poſſit.

Doctiſſime Vir, concede mihi libertatem dicendi, me percipere non poſſe, quomodo poſitiones adeo vanas ſtatuere, eaſque in lucem edere audeant, quod animalcula ſeminis maſculini in utero vel tuba *Fallopiana* effuſi, poſtea per tubam *Fallopianam* deducerentur ab ovario ad uterum. Hæ poſitiones ſunt adeo omni fundamento carentes, ut ulteriori contradictione indignæ ſint.

Ulterius dicis, eruditiſſime Vir, meam priſtinæ ſanitati reſtitutionem tibi gratam fuiſſe, pro qua benignitate erga me gratias ago maximas, & tibi dicere cogor, me multis ab hinc annis pro certo habuiſſe, quod diaphragma inſtrumentum ſit adeo magnum, ut totum abdomen continue moveat, quo motu cibi in ventriculo & inteſtinis adeo conteruntur, ut in materiam tam fluidam redigantur, quæ apta eſt, ut intret parvula illa vaſcula ſanguinea, quæ multa in inteſtinorum cavitatibus ſita ſunt: non autem per oſcula illorum vaſculorum, ut quidam hominibus perſuadere conati ſunt, ſed me judice per tenues tunicas illorum parviſſimorum vaſculorum ſanguineorum.

Si jam ponamus arteriam in corpore bene conſtituto, intra horæ unius ſpatium ter millies & ſexcenties pulſare, quatuorque pulſus fieri in una reſpiratione, computabimus nongentas reſpirationes ſpatio unius horæ, ac toties fiet preſſio ventriculi & inteſtinorum, quam voco ſubactionem in ventriculo & inteſtinis contentorum.

Sit mihi quoque venia tibi, doctiſſime Vir, dicendi, quod duos abhinc menſes varias lentas palpitationes per diaphragma ſenſerim: quare cogitabam de tuba quadam vitrea, à me inventa, ad ſpiritum cujuſdam liquoris in pectus ducendum, ut experimentum facerem, num inſolitus ille diaphragmatis motus, quem cordis palpitationem vocant, ſedari poſſet.

Implevi ergo meam tubam ad altitudinem trium tranſverſorum digitorum ſpiritu vini, in quo jam diu hæc aromata fuerant infuſa, nempe nuces moſchatæ, macis, & parum garyophillorum, ſed cinnamomi multum, ut & croci, & inſpiravi aërem tranſeuntem per hunc ſpiritum vini, & quidem non modeſtè, ſed fortiter aërem in pulmonem impuli, quo diaphragma, quod fortis membrana eſt, valde extendebam, experiendi gratia, num palpitationem, vel inordinatum diaphragmatis motum, hoc facto componere poſſem, quod illo tempore bene ſucceſſit: Sed poſtea quidem evenit, ut experimentum hoc, quamvis aërem tam fortiter in pectus inducerem, ut preſſus aër cum tuſſi ex guttere erumperet, expectationem meam eluſerit.

Sententia tua mihi longe viſu erat gratiſſima, nempe quod diaphragmatis palpitatio validiori niteretur fundamento, quam illa cordis.

I

V. *A Botanical Deſcription of the Flower and Seed-Veſſel of the Plant, called* Crocus Autumnalis Sativus, *that produces the true* Engliſh Saffron *of the Shops: With a Figure. By* Dr. James Douglas, *Honorary Fellow of the* College *of* Phyſicians, *and Fellow of the* Royal Society.

THE Flower of this curious Plant is of the Lilly Kind, monopetalous, infundibuliformis, without any *Calyx* or *Perianthium*, its long fiſtulous Beginning being afterwards expanded into ſix beautiful oblong Segments. *Vid. Lit.* A. A. *Fig. 6.*

It conſiſts of the following Parts, *viz.* the *Petalum*, the *Stamina*, the *Apices*, the *Ovarium*, and the *Stylus*; with its *Capillamenta Tubiformia*, of all which in Order.

PETALON.

The *Petalum* is diſtinguiſhed into a long, hollow, or fiſtulous Part, which lies incloſed within the common and proper *Involucra* of the Plant, ariſing from the Top of the *Ovarium*, or *Seed-Veſſel*, and ſix Segments. *Vid. Lit. a. a.*

While this narrow, tubulous Part runs between the Leaves and Integuments that ſurround them, it is of a white Colour; but, aſſoon as it is diſengaged from theſe, it inſenſibly acquires a Purple Colour, which, a little before its Diviſion, as it begins to be enlarged, and grow more open, inclines to a Red.

This

This narrow fiſtulous Part of the Flower, about 1 or 2 Inches above the *Theca Communis*, forms ſix foliaceous Segments, or divides into ſo many oblong Purple-coloured *Petala*. *Vid. Lit. b. c.*

Three of theſe Flower-Leaves are larger than the other three, but, in all other Reſpects, are much alike. *Vid. Lit. c. c. c.*

The Length of the largeſt is from 1 ¼, to 2 Inches; the Breadth ſeldom above ½ Inch: The ſhorter Leaves are from 1, to 1 ¼ Inch in Length, their Breadth being ſomething leſs in Proportion.

The Inſide of each *Petalum* is of a Violet Purple-Colour, vein'd with a few ſmall Lines of a deeper Dye, running length-ways, intermixed with White, or the whole is beautifully checquered with Blue and White Colours.

The Outſide is of a whiter Blue, with ſeveral whitiſh Riſings or Ridges, and juſt at the Bottom of the Leaf it is of a deeper Blue; the three ſmall Leaves (*Vid. Lit. b. b. b.*) are much of the ſame Colour, only the Purple ſeems to be ſomething deeper.

The Number of the *Petala* is, for the moſt Part, 6; yet, in ſome Flowers we may obſerve 7, or 8; but then they are not ſo large as when they are fewer.

STAMINA.

In every Flower there are three *Stamina*, or Chives, (*Vid. Lit. d. d. d.*) properly ſo called, ariſing from the inner Surface of the tubular Part of the Flower, juſt before its Diviſion into the *Petala*, where they make, for ſome Space, an apparent Ridge, and then they ſtand upright, oppoſite to the three large Leaves; they are of a whitiſh Colour, inclining to a light Purple, being but little above $\frac{1}{4}$ of an Inch in Length.

In thoſe Plants that have 7 or more *Petala*, the Number of the *Stamina* is likewiſe encreaſed to 5, or more.

APICES.

Each of theſe *Stamina* has its proper *Apex*, (*Vid. Lit. e. e. e.*) which is a peculiar longiſh Subſtance, ſtanding upright, oppoſite to the larger Flower-Leaves, of a Yellow Colour, forked, not unlike the Tongue of a ſmall Bird, at the lower End, where it is fixed to the *Stamen*; here likewiſe it is broadeſt, but as it aſcends it becomes narrower, and its upper Extremity is a little crooked, or turned to one Side: It ſeldom exceeds $\frac{1}{2}$ Inch in Length.

It appears as if it was double, with a longitudinal Furrow in the middle, in which Hollow the *Stamen* ſeems to be faintly continued for ſome Space.

When the Plant is full grown, they are all loaded with that Kind of ſmall Duſt called *Farina fœcundans.*

OVARIUM.

Called likewiſe the *Vaſculum Seminale*, and *Piſtillum* by ſome. *Vid. Lit. f. f.*

It is a particular Body, which ariſes from the Top of the *Pedunculus*, (*Vid. Lit. g.*) about $\frac{1}{2}$ of an Inch long, or a little more, of a deep White Colour, three cornered, and divided into three *Loculamenta*, or *Capſulæ*, in which the Seeds (which however ſeldom come

to

to Perfection with us) are formed, growing bigger and bigger after the Flower falls off; nay, even in this blooming State, if you cut this Veſſel acroſs the middle, you may perceive the whitiſh Rudiments of the Seeds.

STYLUS.

From the upper Part, or *Apex*, of the *Ovarium*, ariſes the *Stylus*, (*Vid. Lit. b.*) which is a long ſlender Tube, that lies encloſed within the tubulous or fiſtular Portion of the Flower, being there of a whitiſh Colour, but changing into a Yellow before its Diviſion.

This Stylet ordinarily ſplits into 3 Parts, juſt oppoſite to the Top of the *Stamina*, where the *Apices* take their Riſe, (*Vid. Lit. i, i, i.*) and thus far it ſtands upright in the Middle of the *Stamina*; for the ſake of which only, this Plant is cultivated, and being prepared, (as ſhall hereafter be deſcribed) makes the true Saffron of the Shops, which we ſo frequently uſe in Phyſick.

I call theſe Parts of the *Stylus*, from their Figure and Shape, *Capillamenta Tubiformia*, or, *Appendices Styli Salpingoeides*, in as much as they very exactly repreſent a Trumpet, and are not unlike the *Falloppian Tube* in Women, being narrow at their Origin, and growing gradually larger towards their other Extremity; which, like the common Trumpet, is open and expanded; and, like the *Tuba Falloppii*, is jagged, or fringed at the Edge. *Vid. Lit. k, k, k.*

It may alſo be very fitly named *Crocus officinarum*, becauſe that is the Part only that is uſed in the Shops.

They are of a Yellow Colour, juſt at their Beginning, continued from the upper Part of the *Stylus*, but af-

afterwards they are all of a deep Red Colour, only their jagged Extremities are tipt with a White inclining to a Yellow.

Theſe *Tubæ*, or *Capillamenta*, are from 1, to 1 ½ Inch in Length.

The *Stylus*, while undivided, is ſtrong enough to ſupport itſelf, being encloſed within the tubular Part of the Flower; but the *Capillamenta* being very weak and ſlender at their Beginning, this occaſions them to hang down between the *Petala.*

FIGURE 6.

Repreſents a Root of the Saffron-Plant, that has two Stalks encloſed in one common *Vagina*, the whole Flower with the *Stamina* and *Apices* in one, and the *Stylus* only in the other; with the Leaves, *Pedunculi*, and *Ovarium* in both, being fairly delineated from the Life.

A A. The Flower expanded into ſix beautiful Segments.
a a. The Fiſtulous Part of the Flower.
b. c. The ſix Petala.
c. c. c. The three larger Petala.
b. b. b. The three ſmaller ones.
d. d. The three Stamina, *or Chives.*
e. e. e. The three Apices.
f. f. The Vaſculum Seminale.
g. g. The Pedunculi.
h. h. The Stylus.
i. i. i. The three Capillamenta Tubiformia.
k. k. k. The jagged Extremity of the Capillamenta.
B. B. The Root.
C. C. The Graſſy Leaves.

VI. *Some*

VI. *Some Account of Mr.* Leeuwenhoek's *curious* Microſcopes, *lately preſented to the* Royal Society. *By* Martin Folkes, *Eſq; Vice-Preſident of the* Royal Society.

IT is now above 50 Years, ſince the late *Mr. Leeuwenhoek* firſt began his Correſpondence with the Royal Society; when he was recommended by *Dr. Regnerus de Graaf*, as a Perſon already conſiderable by his Microſcopical Diſcoveries, made with Glaſſes contrived by himſelf, and excelling even thoſe of the famous *Euſtachio Divini*, ſo much talk'd of in the learned World: And as he has ever ſince that Time apply'd himſelf, with the greateſt Diligence and Succeſs, to the ſame Sort of Obſervations, no Doubt can be made of the Excellency of thoſe Inſtruments he ſo long us'd, ſo much improv'd, and upon the fulleſt Experience ſo often commended in his Letters; great Part of which, at his Deceaſe, he thought fit to bequeath to this Society, for whom he ever expreſs'd the greateſt Eſteem and Reſpect.

He had, indeed, intimated this Deſign in ſeveral of his Letters, and in his laſt Will and Teſtament gave Orders, that the Glaſſes ſhould be delivered as ſoon as conveniently might be after his Deceaſe; which was accordingly done, by the Directions of his ſurviving Daughter, *Mrs. Maria Van Leeuwenhoek*, to whoſe great Care we are oblig'd, for the ſafe and ſpeedy Delivery of this very curious and valuable Preſent.

The Legacy conſiſts of a ſmall *Indian* Cabinet, in the Drawers of which are 13 little Boxes or Caſes, each containing two Microſcopes, handſomely fitted up in Silver, all which, not only the Glaſſes, but alſo the *Apparatus* for managing of them, were made with the late *Mr. Leeuwenhoek*'s own Hands: Beſides which, they ſeem to have been put in Order in the Cabinet by himſelf, as he deſign'd them to be preſented to the Royal Society, each Microſcope having had an Object placed before it, and the Whole being accompany'd with a Regiſter of the ſame, in his own Hand-Writing, as being deſirous the Gentlemen of the Society ſhould, without Trouble, be enabled to examine many of thoſe Objects, on which he had made the moſt conſiderable Diſcoveries.

Several of theſe Objects yet remain before the Microſcopes, tho' the greater Number are broken off, which was probably done by the ſhaking of the Boxes in the Carriage. I have, nevertheleſs, added a Tranſlation of the Regiſter, as it may ſerve to give a juſter Idea of what *Mr. Leeuwenhoek* deſign'd by this Legacy, and alſo be of Uſe, by putting any curious Obſerver in Mind of a Number of Minute Subjects, that may in a particular Manner deſerve his Attention.

The 13 Caſes abovemention'd are numbered from 15 to 27 incluſively, correſponding to which is the Regiſter of the Objects, Two to every Caſe, as follows.

No. 15. Globules of Blood, from which its Redneſs proceeds.

A thin Slice of Wood of the Lime-Tree, where the Veſſels conveying the Sap are cut tranſverſely.

N°. 16.

The Eye of a Gnat.

N°. 17. A crooked Hair, to which adheres a Ring-Worm, with a Piece of the Cuticle.

A ſmall Hair from the Hand, by which it appears thoſe Hairs are not round.

N°. 18. Fleſh of the Codfiſh (*Cabeljaeuw*) ſhewing how the Fibres lie oblique to the Membranes.

An Embrio of Cochineal, taken from the Egg, in which the Limbs and Horns are conſpicuous.

N°. 19. Small Pipes, which compoſe the Elephant's Tooth.

Part of the Cryſtalline Humour, from the Eye of a Whale.

N°. 20. A Thread of Sheeps-Wool, which is broken, and appears to conſiſt of many leſſer Threads.

The Inſtrument, whence a Spider ſpins the Threads, that compoſe his Web.

N°. 21. A Granade, or Spark made in ſtriking Fire.

The Veſſels in a Leaf of Tea.

N°. 22. The *Animalcula in Semine Maſculino*, of a Lamb taken from the Teſticle, *Jul.* 24. 1702.

A Piece of the Tongue of a Hog, full of ſharp Points.

N°. 23. A Fibre of Codfiſh, conſiſting of long ſlender Particles.

Another of the ſame.

N°. 24. A Filament, conveying Nouriſhment to the Nutmeg, cut tranſverſly.
Another Piece of the ſame, in which the Figure of the Veſſels may be ſeen.

N°. 25. Part of the Bone or Tooth abovementioned, conſiſting of hollow Pipes.
An exceeding thin Membrane, being that which cover'd a very ſmall Muſcle.

N°. 26. Veſſels by which Membranes receive Nouriſhment and Increaſe.
A Bunch of Hair from the Inſect call'd a Hair-Worm.

N°. 27. The double Silk, ſpun by the Worm.
The Organ of Sight of a Flie.

It were endleſs to enter into any Particulars, of what is to be obſerved in any of theſe Objects, or indeed to give any Account of *Mr. Leeuwenhoek*'s Diſcoveries; they are ſo numerous as to make up a conſiderable Part of the *Philoſophical Tranſactions*, and when collected together, to fill four pretty large Volumes in Quarto, which have been publiſh'd by him at ſeveral Times: And of ſuch Conſequence, as to have opened entirely new Scenes in ſome Parts of *Natural Philoſophy*, as we are all ſenſible, in that famous Diſcovery of the *Animalcula in Semine Maſculino*, which has given a perfectly new Turn to the Theory of Generation, in almoſt all the Authors that have ſince wrote upon that Subject.

For the Conſtruction of theſe Inſtruments, it is the ſame in them all, and the *Apparatus* is very ſimple and convenient: They are all ſingle Microſcopes, conſiſting each of a very ſmall double Convex-Glaſs, let into a Socket, between two Silver Plates rivetted together, and pierc'd with a ſmall Hole: The Object is placed on a Silver Point, or Needle, which, by Means of Screws of the ſame Metal, provided for that Purpoſe, may be turn'd about, rais'd, or depreſs'd, and brought nearer, or put farther from the Glaſs, as the Eye of the Obſerver, the Nature of the Object, and the convenient Examination of its ſeveral Parts may require.

Mr. *Leeuwenhoek* fix'd his Objects, if they were ſolid, to this Silver Point, with Glew; and when they were Fluid, or of ſuch a Nature as not to be commodiouſly view'd unleſs ſpread upon Glaſs, he firſt fitted them on a little Plate of Talk, or exceſſively thin-blown Glaſs, which he afterwards glewed to the Needle, in the ſame Manner as his other Objects.

The Obſervation, indeed, of the Circulation of the Blood, and ſome others, require a ſomewhat different *Apparatus*, and ſuch a one he had, to which he occaſionally fix'd theſe ſame Microſcopes; but as it makes no Part of this Cabinet, I ſhall omit giving any farther Account of it, only taking Notice that it may be ſeen in a Letter to the *Royal Society*, of the 12th of *January*, 1689. and printed in his *Arcana Naturæ Detecta*, Nº. 69. But I was willing to mention juſt ſo much, as it may ſerve to ſhew the univerſal Uſe of theſe Microſcopes, and as it induces me (among other Things) to believe, theſe were the Kind of Microſcopes generally, if not ſolely, us'd by this curious Gentleman in all his Obſervations, and to which we are oblig'd for his moſt ſurprizing Diſcoveries.

Another Particular, to the ſame Purpoſe, I would not omit, and that is, That upon the late Queen *Mary*'s doing Mr. *Leeuwenhoek* the Honour of a Viſit at *Delft*, and viewing his Curioſities with great Satisfaction, he preſented her with a Couple of his Microſcopes, which, as I have been inform'd by one who had them a conſiderable Time in his Hands, were of the ſame Sort as theſe, and did not any ways differ from one of the 13 Caſes contain'd in the Drawers of this Cabinet.

The Glaſſes are all exceedingly clear, and ſhew the Object very bright and diſtinct, which muſt be owing to the great Care this Gentleman took, in the Choice of his Glaſs, his Exactneſs in giving it the true Figure; and afterwards, amongſt many, reſerving ſuch only for his Uſe, as he, upon Tryal, found to be moſt excellent. Their Powers of magnifying are different, as different Sorts of Objects may require; and, as on the one Hand, being all ground Glaſſes, none of them are ſo ſmall, and conſequently magnify to ſo great a Degree, as ſome of thoſe Drops, frequently us'd in other Microſcopes; yet, on the other, the Diſtinctneſs of theſe very much exceeds what I have met with in the Glaſſes of that Sort; and this was what Mr. *Leeuwenhoek* ever principally propos'd to himſelf, rejecting all thoſe Degrees of magnifying in which he could not ſo well obtain that End; for he informs us in one of his Letters, where he is ſpeaking of the exceſſive Praiſe ſome give to their Glaſſes on this Account, that although he had above Forty Years had Glaſſes by him of an extraordinary Smallneſs, he had made but very little Uſe of them; as having found, in a long Courſe of Experience, that the moſt conſiderable Diſcoveries were to be made with ſuch Glaſſes as, magnifying but moderately, exhibited the Object with the moſt perfect Brightneſs and Diſtinction.

But

But however excellent theſe Glaſſes may be judg'd, Mr. *Leeuwenhoek*'s Diſcoveries are not entirely to be imputed to *their* Goodneſs only: His own great Judgment, and Experience in the Manner of uſing them, together with the continual Application he gave to that Buſineſs, and the indefatigable Induſtry with which he contemplated often and long upon the ſame Subject, viewing it under many and different Circumſtances, cannot but have enabled him to form better Judgments of the Nature of his Objects, and ſee farther into their Conſtitution, than it can be imagined any other Perſon can do, that neither has the Experience, nor has taken the Pains this curious Author had ſo long done.

Nor ought we to forget a Piece of Skill, in which he very particularly excell'd, which was that of preparing his Objects in the beſt Manner, to be view'd by the Microſcope; and of this I am perſwaded, any one will be ſatisfied, who ſhall apply himſelf to the Examination of ſome of the ſame Objects as do yet remain before theſe Glaſſes; at leaſt, I have my ſelf found ſo much Difficulty in this Particular, as to obſerve a very ſenſible Difference between the Appearances of the ſame Object, when apply'd by my ſelf, and when prepared by Mr. *Leeuwenhoek*, tho' view'd with Glaſſes of the very ſame Goodneſs.

I have the rather inſiſted upon this, as it may be a Caution to us, that we do not raſhly condemn any of this Gentleman's Obſervations, tho' even with his own Glaſſes, we ſhould not immediately be able to verify them our ſelves. We are under very great Diſadvantages for want of the Experience he had, and he has himſelf put us in Mind, more than once, that thoſe who are the beſt skill'd in the Uſe of Magnifying-Glaſſes, may be miſled, if they give too ſudden a Judgment upon what they ſee, or 'till they have been aſſured from repeated Experiments. But we have ſeen ſo many, and thoſe of his moſt ſurprizing Diſcoveries, ſo perfectly confirm'd, by great Numbers of the moſt curious and judicious Obſervers, that there can ſurely be no Reaſon to diſtruſt his Accuracy in thoſe others, which have not yet been ſo frequently or carefully examin'd.

Upon the whole, it is to be hoped, ſome of the Society will purſue thoſe Enquiries, the late Poſſeſſor of theſe Microſcopes was ſo deſervedly famous for; and that as we have loſt in Mr. *Leeuwenhoek* a moſt worthy Member, and a moſt valuable Correſpondent, this laſt Piece of his Reſpect to the *Royal Society* will not only enrich our Repoſitory, but both encourage and enable ſome other diligent Obſerver to proſecute the ſame curious and uſeful Diſcoveries.

VII. *The Bills of Mortality, &c. of ſeveral conſiderable Towns in* Europe. *Beginning with the Year* 1717, i. e. *from* Chriſtmas 1716, *to* Chriſtmas 1717. *Extracted from the* Acta Breſlavienſia. *By* Conr. Joach. Sprengell, *M. D. R. S. S. & Coll. Med. Lond. Lic.*

THE Uſefulneſs of this Sort of Regiſters is now ſo well known to the Learned World, from the Labours of many curious and inquiſitive Perſons in PoliticalArithmetick, that we conceive the following will need no Recommendation to render them acceptable to the Publick.

Breſlaw, *in* Sileſia.

Buried.	
In January,	91
February,	113
March	136
April	108
May	127
June	144
July	144
Auguſt	208
September	128
October	96
November	113
December, only till the 24th,	77
	1485

Chriſtened.	
Males	584
Females	576
	1160

Among the Buried were		
Married Men		226
Married Women		144
Widows & Widowers		157
Bachelors		60
Maidens		57
Boys	under Ten Years old,	419
and Girls		397
Stillborn	Boys	37
	Girls	17
		1514

N. B. Here

N. B. Here, in the Sum Total, are 29 more than in the Number of the Buried above, by Reaſon of ſeveral Stillborn Children which had no regular Burial, and ſo were not reckon’d.

In the Imperial Town Vienna, *there died,*

Young People———3179
Old ——————— 2026

Total —— 5205

Among which were,

Of 90 Years old	2	Of 99	2
91	1	100	1
92	3	102	2
93	1	103	1
94	3	104	1
95	2	115	1
96	3		

Born and Chriſtened————4030 Children.

In *Dreſden, Saxony,* died —— —— 1908
Chriſten’d —— —— 1443

In *Dantzick* died—————— 1605
Chriſtened—————— 2102

In *Eſperies,* or *Eperies,* alias *Epperies,* a Town in Upper *Hungary,* died ———— 132
Chriſten’d ———— 157
Moſt of theſe died of the Small-Pox.

The

The Bills *of* Mortality *of the Year* 1718. i. e. *from* Chriſtmas 1717, *to* Chriſtmas 1718.

BRESLAW.

Buried.	
From the 25th of Dec. to the 31ſt.	21
January	129
February	112
March	108
April	98
May	88
June	97
July	86
Auguſt	118
September	98
October	119
November	100
December, till the 24th,	81
Total	1255

Chriſtened.	
Males	598
Females	554
	1152

Among the Dead were,		
Married Men		238
—— Women		141
Widows & Widowers		122
Bachelors		60
Virgins		60
Children to ten Years of Age,	Boys	282
	Girls	280
Stillborn	Boys	47
	Girls	25
		1255

In *Vienna* died—Men—1432— Women —1129
Boys—1844— Girls —1705

In all———6110.

Amongſt which were froze to Death in the Street	2
Murder'd	2
Kill'd by a Waggon	2
By Falls from Houſes, &c.	11
Kill'd with Swords, Knives and Sabres,	9
By taking wrong Phyſick	1
Trod to Death by Horſes	4
Drowned	5
Beat to Death	1
Smother'd in a Houſe of Office	4
Cut their Throats	2
Kill'd by the Fall of Houſes	5

Among the Dead were found,

Perſons	Years old.
8	90
7	92
2	93
3	94
3	95
1	96
2	97
2	98
2	99
1	105

Chriſtened.

Males	2185
Females	2057
Total	4242

Amongſt which were 48 Pair of Twins, and once *Tergemini.*

In *Ratisbon* died of the *Lutherans*, 235 Perſons; amongſt which were 117 Children, 13 young Men, and 11 young Women.

The following Liſt *gives an Account of ſeveral Cities in* SAXONY.

	Chriſtened	*Baſtards.*	*Buried.*	*Pair Married.*	*Communicants.*
In Dreſden	1578	99	1412	501	78875
Wittemberg	286	16	317	61	13536
Leipzick	861	68	953	303	
Torgau	136	9	148	54	7917
Freyberg	340	13	373	104	24098
Stollberg	88		112	27	7314
Pulſnitz	73		75	39	4381
Konigsbruck	67		95	16	3351
Elſtra	63		45	11	3021
Bautzen	207	5	135	55	14520
Annaberg	146	19	112	37	8426
Chemnitz	173	4	166	38	10690
Oſchatz	103	3	82	20	5169
Altenburg	141		248	57	12901
Eulenberg	96	5	86	41	6194
Pirna	138	2	133	43	9164
Marienberg	91		194	18	5903
Georgenſtad	182		191	36	3580
Harta, *near* Waldheim	40	3	29	20	3291

This preſents you with a LIST *for a whole Century, of the Numbers of People Married, Chriſtened, Buried, and Communicants,* viz. *from the Year* 1617, *to* 1717 *incluſive, in the Electoral City of* Freyberg, *a famous and pleaſant Town, near the Banks of the* Molde *in* Saxony. *Lat.* 51. *Long.* 32, 43. *about* 8 *Miles* S. W. *from* Dreſden.

Nº Years.	Pair Married.	Chriſtened.	Baſtards.	Buried.	Communicants
1617	89	539	4	430	10530
18	122	423	10	384	9631
19	124	495	13	371	9789
20	97	428	10	451	10093
21	106	430	6	507	9676
22	110	380	7	517	9536
23	100	357	8	589	9206
24	96	383	4	417	9509
25	98	375	7	454	10058
26	78	381	6	549	11642
27	108	372	7	331	11027
28	82	371	4	318	10745
29	87	412	19	536	11531
30	80	330	7	1114	11180
1631	135	353	4	284	10524
32	88	425	5	1343	11326
33	174	163	4	879	9131
34	170	375	1	175	7447
35	87	285	9	231	7323
36	61	299	11	181	7590
37	68	386	3	383	7723
38	53	262	9	151	7682

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Stollberg	88		112	27	7314
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Konigsbruck	67		95	16	3351
Elſtra	63		45	11	3021
Bautzen	207	5	135	55	14520
Annaberg	146	19	112	37	8426
Chemnitz	173	4	166	38	10690
Oſchatz	103	3	82	20	5169
Altenburg	141		248	57	12901
Eulenberg	96	5	86	41	6194
Pirna	138	2	133	43	9164
Marienberg	91		194	18	5903
Georgenſtad	182		191	36	3580
Harta, *near* Waldheim	40	3	29	20	3291

This preſents you with a LIST *for a whole Century, of the Numbers of People Married, Chriſtened, Buried, and Communicants,* viz. *from the Year* 1617, *to* 1717 *incluſive, in the Electoral City of* Freyberg, *a famous and pleaſant Town, near the Banks of the* Molde *in* Saxony. *Lat.* 51. *Long.* 32, 43. *about* 8 *Miles* S. W. *from* Dreſden.

Nº *Years.*	*Pair Married.*	*Chriſtened.*	*Baſtards.*	*Buried.*	*Communicants.*
1617	89	539	4	430	10530
18	122	423	10	384	9631
19	124	495	13	371	9789
20	97	428	10	451	10093
21	106	430	6	507	9676
22	110	380	7	517	9536
23	100	357	8	589	9206
24	96	383	4	417	9509
25	98	375	7	454	10058
26	78	381	6	549	11642
27	108	372	7	331	11027
28	82	371	4	318	10745
29	87	412	19	536	11531
30	80	330	7	1114	11180
1631	135	353	4	284	10524
32	88	425	5	1343	11326
33	174	163	4	879	9131
34	170	375	1	175	7447
35	87	285	9	231	7323
36	61	299	11	181	7590
37	68	386	3	383	7723
38	53	262	9	151	7682

Nº *Years.*	*Pair Married.*	*Chriſtened.*	*Baſtards.*	*Buried.*	*Communicants.*
1639	45	292	4	354	9609
40	65	268	6	173	7594
1641	61	226	2	163	7562
42	48	309	4	193	8128
43	60	383	7	340	7770
44	65	231	7	156	7699
45	62	196	5	163	7050
46	57	230	6	187	7263
47	59	201	2	136	7173
48	59	197	1	182	7698
49	44	222	5	201	7739
50	65	199	1	162	7639
1651	65	232	1	168	8148
52	50	190	2	202	7948
53	62	208	2	215	8342
54	62	234	3	211	8648
55	73	202	1	194	6351
56	60	237	5	151	8493
57	58	230		174	8730
58	61	198	4	184	8562
59	57	251	3	177	8431
60	66	230		240	8856
1661	47	225	3	217	8962
62	53	203	2	206	9423
63	47	206	2	247	9264
64	48	219	2	179	9279
65	61	213		207	9406
66	65	218	4	216	9979
67	79	223		306	9448

1668

Nº *Years.*	*Pair Married.*	*Chriſtened.*	*Baſtards.*	*Buried.*	*Communicants.*
1668	63	228	5	232	9588
69	55	232		204	9572
70	70	218	4	206	9564
1671	65	237	3	291	10080
72	64	207	2	286	9221
73	69	234	1	215	9912
74	55	228		204	9678
75	60	233	1	246	10093
76	58	226	1	277	9956
77	80	240	3	296	10462
78	70	256	4	302	10422
79	91	225	4	254	10356
80	63	292	4	267	11349
1681	87	285	5	218	11043
82	69	282	7	253	10894
83	77	315	8	333	11054
84	78	334	6	314	10623
85	73	249	4	251	11070
86	73	282	5	243	11488
87	80	327	6	292	11542
88	88	308	11	234	11300
89	59	276	4	301	11393
90	72	290	5	310	11380
1691	72	269	6	289	11724
92	58	284	7	296	12679
93	72	316	10	253	11597
94	65	236	7	355	12533
95	57	239	6	339	13196
96	85	277	10	290	14900

1697

N° *Years.*	*Pair Married.*	*Chriſtened.*	*Baſtards.*	*Buried.*	*Communicants.*
1697	76	216	4	180	15619
98	73	274	7	218	15677
99	64	264	5	258	16155
1700	74	251	7	310	18569
1701	81	283	10	188	19282
2	88	272	2	239	20383
3	95	271	6	241	20494
4	88	319	11	273	20837
5	72	295	12	357	20743
6	85	332	10	245	21813
7	82	310	18	378	21367
8	83	310	17	288	21838
9	71	302	10	305	21708
10	83	313	15	283	21699
1711	81	327	8	311	22334
12	82	310	9	325	22837
13	87	308	9	340	23627
14	70	349	6	237	23470
15	82	323	8	283	23273
16	105	317	14	361	23371
17	79	373	13	325	23904
Total	7546	28851	582	30295	1211761

Here is to be obſerved, That in the Years 1625, 26, 27, 32, and 33, the Plague raged very much; as alſo, that in the Year 1630, there died of the Plague meerly, 764. and in 1680, 103. Beſides that, amongſt the Chriſtened in 1713, a Girl was baptized, which was born and bred in *Turkey*, but from *Chriſtian* Parents.

But, in general, it may be obſerved, That this Place was more populous before the War than it is at preſent; tho' in this Century it cannot but very much recover and increaſe.

The following LIST we have receiv'd from *Berlin*, conſiſting of all that were Born, Married and Buried in *Pruſſia*, in Four Years.

	Year.	*Born.*	*Pair married.*	*Buried.*
In the Kingdom of *Pruſſia.*—	1715—	19606—	4571—	12000
	16—	20669—	4530—	12155
	17—	21443—	4743—	12301
	18—	20994—	4287—	11047
	Sum—	82712—	18331—	47503

The next LIST contains the Numbers of all that were Born, Married and Buried, in the reſt of the King of *Pruſſia*'s Dominions in *Germany*, &c. for Four Years.

	Year.	*Born.*	*Pair married.*	*Buried.*
In the Electorate of *Brandenburg*.	1715—	14820—	4368—	11663
	16—	15758—	4995—	13149
	17—	16636—	4539—	10805
	18—	17319—	4657—	12891
	Sum—	64533—	18559—	48508

	Year.	*Born.*	*Pair married.*	*Buried.*
In the *New-marck.*	1715	5487	1683	4757
	16	4973	2010	4502
	17	7052	1850	4673
	18	6684	1882	5069
	Sum	24196	7125	19001
In the Dutchy of *Magdeburg.*	1715	7185	2064	4982
	16	7225	2186	5002
	17	8746	2306	6291
	18	8325	1934	5744
	Sum	31481	6490	22619
The Dutchy of *Cleve.*	1715	6772	1964	5066
	16	7313	2001	6221
	17	7582	2051	6055
	18	7542	1891	5865
	Sum	29209	7907	23207
The Dutchy of *Pomerania.*	1715	7243	2190	5373
	16	7386	2407	5557
	17	8196	2108	5020
	18	7896	2138	6231
	Sum	30721	8843	22182

The

	Year.	*Born.*	*Pair married.*	*Buried.*
The Principality of *Halberſtad.*	1715	2371	710	1834
	16	2567	759	2542
	17	2759	764	1961
	18	2527	715	2022
	Sum	10224	2948	8359
The County of *Hobenſtein.*	1715	528	164	406
	16	639	155	444
	17	644	148	449
	18	612	158	416
	Sum	2383	625	1715
The Principality of *Minden.*	1715	1711	579	1618
	16	1788	564	1829
	17	1991	542	1419
	18	1832	552	1486
	Sum	7322	2237	6352
The City of *Meurs.*	1715	406	151	386
	16	439	161	331
	17	454	166	281
	18	470	170	368
	Sum	1769	648	1366

	Year.	*Born.*	*Pair married.*	*Buried.*
In *Geldern.*	1715			
	16			
	17	1990	497	1066
	18	2053	489	1064
	Sum	4043	986	2130
The County of *Ravensberg.*	1715	2188	679	1987
	16	2410	716	2349
	17	2312	672	1817
	18	2344	641	1951
	Sum	9254	2708	8104
The County of *Tecklenberg.*	1715	342	153	513
	16	553	163	543
	17	505	186	370
	18	507	135	387
	Sum	1907	637	1813
The County of *Lingen.*	1715	628	213	484
	16	699	154	638
	17	697	248	449
	18	706	221	840
	Sum	2730	836	2411

	Years.	*Born.*	*Pair married.*	*Buried.*
In *Lauenburg* and *Butow.*	1715	585	144	362
	16	587	169	420
	17	586	118	366
	18	554	153	321
	Sum	2312	584	1469
In the *French* Colonies.	1715	—	—	
	16	—	—	
	17	607	181	564
	18	603	136	639
	Sum	1210	317	1203

The Sum Total of all that were Born in 4 Years - - - - - - -	306006	
- - - - of all that were married	81881	*Pair.*
- - - of all that were Buried	217942	
More Born than Buried, in Number,	88064	

Here is alſo to be obſerv'd, that in the Year 1718. there died 84 Perſons above 91 Years old, and 32 above 100; and, 1 died in the 116th Year of his Age. Beſides in that very Year are reckoned 2088 Baſtards.

Here follows a particular Liſt of ſeveral Borough Towns in the Kingdom of Pruſſia, *receiv'd from the* Chancery *of* Koningſberg, *of all that died in the Year* 1718.

In the Borough of	*Died.*	*In the Borough of*	*Died.*
Brandenburg	581	— Raſtenburg	275
— Schacken	346	— Preuſs Holland	264
— Fiſhauſen	312	— Marungen *and* Liebſtadt - - -	206
— Taxium	456	— Marienwerder *and* Rieſenburg - - -	393
— Balga	392	— Preuſs Marck	312
— Preuſs Eylau	319	— Tilſit	529
— Bartenſtein	167	— Ragnit	381
— Birthen	185	— Inſterburg — Georgenburg — Salau	808
— Oſterode *and* Hohenſtein - - - -	277	— Mummel	556
— Scheſten	159	— Schonberg	126
— Neudenburg *and* Saldau - - - -	330	— Gerdauen *and* Nordenburg - - - -	269
— Lyck	141	— Gilgenburg	109
— Oletzko	188	— Teutſch Eylau	41
— Angerburg	182	— Neuhaff	15
— Rhein	107	*In the three Cities of* Koningſberg - - -	1756
— Orſelburg	220		
— Johanniſburg	141		
— Lotzen	127		
— Neuhauſen	148		
— Labiau	240		
		Sum - - - - -	11047

In all theſe forementioned Places were Chriſtened 20994
Pair Married 4287

	Died.	*Chriſtened.*	*Married.*
In *Dantzick* in the Year 1718.	1579	1470	446 Pair.
In *Epperies*	123	173	

In the Royal Hoſpital at *Liſbon* out of 1251 Foundlings maintained by the King, without knowing their Parents, there died 469, remained 782.

N. B. *The Remainder of theſe Liſts will be communicated at another Opportunity.*

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1724.

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PHILOSOPHICAL TRANSACTIONS.

Giving Some

ACCOUNT

OF THE

Preſent Undertakings, Studies, and *Labours*

OF THE

INGENIOUS,

IN MANY

Conſiderable Parts of the WORLD.

VOL. XXXIII For the Years 1722, 1723.

LONDON:
Printed for W. and J. INNYS, *Printers* to the *Royal Society*, at the Weſt End of St. *Paul's*. 1724.

TO

Sir *HANS SLOANE*, Bar^{t}.

PRESIDENT of the

College of *PHYSICIANS*,

And VICE-PRESIDENT of the

ROYAL SOCIETY,

This Thirty Second Volume of

Philoſophical Tranſactions

is Humbly Dedicated

BY

His Moſt Obliged, and

Moſt Humble Servant,

JAMES JURIN, *R. S. Secr.*

Numb. 381.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *January*, and *February*, 1724.

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I. *Expe-*

I. *Experimenta circa gradum caloris liquorum nonnullorum ebullientium inſtituta.* *A* Daniele Gabr. Fahrenheit, *R. S. S.*

CUM elapſis abhinc circiter decem annis in Hiſtoriâ Scientiarum Societatis Regiæ Pariſienſis legiſſem, quod celeberrimus *Amontonius*, ope alicujus thermometri ab eo inventi, detexiſſet, aquam fixo caloris gradu ebullire; ſtatim magno accendebar deſiderio, thermometrum ejuſmodi mihimet ipſi præparare, ut pulchrum hocce naturæ phænomenon mihi oculis perluſtrare liceret, & de veritate experimenti convictus eſſem.

Quapropter thermometri ſtructuram quidem tentabam, ſed ob habitudinis ſufficientis in elaboratione illius defectum, vana erant conamina, licet ſæpius iterata; & quoniam etiam alia negotia prohibebant thermometri elaborationi magis inſiſtere, opportuniori repetitionem illius dedicabam tempori. Cum defectu virium atque temporis ardor non langueſcebat, æque avidus enim experimenti exitum videndi manebam. In mentem autem mihi veniebant ea, quæ ſolertiſſimus ille rerum naturalium ſcrutator de rectificatione barometrorum ſcripſerat; obſervaverat enim altitudinem columnæ mercurialis in barometro a vario temperamento mercurii aliquantulum (ſatis ſenſibiliter tamen) turbari. Ex his rebar, quod thermometron fortaſſe e mercurio conſtrui poſſet, cujus ſtructura non adeo difficilis foret, & cujus tamen ope experimentum maxime a me deſideratum explorare liceret.

Præparato ejuſmodi thermometro (licet in multis adhuc imperfecto) voto tamen meo eventus reſpondebat; magnâ enim animi voluptate rei veritatem contemplabar.

Tres jam erant anni elapſi, in quibus opticis aliiſque incubuiſſem laboribus, cum cupidus fierem experimentis explorare, an etiam alii liquores fixo ebullituri eſſent gradu caloris. Exitus experimentorum ſequenti continetur tabula, cujus prima columna exhibet liquores adhibitos; ſecunda illorum gravitatem ſpecificam; tertia gradum caloris, ad quem unuſquiſque liquor ebulliendo pertigit.

Liquores.	Gravitas ſpecifica Liquorum ad 48 Gr. calidorum.	Gradus ebullitione acquiſiti.
Spiritus vel Alcohol vini.	8260	176
Aqua Pluvia.	10000	212
Spiritus Nitri.	12935	242
Lixivium cineris clavellati	15634	240
Ol. Vitrioli	18775	546

Gravitatem ſpecificam cujuſcunque liquoris addendam neceſſe judicavi, ut ſi aliorum experimenta jam inſtituta, vel adhuc inſtituenda, a memoratis differrent, colligi poſſit, an e variatione gravitatis ſpecificæ, vel ex aliis differentia petenda ſit cauſis. Experimenta præterea non eodem tempore ſunt facta, & inde etiam liquores vario temperamenti vel caloris gradu erant affecti, ſed quoniam illorum gravitas diverſimode & inæqualiter turbatur, calculo illorum gravitatem ad 48 gradum (qui in thermometris meis medium tenet locum inter terminum intenſiſſimi frigoris arte commixtione aquæ, glaciei, ſaliſque Armoniaci, vel etiam maritimi, confecti, &

inter terminum caloris, qui in ſanguine hominis ſani reperitur) revocavi.

Olea Volatilia aliquo gradu quidem incipiunt ebullire, ſed eorum calor ebulliendo ſemper augetur. Cujus rei cauſa fortaſſe erit, quod nempe volatiliores particulæ avolent, dum reſinoſæ majori attractione præditæ reſtant.

Olea fixa autem tanto calore afficiuntur, ut Mercurius in thermometro ſimul cum illis ebullire incipiat, & inde eorum calor memorato modo vix certe explorari poterit. Sed alium excogitavi modum, cujus in alia ſchedula coram Illuſtri Societate Regia meminiſſe me honorem habiturum eſſe ſpero.

Excepto ſpiritu vini & aqua, fortaſſe etiam gradus cœterorum liquorum hic commemoratorum variabit, precipue ſi magna ſatis quantitate adhibeantur & longius ebulliant.

II. *Obſervationes accuratæ captæ, anno* xvij = xxiij Lugduni Batavorum, Delphis Bataviæ, *& in pago* Rhenoburgo. *Circa mediam* BAROMETRI *altitudinem, mediam* THERMOMETRI *elevationem, tum &* HYGROMETRI *varietatem mediam, circa copiam* PLUVIÆ, *Roris, Nivis, Grandiniſque, ſimul & circa copiam* AQUÆ *quæ exhalavit, & altitudinem putealis* AQUÆ *in* PUTEO, *unde nihil Aquæ exhauſtum fuit toto anni decurſu, de variatione* HOROLOGII PORTATILIS *ſingulis menſibus, ut certa hæc experimenta ſervire queant contemplatoribus* HISTORIÆ NATURALIS, *offert eadem harum deliciarum ſtudioſis* NICOLAUS CRUQUIUS; *Geometra.* R.S.S.

BAROMETRUM.	THERMOM.	HYGROMETRUM.	
Hic pono *Pondus* Athmoſphæræ incumbentis, in planum pedis quadrati *Rhenolandici*, cujus athmoſ. pondus 1947 libras Amſtelædamenſes pendet, quando ☿^{ii} in barometro altitudo eſt 27 *poll.* 7 *lin.* pendet 2094 *lb.* Amſt. quando altitudo fuit ☿ in barometro 29 *poll.* 8 *lin.* hæc enim fuit maxima & minima ☿^{ii} in barometro obſervata altitudo intra plures jam annos.	Hic calorem Atmoſphæræ in loco obſervationis deſtinavi ex rareſcente Aere ita ut ſummum frigus obſervatum ſit ubi notabatur gradus 1000, aqua pura gelaſceret ad grad. 1070, ebulliret vero ad grad. 1510.	Menſuravi hic aquæ copiam in Athmoſphærâ in loco obſervationis; juxta accreſcens decreſcenſve pondus ſpongiæ ad bilancem appenſæ, quam ſpongiam prius Muria Salis Ammoniaci humectaveram.	Quantitas aquæ, quæ totius hujus anni decurſu menſe quolibet exhalaverat, in Aere aperto, & ventis perflato. *Rhenoburgi.*

	lb.	*grad.*	*pond.*	*lin.*
Januar.	2051	1076	81	7
Februar.	46	85	80	14
Martio	35	102	80	33
Aprili	46	109	60	36
Maio	57	126	57	58
Junio	53	140	57	57
Julio	2044	1129	58	37
Augusto	46	141	60	39
Septemb.	54	132	61	24
Octobri	55	121	71	15
Novemb.	53	104	77	15
Decemb.	35	96	79	12
Summa	575 22 —	1361	821	347
Medium	2048	1113	68	toto anno. 28 *pol.* 11 *lin.*

Aquæ Cœlo delapſæ, nimirum Pluviæ, Roris, Nivis, Grandiniſque Altitudo;

Cum cauta cura, ne exhalatione, aut alio modo vel minimum perierit.

Aquæ altitudo putealis, in puteo, menſurata a ſummo ejus margine, ad aquæ ſuperficiem, in fine cujuſque menſis. Fuit vero putei profunditas ima uſque ad ſabulum ſcaturiens vel currens, nihilque aquæ inde eductum toto obſervationis tempore.

Obſervatio accelerati, vel retardati Curſus, in Horologio portatili accuratiſſimo.

Ut pateat quot minuta, in quoque Menſe, plura vel pauciora, abſolverit. Hic (+) augmenti, (÷) decrementi nota eſt.

Ratio relata ad Solis decurſum.

Delphis. *lin. dec.*	*Rhenoburgi.* *lin. dec.*	*ped. pol.*	*Minuta.*	
17.9	21.2	5 : 4	+ 151	Jan.
25.1	23.8	4 : 11	+ 21	Febr.
18.8	28.-	5 : 7	÷ 168	Mar.
5.5	7.5	6 : 10	÷ 120	Apr.
4.2	2.7	8 : 1	÷ 123	Maio.
3.2	4.8	9 : 3	÷ 130	Jun.
38.6	28.-	9 : 7	÷ 90	Jul.
41.9	40.2	9 : 7	+ 133	Aug.
15.1	14.8	9 : 9	+ 24	Sept.
8.2	11.3	9 : 8	+ 19	Oct.
30.7	29.7	9 : 3	+ 266	Nov.
30.6	40.-	8 : 2	+ 252	Dec.
239.8	252	96 : —	+ 866, & ÷ 631 ſive + 235	
poll. 20 (in toto anno)	21 poll.	Med. 8 ped.	in quoq; menſ. 20′ accelerat.	

Obſervationes per aliquot annos factæ.

Ann.	Jan.	Feb.	Martio	Ap.	Mai.	Jun.	Jul.	Aug.	Sept.	Oct.	Nov.	Dec.	Sum.	Med.	Ann.
	lb.			BAROMETRUM									12	lb.	
1720	2034	34	38	36	38	42	45	36	50	40	38	23	454	2038	1720
1721	52	42	36	29	29	43	51	46	43	42	32	29	474	39	1721
1722	76	41	32	41	42	45	36	39	50	59	37	30	528	44	1722
1723	51	46	35	46	57	53	44	46	54	55	53	35	575	48	1723
4	213	163	141	152	166	183	176	167	197	196	160	117		169	
	2053	41	35	38	41	46	44	42	49	49	40	29		2042	
	grad.			THERMOMETRUM										Grad.	
1720	1087	89	86	102	126	126	145	138	121	107	91	91	1309	109	1720
1721	90	74	75	112	116	137	136	142	133	110	100	84	1309	109	1721
1722	79	90	97	109	122	134	139	140	135	117	101	90	1353	113	1722
1723	76	85	102	109	126	140	129	141	132	121	104	96	1361	113	1723
4	332	338	360	432	490	537	549	561	521	455	396	361		444	
	83	85	90	108	122	134	137	140	130	114	99	90		111	
	pond.			HYGROMETRUM										pond.	
1721	89	82	73	80	69	64	63	68	76	76	89	88	917	76	1721
1722	88	85	76	63	62	62	63	68	70	76	81	88	882	74	1722
1723	81	80	80	60	57	57	58	60	61	71	77	79	821	69	1723
3	258	247	229	203	188	183	184	196	207	223	247	255		219	
	86	82	76	68	63	61	61	65	69	74	82	85		73	
	lineæ			Pluvia, &c. Delphis.										poll. lin.	
1715	11	20	36	8	15	18	95	62	36	37	47	15	400	33: 4	1715
1716	19	20	14	7	17	4	48	19	55	57	21	32	313	26: 1	1716
1717	31	15	29	29	31	28	29	28	24	32	29	28	333	27: 9	1717
1718	31	18	6	30	17	17	35	27	14	46	21	25	277	23: 1	1718
1719	33	20	10	11	17	4	12	34	23	25	24	22	235	19: 7	1719
1720	36	34	21	21	25	20	23	55	47	56	25	20	363	30: 3	1720
1721	20	31	27	59	30	34	15	41	27	57	30	48	419	34:11	1721
1722	2	20	25	23	15	22	49	53	25	7	21	53	315	26: 3	1722
1723	18	25	19	5	4	3	39	42	15	8	31	31	240	20:	1723
9	191	193	187	193	161	150	345	361	266	325	249	274		241: 3	
	21	21	21	21	18	17	38	40	30	36	28	30		26:10	

lin.				*Pluvia &c. Rhenoburgi*									*pol. lin.*		
1720	35	16	13	24	29	14	20	48	54	50	39	25	367	30:7	1720
1721	21	29	28	59	35	28	14	47	27	51	32	46	417	34:9	1721
1722	3	20	27	29	28	27	60	64	26	10	21	61	376	31:4	1722
1723	21	24	28	7	3	5	28	40	15	11	30	40	252	21:	1723
4	80	89	96	119	95	74	122	199	122	122	122	172		117:8	
	20	22	24	30	24	19	30	50	30	30	30	43		29:5	
lin.				*Aqua quæ exhalavit Rhenoburgi.*									*pol. lin.*		
1720	10	10	10	25	44	55	59	56	50	30	10	12	371	30:11	1720
1721	8	9	15	54	47	60	48	42	27	20	22	10	362	30:2	1721
1722	5	7	20	28	54	54	65	59	34	14	13	13	366	30:6	1722
1723	7	14	33	36	58	57	37	39	24	15	15	12	347	28:11	1723
4	30	40	78	143	203	226	209	196	135	79	60	47		120:6	
	8	10	20	36	51	56	52	49	34	20	15	12		30:2	
ped. pol.				*Putealis Aquæ Altitudo.*									*ped. pol.*		
1720	6:	6:3	6:5	6:7	6:6	8:	8:11	9:	8:3	6:11	5:8	5:5	84:	7:	1720
1721	5:6	5:6	4:6	4:2	5:7	7:	8:1	7:9	7:7	6:3	4:6	4:4	70:9	5:11	1721
1722	6:	5:7	5:11	6:1	6:10	7:11	8:	7:10	7:4	7:9	7:3	4:6	81:	6:9	1722
1723	5:4	4:11	5:7	6:10	8:1	9:3	9:7	9:7	9:9	9:8	9:3	8:2	96:	8:	1723
4	22:10	22:3	22:5	23:8	27:	32:2	34:7	34:2	32:11	30:7	26:8	22:5		27:8	
	5:8	5:7	5:7	5:11	6:9	8:	8:8	8:6	8:3	7:8	6:8	5:7		6:11	
Minuta				*Horologii portatilis Curſus.*											
1722	— 34	÷ 9	÷ 7	+ 5	÷ 38	÷ 55	÷ 89	÷ 78	÷ 84	÷ 8	+ 151	+ 246	+ & ÷	402	1722
1723	+ 151	+ 1	÷ 187	÷ 139	÷ 142	÷ 149	÷ 109	+ 113	+ 4	÷ 1	+ 246	+ 232	+ & ÷	727	1723
2	+ 97	÷ 8	÷ 194	÷ 134	÷ 180	÷ 204	÷ 198	+ 35	÷ 80	÷ 9	+ 397	+ 478	+ & ÷	1129	
	+ 48	— 4	÷ 97	÷ 67	÷ 90	÷ 102	÷ 99	+ 17	÷ 40	÷ 4	+ 199	+ 239	+ & ÷	564	
1	*Jan.*	*Feb.*	*Mar.*	*Apr.*	*Maii*	*Jun.*	*Jul.*	*Aug.*	*Sept.*	*Oct.*	*Nov.*	*Dec.*			

III. *An Account of a Dropſy in the left Ovary of a Woman, aged 58. Cured by a large Inciſion made in the Side of the* Abdomen, *by Dr.* Robert Houſtoun.

AUGUST 1701. I was in the Country, with a Patient, the Lady *Anne Houſtoun*, Wife to Sir *John Houſtoun*, Baronet; in the Shire of *Renfrew*, ten Miles from *Glaſgow*, *North Britain*. This charitable Lady preſs'd me with great Earneſtneſs to viſit a Tenant's Wife, who lay bedridden, of an uncommon Diſeaſe, which no Phyſician, or Surgeon, who had ſeen her, could give any Name to, or account for. She inform'd me, the ableſt of that Country had forſaken her, and declared her incurable, ſo that I could loſe no Reputation by the Reſult of my Endeavours.

In order to oblige this worthy Lady, and in Compaſſion to the Diſtreſs of a poor Woman in ſo deplorable Condition, deſerted and given over on all ſides, I went, determined to do every thing in my Power for her Relief. She was in the 58th Year of her Age, her Name was *Margaret Millar*.

She inform'd me that her Midwife, in her laſt lying-in, at 45 Years old, having violently pull'd away the Burthen, ſhe was ſo very ſenſibly affected by a Pain, which then ſeiz'd her in the left Side, between the *Umbilicus* and Groin, that ſhe ſcarce ever had been free from it after, but that it had troubled her more, or leſs, during 13 Years together; that for two Years paſt ſhe had been extremely uneaſy, her Belly grew very large, and a Difficulty of breathing increaſed continually upon her; inſomuch that for the laſt ſix Months, ſhe had ſcarce breath'd at all but with the utmoſt Difficulty. That in all that Space of Time, having quite loſt her Appetite, ſhe had ſcarce eat ſo much as would nouriſh a ſucking Child; and that for three Months together ſhe had now been forc'd to lie conſtantly on her Back, not daring to move at all, to one ſide or other.

This Tumour was grown to ſo monſtrous a Bulk, that it engroſs'd the whole left Side, from the *Umbilicus* to the *Pubes*, and ſtretch'd the Abdominal Muſcles, to ſo unequal a Degree, that I don't remember ever to have ſeen the like in the whole Courſe of my Practice. It drew towards a Point. Her being ſo long confined to lie continually on her Back, having grievouſly excoriated her, added much to her Sufferings, which, with want of Reſt and Appetite, had waſted her to Skin and Bone, as the poor Woman herſelf expreſſed it. Indeed ſhe needed not to have told me ſo, my Eyes were too faithful Witneſſes of her low and wretched Condition.

Scarce able to ſpeak out, ſhe told me, that having heard much of my Succeſs, ſhe had ſtrong Hopes of Relief, provided I would try at leaſt, and do ſomething in Pity to her Affliction.

I anſwer'd her that I was willing, but afraid, in her low State, ſhe would want Strength to undergo a large Inciſion; that in order effectually to relieve her, I muſt be oblig'd to lay open a great Part of her Belly, and remove the Cauſe of all that Swelling: ſhe ſeem'd not frightened, but heard me without Diſorder, and, as if inſpir'd with ſudden Courage, preſs'd, and urg'd me to the Operation.

I drew (I muſt confeſs) almoſt all my Confidence from her unexpected Reſolution, ſo that without loſs of Time, I prepared what the Place would allow, and with an Impoſthume Lancet, laid open about an Inch, but finding nothing iſſue, I enlarged it two Inches, and even then nothing came forth but a little thin yellowiſh *Serum*, ſo I ventured to lay it open about two Inches more: I was not a little ſtartled, after ſo large an Aperture, to find only a glutinous Subſtance bung up this Orifice. All my Difficulty was to remove it; I try'd my Probe, I endeavour'd with my Fingers, but all was in vain; it was ſo ſlippery that it eluded every Touch, and the ſtrongeſt hold I could take.

I wanted, in this place, almoſt every thing neceſſary, but bethought myſelf of a very odd Inſtrument, yet as good as the beſt in its Conſequence, becauſe it anſwer'd the End propos'd. I took a ſtrong Firr-Splinter, ſuch as the Poor in that Country ordinarily uſe to burn inſtead of Candles; I wrapt about the End of this Splinter ſome looſe Lint, and thruſt it into the Wound, and by turning and winding it, I drew out above two Yards in Length of a Subſtance thicker than any Gellie, or rather like Glue that's freſh made and hung out to dry; the Breadth of it was above ten Inches; this was followed by nine full Quarts of ſuch Matter, as I have met with in Steatomatous and Atheromatous Tumours, with ſeveral *Hydatides*, of various Sizes, containing a yellowiſh *Serum*, the leaſt of 'em bigger than an Orange, with ſeveral large Pieces of Membranes, which ſeem'd to be Parts of the diſtended Ovary. Then I ſqueez'd out all I could, and ſtitch'd up the Wound in three Places, almoſt equi-diſtant: I was oblig'd to make uſe of *Lucatellus*'s Balſam, which was made by her Lady for the Uſe of the Poor; with this Balſam I covered a Pledget, the whole Length of the Wound, and over that laid ſeveral Compreſſes, dipp'd in warm *French* Brandy, and becauſe I judg'd that the Parts might have loſt their Spring, by ſo vaſt and ſo long a

Diſtention, I dipt in the ſame Brandy a large Napkin four times folded, and applied it over all the Dreſſings, and with a couple of ſtrong Towels, which were alſo dipt, I ſwathed her round the Body, and then gave her about four Ounces of the following Mixture, which I had from her Lady.

℞ *Aq. Menthæ* ℔ ſs. *Aq. Cinnamomi fort.* ℔ iſs.
Syr. Diacodii ℥ vi. *M.*

The Cinnamon-Water was drawn off from Canary and the beſt Cinnamon; indeed it was the fineſt and moſt fragrant Cinnamon-Water I ever taſted; of this Mixture I ordered her 2 or 3 Spoonfuls 4 times a Day.

Next Morning I found her in a breathing Sweat, and ſhe informed me, with great Tokens of Joy, that ſhe had not ſlept ſo much, nor found herſelf ſo well refreſh'd, at any Time for three Months paſt. I carefully attended her once every Day, and as conſtantly dreſſed her Wound in the ſame Manner as above, for about eight Days together; I kept in the lower Part of the Wound a ſmall Tent, which diſcharged ſome Seroſities at every Dreſſing for 4 or 5 Days. But Buſineſs calling me elſewhere, I left her, having firſt inſtructed her two Daughters (both Women, who carefully attended her) how to dreſs her Wound, and told 'em what Diet I thought moſt proper, enjoining 'em ſtrictly to obſerve what I order'd.

Her chief Food was ſtrong Broth made of an old Cock, in each Porringer of which was one Spoonful of the Lady's Cinnamon-Water; this was repeated 4 times a day, and gave her new Life and Spirits.

After three Weeks Abſence, I called at her Houſe, and finding it ſhut up, was a little ſurpriz'd, but had not gone far before I was much more ſurprized, when I found her ſitting wrapt up in Blankets, giving Directions to ſome Labourers who were cutting down her Corn.

She mended apace to the Admiration of every body thereabouts, recovered ſurprizingly, and lived in perfect Health from that time, which was in *Auguſt* 1701, till *October* 1714. when ſhe died in ten Days Sickneſs.

That this Tumour, or rather Dropſy of the *Ovarium*, proceeded from the Midwife's Raſhneſs in pulling away the *Placenta*, not knowing how to ſeparate it from the *Uterus* ſkilfully, ſeems to me plain from what the Woman herſelf told me, and what fell out afterwards.

The *Placenta* adhering faſt to the *Uterus*, required more Art to bring it away than ſhe was Miſtreſs of, which probably induc'd her to uſe Violence; by which ſhe forc'd down the *Fundus Uteri*; ſo overſtrain'd the Ligaments, and all that's appended to 'em; eſpecially the *Ligamentum latum* of the left Side and its *Ovarium*, which may be reaſonably allow'd to have been hurt in the Relaxation with the reſt. Hence the Elaſticity of theſe læſed Parts was not only impair'd, but the ſmall Lymphaticks ruptur'd, ſo that the extravaſated *Lympha* ruſhing out, thicken'd, and not being able to recirculate, dilated the injur'd *Ovarium*, and thus increas'd the Tumour, and the Parts being already exceſſively diſtended, and being no longer able to reſiſt the new Influx of freſh Secretions, ruptur'd alſo, and by Degrees augmented to that huge and enormous Bulk.

Cyprianus in his Letter to Sir *Thomas Millington*, gives ſeveral Inſtances of the Miſchiefs committed by Midwives, and other ignorant Perſons. When a *Placenta* adheres, then (ſays he) they tear all before 'em, by pulling rudely, they force down the *Fundus Uteri*, conſequently do Violence to the *Ovaria* and other Parts, whence follow Inflammations, *&c.*

Foreſtus, *l.* 28. *Obſ.* 80. ſays, that an over-bold Midwife pulled out the *Placenta* too haſtily, and that the Woman preſently fell into a Swoon, and died immediately.

Frederick Ruyſch attributes the Cauſe of a *Placenta*'s adhering cloſe, to a central Inſertion of the Navel-String into the *Placenta*, in which Caſe it is hard to ſeparate; a Train of diſmal Accidents attend thoſe who, not knowing how to do it, venture to pull it away by Force. *Vide Obſ. Anat.* 97.

Dionis and *la Motte* have made the ſame Remark, by the latter of which we are told of innumerable Accidents, which he has known happen to Women in hard Labour, by the Raſhneſs and Ignorance of bold Pretenders, who, without Rule, Precepts, Practice, or any ſuitable Inſtructions, venture to practiſe Deliveries. The aforeſaid *Ruyſch* has a remarkable Inſtance of the Effects of Violence in hard Labour: The Caſe is curious. *Vide Obſ. Anat.* 63.

Manget, in his *Theatrum Anat.* tells us that in all hard Labours, where the *Placenta* ſticks, the Ligaments ſuffer more or leſs by a raſh and ignorant Way of Pulling; Examples of which ſee in *Platerus*'s *Obſ. lib.* 3. *Bartholin. Cent.* 2. *Hiſt.* 91. *Cent.* 5. *Hiſt.* 19. *Marchett. Anat. cap.* 7. and others, of which *Blaſius* has given us an ample Liſt in his Commentaries on *Veſlingius*.

He likewiſe tells us, that the *Ovaria* grow to a wonderful Bulk, and contain ſuch a huge Quantity of Liquor, that they become dropſical, whereof *Skenckius* in his *Obſ.* has recorded many Examples, as hath *Riolan*, and many others.

Munnicks in his *Bibliothec. Anat.* gives us the Hiſtory of a large Dropſy of the right *Ovarium*.

Mortgagni has told us, that he has often met with large Veſicles in the *Ovaria*, and, in morbid Bodies, ſometimes full of purulent Matter.

Sylvius. Ex lymphaticis læſis tam Virilium, quam Muliebrium Teſticulorum, &c. Hydrops particularis excitari poteſt.

Veſlingius cap: 7. has often met with large Tumours, from Obſtructions in the *Ovaria* of Women.

Veſalius found in the right *Ovarium*, 9 or 10 *Glandules*, like Gooſe Eggs, with Matter not unlike the Whites of Eggs, or rather thicker.

Gul. Ballon, paradig. vi. found an Impoſthume of the Nature of a *Steatoma*, near the *Collum Uteri*; The Woman had a large Tumour on her Side 16 Years, and had only one *Ovarium*, which was ſhown as a wonderful thing.

At *Weſtminſter*, about two Years ago, I opened the Body of a Gentlewoman, about 60, whoſe left *Ovarium*, of a great Bulk, weigh'd vi. ℔. to which adher'd ſeveral *Hydatides* like Pullets Eggs.

Hildan mentions an *Hydropick* Tumour of the right *Ovarium*, of a prodigious bulk, ſtuff'd with Hair, *vide Gul. Fab. Hild. Cent.* 5 *Obſ.* 48. *Greg. Horſt. Tom.* 2. *L.* 4. *Ob.* 53.

Ruyſch Obſ. Anat. 17. A Dropſie (ſays he) of the *Ovarium*, or (if you pleaſe) of the Eggs, is an Affection, or Diſeaſe, well enough known to others, but I am afraid not ſo well conſider'd; Authors have agreed to call it *Hydatis*, with which name I ſhall reſt ſatisfied, provided we agree about the ſubject of the Diſtemper. I obſerve this, moſt frequently, if not always, to be the Eggs, but ſo chang'd, and ſometimes ſwell'd to ſuch a Bulk, that I have often ſeen them larger than a Child's Head.

Drelincourt has given us a very full and exact Account of a Dropſie of the left *Ovarium*, in a Lady of 35 Years. The Tumour, of an enormous Bulk, continu'd three Years. The Body of the *Ovarium*, with all contain'd in it, weigh'd, 60 ℔. It was nothing but a number of little Globules, cluſter'd together, that differ'd in their Roundneſs, Form, Colour, and Conſiſtence, proceeding from little ſeminary Veſicles in a Cluſter. Some had Water extremely clear and liquid, others a yellowiſh *Serum* thin, others a glutinous Matter; ſome were as big as Pullets Eggs, others bigger than one's Fiſt.

Let theſe few, out of the many Inſtances which I could produce from Authors of undoubted Reputation, ſuffice to prove, that the *Ovaria*, as well as the *Tubæ Falloppianæ*, Ligaments, and *Uterus* itſelf, are not free from Dropſies, *&c.* and that they proceed from Obſtructions, often occaſioned by rude and violent dealing with Women in difficult Labours, which generally bring on a train of diſmal Symptoms, that ſooner or later, according to the Strength of the Sufferer, after a miſerable, painful, languiſhing Life, end in Death.

The manifeſt Succeſs in this uncommon Caſe may be of uſe, and may ſhew, that we ought not to diſpair too ſoon, in Diſtempers that are ſeemingly moſt dangerous.

IV. *Præparatio Cærulei Pruſſiaci ex Germaniâ miſſa ad* Johannem Woodward, *M. D. Prof. Med. Greſh. R. S. S.*

℞ TArtari crudi & Nitri crudi Siccati ad ℥iiii. Pulveriſentur minutiſſime, & commiſceantur, deinde admoto igneo carbone detonentur, & habebis Salis Tartari extemporanei ℥iiii. Dum adhuc calidum eſt hoc Sal, pulveriſetur ſubtiliſſime, & addantur ſanguinis Bovini probe exſiccati & ſubtiliſſ. pulv. ℥iiii. Hæc,

bene mixta, indantur crucibulo, ut tertia pars vacua ſit; impoſito dein operculo igni committatur, & circumdetur crucibulum carbonibus, ut ſenſim ardeſcat, & materia ſine præpropera accenſione flammam concipiat & igneſcat. In hoc ignis gradu teneatur materia, donec flamma & accenſio remittat; augeatur demum ignis, ut valde candeat materia, & parum flammæ e crucibulo amplius emineat. Remove demum ab igne crucibulum, & materiam mortario ingeſtam leviter contere, & ad manus habeto aquæ ferventiſſimæ pluvialis libras 4 ponderis civilis, cui materiam, adhuc ferventem, immittas, & per ſemihoræ ſpatium coque; decoctum per linteum coletur, & materia remanens nigra, aquæ portioni denuo affuſa, igni iterum apponatur, coquatur, & percoletur; id quod eouſque continuandum, donec ſalſedo & acrimonia omnis e materia ſit elixiviata, & aqua redeat inſipida. Humores omnes in linteo & materia reſiduos, fortiter exprime, & ubi ſingula in unum colligeris, igni iterum committe, & ad remanentiam 4 librarum evapora, & ulteriori uſui ſerva, ſub. No. 1.

℞. Porro Vitrioli Anglici ad albedinem leviter calcinati ʒi. ſolvatur in Aquæ pluvial: ʒvi. filtretur per chartam & ſignetur: No. 2.

℞. Denique Aluminis crudi ʒviii. Solvatur in libris 4. aquæ ferventiſſimæ ad omnimodam Aluminis conſumptionem, hoc rite peracto, adjunge ſolutionem Vitrioli ſub No. 2. aſſervatam, quæ ex igne fervens ingeratur ollæ ſatis magnæ & amplæ, & cum lixivio No. 1. ſeorſim bene fervefacto, combinetur. Fiet ex continenti magna ebullitio, & apparebit color viridis montani ſeu chryſocollæ; effundatur alternis vicibus, durante ebullitione, ex uno vaſe in aliud, qua ceſſante, quieti committe. Tum linteo inſinuetur, ut aquoſitas tranſeat, color vero in linteo remaneat; ſi igitur nihil humiditatis amplius diſtillet, cum ſpathula lignea

e linteo

è linteo in ollam novam minorem remove; ſuperfunde poſtea ſpiritus ſalis comm. ʒii. vel ℥iii. & ſtatim apparebit color cæruleus pulcherrimus: quæ probe mixta per noctem quieſcant, quo facto aquæ pluvialis magna quantitas addatur, in gyrum moveatur ſpathula, & poſteaquam reſedit materia, aqua decantetur, & recens aqua ſuperfundatur, & eouſque labor reiteretur, donec omnis acrimonia ſit deſumpta, & aqua inſipida defluat; hoc pacto præcipitatum tuum ſummè cæruleum linteo expanſo ingere, ut aqua diſtillet, ſenſimq; color calore leni exſiccetur uſui.

N. B. Calcinatio magni momenti eſt in hoc opere, nam color cyaneus & cæruleus obſcurus ortum ſuum trahit a calcinatione levi, mediocri, & forti ſanguinis arefacti cum ſale Tartari, & inde diverſitas coloris.

Lixivia ferventiſſima uno eodemque feſtinatiſſimo actu ſunt confundenda.

V. *Obſervations and Experiments upon the foregoing Preparation. By Mr.* John Brown, *Chymiſt, F. R. S.*

DR. *Woodward* having lately communicated a Paper (which he receiv'd from another hand) to this Society, containing a Proceſs for making the *Pruſſian* Blue. I was willing to go thro' it exactly, according to the Proportions there preſcrib'd; and obſerv'd that by a Calcination of ʒ 4 of Blood dry'd, with ʒ 4 of *Sal Tartari*, in two Hours time that Part of the Operation was over, and a black ſpongy Subſtance remain'd in the Crucible weighing ʒ 4. a Diſſolution of which being made in boiling Water and afterwards filtred, the Remainder, when dried, weigh'd

9 Drachms, *Avoirdup.* the former having been weigh'd by the ſame kind of Weight.

The Loſs in the Diſſolution and Filtration of the Vitriol and Alum, is not worth taking Notice of, they having both been very clean before they were diſſolv'd. The Mixtures being made as preſcrib'd, with the Addition of the *Spiritus Salis*, the Product was a very fine Blue, which when well edulcorated by frequent Waſhings, and after that thoroughly dried weigh'd ʒ1. or a little more, and entirely anſwer'd the Character the Author gave of it.

Among the ſeveral Experiments that were made with theſe Liquors, I mean the *Lixivium* with Blood, the Solution of Vitriol, the Solution of Alum, and the Spirit of Salt, tho' they always produced a Blue, yet that Blue differ'd in Degrees of Colour, according to the varied Proportions of the Vitriol and Alum, and the Colours produc'd from theſe ſeveral Proportions were each of them improv'd by the Addition of the *Sp. Salis.*

I ſhall mention only two of the ſeveral I tried, in one of which the Alum was entirely left out, and a pale Blue produc'd; in the other, the Proportions of Vitriol and Alum were equal, and a very deep Blue was produc'd.

Theſe Differences in Colour, ariſing from the ſeveral Proportions of the Vitriol and Alum, are only mention'd to confirm the Truth of the Author's Preſcript, as being the moſt exact and beſt proportion'd to produce the fineſt Colour, of any I have try'd. The only Misfortune he takes notice of, as attending his Preſcript, is what may happen in the Calcination.

It would be curious to know what gave the firſt Hint for the Production of ſo fine a Colour, from a Combination of ſuch Materials; eſpecially when we come to conſider, that the Blood has the greateſt and principal

Share in this ſuprizing Change. I doubt not but Blood of any kind, or Fleſh of any kind, would produce the ſame Effects, but have reaſon to believe the latter would not produce ſo beautiful a Colour as the former. I purpoſely dry'd ſome Beef freed from its Skin and Salt, and purſued the ſame Courſe as with the Blood; but there was a ſenſible Difference to be obſerv'd during the Calcination, and a very manifeſt one in the Beauty of the two Colours, when finiſh'd.

To prove the Share the Blood has in this Change, the following Experiments (ſome of which I had the Honour of ſhewing before this Society) may be convincive.

The Solution of Alum mix'd with that of the Vitriol produceth no Alteration of Colour: if to theſe you add the *Spiritus Salis*, the Appearance is the ſame; but if to the whole you put the *Lixivium* with Blood, there precipitates a Blue.

If you ſubſtitute, inſtead of the *Lixivium* with Blood, a *Lixivium* made with the ſame Salt of Tartar only, which then becomes an *Ol. Tartari*; and after the Mixture of the Solution of Alum, with that of the Vitriol, you pour on this *Ol. Tartari*, there follows indeed a Precipitation, but of no Colour; and if you add the Spirit of Salt, it ſo ſtrongly attracts what is precipitated, as to render the muddy Mixture perfectly clear.

The very ſame Effect will follow, if any Volatile Alcalious Spirit is made uſe of as a precipitant, or any Volatile Salts diſſolv'd in Water; nor can the Blood it ſelf be ſuppoſed to communicate this Change from any ſuch Properties, the Heat of Fire it undergoes in the Calcination, being ſufficient to throw them off.

In the Calcination of the dry'd Blood and Salt of Tartar it was obſerv'd, that there was a Loſs of juſt half. It is difficult to determine exactly what Quantity of either was loſt by this Calcination, but it will eaſily be granted, that there was loſt a far larger Quantity of the Blood, than of the Salt of Tartar; and that is obvious from an Experiment, by which, when the Salt of Tartar was calcined by itſelf, with the ſame Degree of Heat, it loſt leſs than an $\frac{1}{11}$ Part, whereas, when the dry'd Blood was calcined by itſelf, it loſt more than $\frac{6}{7}$.

The Blood, in Calcination with the Salt of Tartar, communicates its tinging Quality to the Salt, or that Quality is extracted from it by the Salt, and paſſes with it in its Diſſolution in the boiling Water.

To prove this, ſome dry'd Blood was calcin'd by it ſelf, and a ſtrong Decoction was made of it in Water, and afterwards filtred: this, when mix'd with the former Solutions, produced little or no Alteration; but on the Addition of the *Spiritus Salis*, changed to an Amber Colour, without any Precipitation.

When this Liquor was mix'd with the *Ol. Tartari*, and poured to the former Solutions, it caus'd a Precipitation, but no Colour, and the *Spiritus Salis*, as in the other Experiment, made the Liquor clear again, but left this alſo of an Amber Colour.

The Change of Colour is not effected in any of the Materials, except in that of the Solution of Vitriol, ſo that the Alum ſeems only to be of uſe in fixing the Colour, as it is often us'd by the *Dyers* for that Purpoſe, and the *Spiritus Salis* gives it a deeper Dye. For if the *Lixivium* with Blood be poured to the Solution of Alum alone, there will fall a Sediment a little on the Purple, to which if you add the *Spiritus Salis*, it changes the Colour, and the Sediment is of a Brown.

So, much the ſame changes will be produced, if you pour the *Spiritus Salis* to the *Lixivium*, but not the leaſt Appearance of a Blue, whereas, as is beforemention'd, when the *Lixivium* is poured to the Solution of Vitriol, there immediately follows the Blue, which is ſtill heighten'd by the Addition of the *Spiritus Salis*.

It will not be improper to take notice, that as the Author orders all the Liquors, except the *Spiritus Salis*, to be boiling hot when mix'd, ſo it is certain the Colour is thereby more immediately produc'd, and looks more beautiful; but moſt of the Experiments here mention'd were made with the Liquors cold, and the Colours came to their Beauty with a little waſhing. In one of the Experiments with the Liquors cold, after the *Lixivium* with Blood had precipitated the Blue in the Mixture of Alum and Vitriol, by pouring in a little more of the *Lixivium*, the Blue all diſappear'd, and an ugly muddy Colour was left; but the Addition of the *Spiritus Salis* ſoon diſcharg'd that, and the Blue return'd.

In calcining the Beef and Salt of Tartar, I found the Matter left in the Crucible to weigh juſt half of the whole Mixture, as in that with the Blood; but after the boiling it in Water, the *Reſiduum* in the Filtre when dry'd, was very near a third leſs in Proportion than the other. From whence may be reaſonably inferr'd, That the Salt of Tartar holds a larger Share of the Beef in the one Operation, than of the Blood in the other.

Having in the former Part of this Account of the *Pruſſian Blue* prov'd, by the Experiments there mentioned, that the Solution of Vitriol was the only Subject among thoſe Ingredients, that the *Lixivium* of Blood produc'd this Change of Colour in it; and having ſince conſider'd that the Vitriol made uſe of in this Preparation, is no more than Iron diſſolv'd by a Liquor running from the *Pyrites*, when expos'd to the Weather, which is afterwards boil'd up and ſhot into Cryſtals; it ſeem'd to follow as a natural Conſequence, that this Metal is the Subject on which the *Lixivium* of Blood produces the change; and this Thought gave occaſion to the following Experiments on metallick Bodies, in order to obſerve if the ſame Change of Colour could be produc'd in any of them.

To a Solution of Silver in *Aquafortis* was pour'd the *Lixivium* of Blood, which occaſion'd a *Coagulum* of a pure Fleſh Colour. The *Lixivium* made with Fleſh produc'd a whitiſh *Coagulum*, and the *Ol. Tartari* (which was continued to be us'd by way of compariſon with the other *Lixivia*) a much whiter. By the Addition of the *Spiritus Salis* to each of theſe, the Bloom of the Fleſh Colour was taken off in the firſt, but ſuffer'd no other Change. In the ſecond the *Coagulum* was a little ting'd with Blue; and in the third the White was manifeſtly improv'd. The bluiſh Tinge in the ſecond of theſe Experiments cannot entirely be aſſigned as the Effect of the *Lixivium* with Fleſh, becauſe Silver, when thus diſſolv'd, whether precipitated with Salt Water, or *Ol. Tartari*, will, after it has ſtood ſome Time, contract a bluiſh Tinge, and this from an Alloy of Copper, from which it is not entirely freed.

The ſame Liquors were made uſe of to precipitate the Mercury in the *Mercurius Sublimatus Corr.* diſſolv'd in Water; the Conſequence of which was, that the *Lixivium* with Blood produced a pure yellow; the *Lixivium* with Fleſh an Orange Colour; and the *Ol. Tartari* a dingy red. The addition of the *Spiritus Salis* to theſe, made ſome very odd Alterations; for the firſt chang'd its yellow Colour for an Orange; the ſecond, its Orange for a Blue; and the third became quite clear again without any Colour. The blue Colour in the Mixture of the *Lixivium* with Fleſh, and Solution of Sublimate, may be accounted for from the

Vitriol in the Compoſition of the Sublimate; but it will not be ſo eaſy to give a Reaſon why the ſame Colour ſhould not have been produced from the *Lixivium* with Blood, and the ſame Solution.

Copper, when diſſolv'd in *Aquafortis*, tinges the Water of a green Colour; and if to this you pour the two *Lixivia* of Blood and Fleſh, the *Coagula* are much alike, *viz.* a white ting'd with green; but when you add the *Spiritus Salis*, they both change and become of a Colour not unlike the Copper itſelf before it is diſſolv'd in the *Aquafortis*. If the *Ol. Tartari* be pour'd to a Solution of the Copper, the *Coagulum* is a pale green, which *Coagnlum* the *Spiritus Salis* diſſolves, and leaves the Liquor clear, but green, as before Precipitation.

Tin-Glaſs (an imperfect Metal) diſſolv'd in *Aquafortis*, and mix'd with the *Lixivium* of Blood made a milky *Coagulum*, and by the Addition of the *Spiritus Salis*, after ſome Time ſtanding, its upper Surface chang'd to a light Blue. The *Lixivium* of Fleſh and the *Ol. Tartari* produc'd both white *Coagula*, which the *Spiritus Salis* ſcarcely alters.

Lead diſſolv'd in Spirit of Vinegar produceth much the ſame white *Coagulum*, when mix'd either with the *Lixivium* of Blood, Fleſh, or the *Ol. Tartari*, nor doth the *Spiritus Salis* make any Alteration.

By all theſe Experiments it is pretty evident, that not any of theſe metallick Bodies were affected by the *Lixivium* of Blood, ſo as to produce this fine Blue. The two Metals untried are Gold and Tin, the latter of which, when diſſolv'd in Spirit of Vinegar, has ſo near a Reſemblance to Lead diſſolv'd in the ſame *Menſtruum*, that in all Probability the Experiments would anſwer much alike in both. What may be expected from Gold, I am not yet ſo well aſſured of, as I am from Iron, which when diſſolv'd in *Spiritus Vitrioli*, will anſwer all the Experiments that have been tried with the Solutions of Vitriol, and produce as fine a Colour; nor can this be owing to any Property in the Diſſolvent itſelf, which, tho' drawn from the ſame kind of Vitriol all along made uſe in theſe Experiments, yet is ſo alter'd by the violent Fire in the Production of it, as not to anſwer in any Trials to the Vitriol itſelf.

May we not therefore hence conclude, that Iron is the Metal, that is the Subject of this beautiful Colour produc'd by Means of the *Lixivium* with Blood?

VI. *The Remainder of the Bills of Mortality,* &c. *of the ſeveral Towns of* Europe. *Extracted from the* Acta Breſlavienſia. *By Dr.* Sprengell, *F. R. S.*

A Liſt of thoſe that were Born and Buried in Breſlaw, *in the Year* 1720, i. e. *from the 25th of* December 1719 *to the 24th of* December 1720.

Buried.	
From the 25th of Decemb. to the 31ſt,	40
In January,	160
February,	107
March	139
April	192
May	158
June	120
July	131
Auguſt	182
September	189
October	130
November	132
December, only till the 24th,	127
	1816

Chriſtened.		
Males		564
Females		556
		1120
Married 460 Pair		
Among the Dead were		
Married Men		385
Married Women		186
Widows & Widowers		285
Bachelors		113
Maidens		113
Children to ten Years of Age	Boys	345
	Girls	300
Stilborn	Boys	53
	Girls	30
		1810

In the Year 1720. *In* Lignitz *were*

Married	92 Pair.
Chriſtened	283
Buried	366

Among which were 149 Children.

In Jauer.

Chriſtened of *Proteſtants* - - - -	100 Ch.
Buried	175

In Oels.

Chriſtened	180
Buried	203

In Schweidnitz.

Married	218 Pair.
Chriſtened	920
Buried	250

In Vienna.

From the 1ſt of *Jan.* to the 31ſt of *December.*

Buried	6825
Chriſtened	4126

In Lobau.

Married	58 Pair.
Born	160
Buried	355

Amongſt which were born 80 Boys and 80 Girls: Buried 95 Men and 48 Women, beſides 4 who died in Childbed: 26 Bachelors and 22 Maidens: 36 Boys and 42 Girls: 25 Chriſoms: 7 Stilborn: 55 Widows & Widowers. Likewiſe 51 Perſons between 60 and 70 Years of Age: 29 between 70 and 80: 7 betwixt 80 and 90: 1 of 90, and 1 of 99.

In Dreſden.

Married	368 Pair.
Born	1448

Among which were 719 Boys, and 641 Girls, legitimate: Illegitimate Boys 44 and Girls 44.

Buried	1733

Amongſt which were 255 married Men and 182 married Women, 189 Widowers and Widows: 88 young Men and 84 young or ſingle Women: 883 Children, *viz.* 461 Boys and 422 Girls, amongſt which were Stillborn 72, *viz.* 37 Boys & 35 Girls.

In John-George *City.*

Married	24 Pair.
Chriſtened	148

i. e. 79 Boys and 69 Girls, among which were 2 Stilborn, and 10 Baſtards.

Buried 243

viz. 37 married Men and 18 married Women: 5 Widowers and 37 Widows, 6 Bachelors, and 18 Maidens: 64 Boys, 58 Girls, *&c.*

In St. Annaberg.

Married 18 Pair.

Born 105

i. e. 63 Boys and 42 Girls, among which were two Pair of Twins, and 4 Baſtards.

Buried 180

viz. 96 Men and 84 Women; among which were 34 married Men and 18 married Women, 1 in Childbed; 12 Widowers and 28 Widows: 12 Bachelors, and 17 Maidens: 38 Boys and 20 Girls, among which was 1 dead born.

In Schneeberg.

Married 12 Pair.

Born 89

As 44 Boys: 45 Girls; among which were 4 Stilborn, and 3 Baſtards.

Buried 157

viz. 28 married Men, and 11 married Women: 1 Childbed Woman: 5 Widowers and 25 Widows: 9 Bachelors and 10 Maidens: 68 Children, *viz.* 49 Boys and 19 Girls.

In Leipſic.

Married 314 Pair.

Born 790 *viz.*

405 Boys and 385 Girls.

Buried 1264

Among which were 233 Men and 146 Women: 86 Bachelors, and 62 Maidens: 233 Boys and 195 Girls: 9 Women in Childbed: 98 Chriſoms, *i. e.* 53 Boys and 45 Girls: 58 Stilborn, as 36 Boys and 22 Girls.

In Berlin.

Married 669 Pair.

Born 2276

Buried 2426

In Wiſmar.

Chriſtened 168

Buried 100

In Epperies.

Born 171

Buried 116

In Rawitz.

Born 134

and juſt as many Males as Females, except two Pair of Twins.

Buried 95

among which were 15 Childbed Women and 3 Stilborn.

In Dantzig.

Born 1862

Buried 1610

Married 442 Pair.

This is merely of the City, and not of the Suburbs of *Dantzig.*

A Liſt of the ſeveral Cities and Towns of the Kingdom of Pruſſia.

In the Year 1720.	*Chriſtened.*	*Pair Marr.*	*Buried.*
In Angerberg - - - - - -	535	88	259
Balga - - - - - - - - -	638	133	395
Bartenſtein - - - - - - - -	224	63	188
Barthen - - - - - - - -	337	66	246
Brandenburg - - - - - -	936	213	576
Teutſch-Eulau - - - - -	100	25	82
Fiſhauſen - - - - - - - -	478	103	270
Gerdauen - - - - - - - -	489	98	206
Gilgenburg - - - - - -	225	56	137
Inſterburg - - - - - - -	2386	336	1398
Johanniſburg - - - - - -	378	68	188
Labiau - - - - - - - -	616	92	273
Liebſtadt - - - - - - - -	440	73	343
Lotzen - - - - - - - -	256	45	148
Luck - - - - - - - - - -	364	65	174
Marienwerder - - - - -	769	145	641
Mummel - - - - - - - -	953	168	448
Neydenburg - - - - - -	512	104	437
Neuhauſen - - - - - -	243	62	141
Neuhoff - - - - - - - -	28	6	10

	Chriſtened.	Pair Marr.	Buried.
In Oletzko	688	111	261
Ortelſburg	476	82	330
Oſterode	784	92	300
Preuſs Eulau	479	112	282
—— Marck	705	130	478
—— Holland	705	125	557
Ragnit	930	93	516
Raſtenburg	610	155	452
Rhein	482	89	193
Schacken	576	96	259
Schonberg	276	51	230
Seheſten	354	62	181
Taxium	876	157	441
Tilſit	1139	187	647
the City of Koningſberg	1682	474	1402

Of the Year 1721. *In* BRESLAW.

Buried.	
From the 25th of Dec. to the 31ſt, 1720.	38
January, 1721.	123
February	129
March	161
April	127
May	149
June	132
July	86
Auguſt	103
September	125
October	99
November	121
December, till the 24th,	89
Total	1482

Chriſtened.	
Males	610
Females	585
Total	1195

Married 405 Pair.

Among the Dead were.		
Married Men		301
—— Women		157
Widows & Widowers		208
Bachelors		92
Maidens		82
Children to ten Years of Age,	Boys	324
	Girls	248
Stilborn	Boys	42
	Girls	28
Total		1482

In *Jauer*.

Buried 114 Chriſten'd 124.

In *Vienna*.

Buried 6490 Chriſten'd 4104

So that there died 2386 more than were chriſtened.

Among the Dead were 43 Caſualties.

Beſides 8 Perſons of 90 Years old.

1	91
2	92
3	93
3	94
3	96
1	98
4	99
2	100
1	105

In *Dreſden*.

Buried 1850 Chriſtened 1396 Married 404 Pair.

Amongſt the Buried were.

Married Men		274
——— Women		206
Widowers		42
Widows		238
Bachelors		128
Maidens		93
Boys		479
Girls		400
Stilborn	Males	50
	Females	28

Among the Baptiz'd were,

Boys		701
Girls		690
Baſtards	Males	40
	Females	51

In *LEIPZIG.*

Buried.

	Married Men.	*Married Women.*	*Bachelors.*	*Maidens.*	*Boys.*	*Girls.*	*Childbed Women.*	*Chrysoms* *Males.*	*Chrysoms* *Females.*	*Still-born.* *Males.*	*Still-born.* *Females.*	*Widowers & Wid.*	*Summa*
January	28	25	13	11	15	14	1	1	1	2	3	20	134
Febr.	17	12	15	9	16	10	2	4	3	7	2	17	114
March	28	29	13	12	14	15	0	1	3	3	3	17	138
April	31	24	9	10	17	22	2	2	3	2	0	13	135
May	25	23	10	9	23	16	3	6	5	0	0	16	136
June	28	16	17	4	15	13	2	3	6	2	2	12	120
July	7	10	7	5	8	12	2	1	4	0	3	15	74
Auguſt	14	8	6	6	19	11	3	2	3	3	2	9	86
Sept.	14	14	1	2	13	18	2	4	4	1	1	7	81
Octob.	17	10	4	7	14	20	6	7	2	7	4	5	103
Nov.	12	13	3	6	20	7	1	3	7	2	0	11	85
Dec.	16	12	2	12	19	17	10	3	3	2	1	7	94
Summ.	237	196	100	93	193	175	24	37	44	31	21	149	1300

Chriſtened.

Chriſtened.

	Male.	*Females*	*Total*	*Pair marr.*
Januar.	43	31	74	13
Feb.	31	23	54	30
March	34	31	65	1
April	32	38	70	
May	31	22	53	20
June	21	31	52	24
July	37	26	63	21
Auguſt	27	38	65	17
Sept.	29	46	75	30
Octob.	38	33	71	20
Nov.	37	34	71	52
Decem.	27	20	47	1
Sum	387	373	760	268

Among the Dead were
218 from 60 to 69 Years
82 —— 70—79 old.
16 —— 80—88 ——
2 —— 90—91 ——

Beſides 1 Bachelor of 60 and 2 of 61; and 5 Maidens of 60. 62. 66. 70. and 73 Years old.

Among the Chriſtened were
8 Poſthumous:
14 Twins:
63 Baſtards:
and 2 *Jews* baptized.

So that in this Year there died 1300. Born and Chriſtened 760. Hence there are 540 fewer born than died.

In the *Marck.*

Chriſtened 16086 Amongſt which were 596 Baſtards.
Married 4613 Pair.
Buried - - 13511

More born 2575 than buried.

Among the Buried were 28 Perſons of 90 Years old and upwards, beſides 3 of 100, 1 of 101, 1 of 102, 2 of 104, 2 of 107, and 1 of 112 Years.

In the whole *Royal Pruſſia.*

Born 75275. Buried 58017. That there are more Born 17258 than Buried.

Next follows the Special Liſt of the Kingdom of Pruſſia.

	Chriſtened.	Pair Marr.	Buried.
In Angerberg - - - - - -	518	104	282
Balga - - - - - - - -	605	168	381
Bartenſtein - - - - - - -	278	65	218
Bahrten - - - - - - - -	357	64	208
Brandenburg - - - - - - -	944	222	695
Dutch-Elau - - - - -	103	20	78
Fiſhauſen - - - - - - - -	498	112	300
Gerdauen - - - - - - - -	490	75	219
Gilgenburg - - - - - - -	110	37	76
Inſterburg - - - - - - - -	2235	359	889
Johanniſburg - - - - - -	463	76	174
Labiau - - - - - - - -	569	92	277
Liebſtadt - - - - - - - -	431	94	252
Lotzen - - - - - - - - -	247	68	180
Lyck - - - - - - - - - -	352	96	183
Marienwerder - - - - -	765	162	400
Mummel - - - - - - - -	914	193	522
Neydenburg - - - - - - -	545	121	326
Neuhauſen - - - - - - -	234	59	138
Neuhoff - - - - - - -	34	10	40

	Chri-ſtened.	Pair Marr.	Buried.
In Oletzko - - - - - - - -	631	103	312
Ortelſburg - - - - - -	497	92	282
Oſterode - - - - - - - -	369	144	183
Preuſs Eylau - - - - - - -	503	113	428
——— Holland - - - - -	605	166	376
——— Marck - - - - -	657	134	335
Rangnit - - - - - - - -	791	95	336
Raſtenburg - - - - - - -	738	165	537
Rhein - - - - - - - - - -	416	108	211
Schacken - - - - - - - -	514	87	326
Schonberg - - - - - - - -	255	61	147
Scheſten - - - - - - - -	330	80	233
Taxium - - - - - - - -	870	159	508
Tilſit - - - - - - - -	1245	185	584
the Cities of Koningſberg -	1655	424	1776
Total	20668	4313	12406

In Ratisbon.

Chriſtened Males - - - 130
Females - - 120
Total 250

Amongſt which were 3 Pair of Twins, *viz.* 2 Boys and 4 Girls. Beſides 6 Baſtards, *i. e.* 3 Boys and 3 Girls.

Buried 220. *viz.* Married Men 41. Married Women 43. Young Men 11. Young Women 15. Children 110. *i. e.* 67 Boys and 43 Girls. Amongſt which, Widows 23. Lying-inn Women 2. Stilborn 2. Married 67 Pair.

In *Nurnburg.*

Buried.	
Married Men	234
—— Women	257
Bachelors	48
Maidens	73
Boys	232
Girls	191
Stilborn	18
In the Suburbs	10
Total	1063

Chriſtened.	
Males	541
Females	543
Total	1084

21 more born than died, amongſt the Chriſtened were 16 Pair of Twins.

In *Copenhagen.*

Born 2630 Buried 2247.

In *Amſterdam.*

The following Liſt contains 7 *Years,* viz. *from* 1715 *to* 1721.

Anno 1715 died 7633 Perſons.
—— —16 —— 7078
—— —17 —— 7451
—— —18 —— 8644
—— —19 —— 9726
—— —20 —— 7820
—— —21 —— 7632

In *Epperies.*

Born 214. Buried 142.

In *Dantzick.*

Born 1833. Buried 1435. Married 457 Pair.

VII. *An Account of the Diſſection of an Eye with a Cataract. By Mr.* John Ranby, *Surgeon.*

SEpt. 21. 1723. I was conſulted by one *William Sollars*, aged Fifty, who complain'd of a Decay in his Sight; upon examining his Eyes, I found two Cataracts, that in his right Eye almoſt ripe, the other juſt forming. There being no other obvious Method of relieving him, I propos'd the Operation, but firſt advis'd him to conſult ſome others of the Profeſſion, and accordingly he advis'd with my ingenious Friend Mr. *Tanner*, who, upon viewing his Eyes, declar'd them both Cataracts, but that neither of them was as yet fit for the Operation. In the mean time the poor Man fell ill of a Fever, and died the 2d of *March*. I procur'd the Right Eye, in which the Cataract was moſt confirm'd, in order to make an exact Diſſection of it: In examining it, I found the Aqueous and Vitreous Humours in their natural State, but the Cryſtalline Humor was opake and of a foul Pearl Colour, and more ſolid Subſtance than in its natural State. The generally receiv'd Opinion, that a Cataract is a Film growing behind, or ſometimes before the Pupil, made us examine both Chambers of the Aqueous Humour, with the utmoſt Exactneſs, but all to no Purpoſe, for the Aqueous Humour had its natural Tranſparency, nor could we obſerve any thing preternatural either on the *Iris* or *Uvea*, except too great a Contraction of the Pupil. This very much ſtrengthens the Opinion of *Maitrejan*, *Briſſé*, *Heiſter*, and *Valſalva*, who have ſeverally aſſerted, that a Cataract is only an Opacity of the Cryſtalline Humour, and that it naturally proceeds from a Serous

Acid, which ſo far aſtringes and corrodes its Subſtance, as to deſtroy its Tranſparency. This, *Maitrejan* confirms by an Experiment of immerging the Cryſtalline Humour in a Compoſition of three Parts Water and one of *Aquafortis*, by which he tells us it may be render'd hard and opake; but in this Point I cannot help ſiding with the learned Dr. *Pitcairn*, who has ſufficiently proved, that there is no ſuch Serous Acidity in an animal Body. To me nothing ſeems more eaſy than to deduce this Opacity of the Cryſtalline Humour from an Inflammation in the Blood, or an increas'd *Momentum* in the Fluids, with which it is ſupply'd: for in that Caſe groſſer Particles, inconſiſtent with the Tranſparency, may be impell'd into the Lymphatick Veſſels of which it is compos'd; and that there is an Inflammation is ſufficiently demonſtrated from hence; firſt, the Patient feels often a pungent Pain in the Eye, which as it is generally the forerunner of a Cataract, ſo it certainly indicates an Inflammation of the Part. Secondly, Thoſe *Maculæ*, which appear as it were ſwimming in the Air, plainly prove that there are opake Particles already enter'd the Lymphatick Veſſels, which compoſe the Vitreous Humour.

Thirdly, the *Iris*, whoſe Colour ariſes from the Blood Veſſels, as it changes from a lighter to a darker Colour, ſhews the Violence of the Inflammation, and is therefore eſteem'd a Symptom of the worſt Conſequence.

FINIS.

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VIII. ***An***

Numb. 382.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *March* and *April*, 1724.

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I. *Obſer-*

I. *Obſervations upon the Comet, that appear'd in the Months of* October, November, *and* December, 1723. *By the Reverend Mr.* Bradley, *M. A. Prof. Aſtron. Oxon. F. R. S.*

THE ſmall Comet which was ſeen in theſe Parts of *Europe*, in the Months of *October*, *November*, and *December*, 1723. was firſt obſerved in *England* by Dr. *Halley*, on *Octob.* 9. between 7 and 8 of the Clock in the Evening; it appearing then to the naked Eye not much unlike a Star of the third Magnitude. Looking at it through a Teleſcope, he ſaw ſome ſmall Teleſcopical Stars near it, whoſe Situation he noted together with the Comet's, in order to ſee which way it tended. About 9 he again viewed the Comet, and found it conſiderably moved from its former Station, having now paſſed a ſmall Star, which at the time of the firſt Obſervation was on the other ſide of it. Comparing the two Situations of the Comet together, he perceived that its apparent Motion at that time was about 8 or 9 Minutes in an Hour, in a Direction towards *Sagitta*; and that the Comet paſſed very near, if it did not wholly eclipſe the forementioned ſmall Star, whoſe place he afterwards found to be in ♒ $7^\circ\ 22'\ 15''$ with $5^\circ\ 2'$ N. Latitude. From the Situation of the Comet at the time of the firſt Obſervation, he judged that it was in Conjunction with the Star at 8 *h.* 5′. equal Time. Note that the equal, and not the apparent, Time, is likewiſe made uſe of in all the following Obſervations.

The next Day he was pleaſed to communicate to me the Subſtance of what he had obſerved, whereby I was enabled, the Night following, to ſee the Comet at *Wanſted*. The Clouds hindered me from obſerving it in the manner that I had deſigned; but I had Time enough to meaſure its Diſtance (with a Micrometer in a Teleſcope of 7 Foot) from a Star in *Aquarius*, marked ε by *Bayer*. At 6 *h*. 21′ the obſerved Diſtance between this Star and the Comet was 1° 13′ 53″, and a great Circle paſſing through the Star and Comet, made an Angle with the Vertical Circle of 60° ¼. The Comet was more ſoutherly and weſterly than the Star. By this Obſervation the Comet preceded the Star in Right Aſcenſion 1° 3′ 50″ being 39′ 5″ more ſoutherly; ſo that the Comet's Right Aſcenſion was 307° 6′ 40″ and its Declination 11° 8′ 15″ S.

The Place of ε here aſſumed is according to the *British* Catalogue, as are alſo the Places of the other Stars hereafter mentioned from which the Comet was obſerved. The Right Aſcenſions and Declinations, which are here ſet down, of ſeveral ſmall Stars that are not in that Catalogue, were determined by obſerving the Differences of Right Aſcenſion and Declination between thoſe ſmall Stars and others that were in the Catalogue, and had nearly the ſame Declinations.

The ſame Evening, at 7 *h*. 3′ a ſmall Star that was more eaſterly than the Comet, and had about the ſame Declination with it, was diſtant from it 35′ 40″. About the ſame time another ſmall Star that had nearly the ſame Right Aſcenſion with the Comet, but was more ſoutherly, was diſtant from it 39′ 58″. The Places of theſe two Stars I have not yet obſerved.

The next Night proved cloudy, ſo that I could not ſee the Comet again till *October* 12. when (the Air being very ſerene and clear) we had an Opportunity of comparing it with two or three ſmall Stars that were near it; my Uncle, the Reverend Mr. *Pound*, aſſiſting in this and moſt of the following Nights Obſervations.

At 7 *h*. 22′ a ſmall Star, whoſe Right Aſcenſion was found 304° 40′ 23″ and its Declination 7° 8′ 22″ S. preceded the Comet in Right Aſcenſion 26′ 21″ being 10′ 42″ more Northerly. Hence the Comet's Right Aſcenſion was 305° 6′ 44″ and its Declination 7° 19′ 4″ S.

At 8 *h*. 50′ the Comet was in the ſame Parallel of Declination with another ſmall Star, whoſe Right Aſcenſion was found 305° 9′ 56″ and its Declination 7° 13′ 20″ S. and preceded the ſaid Star 6′ 20″ in Right Aſcenſion. Hence the Right Aſcenſion of the Comet was 305° 3′ 36″ and its Declination 7° 13′ 20″ S. Theſe Obſervations were made with a Teleſcope of 15 Foot furniſhed with a Micrometer, as were alſo all thoſe of the following Nights.

The next Night, *October* 13. 6 *h*. 58′ the Comet followed a ſmall Star, 4′ 10″ in Right Aſcenſion, being more Northerly than the Star 11′ 45″. The Clouds did not permit us to obſerve the Place of this Star; but its Right Aſcenſion muſt be about 304° 22′ and its Declination 6° 10′ S.

October 14. the Comet was near two Stars which are the 66th and 67th of *Aquila* and *Antinous* in the *Britiſh* Catalogue, and at 8 *h*. 57′ it followed the ſouthermoſt of them 20′ 37″ in Right Aſcenſion. being 29′ 8″ more ſoutherly. Hence the Comet's Right Aſcenſion was 303° 49′ 10″ and its Declination 4° 43′ 54″ S.

October 15. 6 *h* 35′ the Comet preceded the northermoſt of the ſaid Stars 23 6″ in Right Aſcenſion, being more ſoutherly than the Star 4′ 15″. Hence the Right Aſcenſion of the Comet was 303° 24′ 40″. Its Declination 3° 51′ 3″ S.

October 21. 6 *h*. 22′ a ſmall Star, whoſe Right Aſcenſion was found 301° 7′ 17″, and its Declination 0° 11′ 50″ S. preceded the Comet 41′ 6″ in Right

Aſcenſion, being 5′ 50″ more ſoutherly. Hence the Comet's Right Aſcenſion was 301° 48′ 23″ and its Declination 0° 6′ 0″ S.

October 22. 6 *h.* 24′ a ſmall Star, whoſe Right Aſcenſion was found 301° 39′ 47″ and its Declination 0° 32′ 43″ N. followed the Comet ½ a Minute in Right Aſcenſion, being 13′ 43″ more northerly. Hence the Comet's Right Aſcenſion, was 301° 39′ 17″ and its Declination 0° 19′ 0″ N.

October 24, 8 *h.* 2′ a ſmall Star whoſe Right Aſcenſion was found 301° 24′ 57″ and its Declination 1° 9′ 22″ N. preceded the Comet 0′ 37″ in Right Aſcenſion, being 5′ 12″ more Northerly. Hence the Comet's Right Aſcenſion was 301° 25′ 34″; and its Declination 1° 4′ 10″ N.

October 29, 8 *h.* 56′ a ſmall Star whoſe Right Aſcenſion was found 301° 6′ 20″ and its Declination 2° 51′ 0″ N. preceded the Comet one Minute in Right Aſcenſion, being 23′ 40″ more Northerly. Hence the Comet's Right Aſcenſion was 301° 7′ 20″ and its Declination 2° 27′ 20″ N.

October 30, 6 *h.* 20′. The ſame Star had exactly the ſame Right Aſcenſion with the Comet, being 11′ 33″ more Northerly. Hence the Comet's Right Aſcenſion was 301° 6′ 20″ and its Declination 2° 39′ 27″ N.

November 5, 5 *h.* 53′ a ſmall Star whoſe Right Aſcenſion was found 300° 35′ 00″ and its Declination 3° 45′ 30″ N. preceded the Comet 33′ 0″ in Right Aſcenſion, being 2′ 8″ more Southerly. Hence the Comet's Right Aſcenſion was 301° 8′ 0″ and its Declination 3° 47′ 38″ N.

November 8, 7 *h.* 6′ a bright Star (placed by *Hevelius* in *Roſtro Aquilæ*, but not inſerted in the *Britiſh* Catalogue) whoſe Right Aſcenſion at this time was found 302° 21′ 30″ and its Declination 4° 28′ 40″ N. followed the Comet 1° 7′ 40″ in Right Aſcenſion, being 13′ 3″ more Northerly. Hence the Comet's Right Aſcenſion was 301° 13′ 50″ and its Declination 4° 15′ 37″ N.

November 14, 6 *h.* 20′ a Star, whoſe Right Aſcenſion was found 301° 27′ 10″ and its Declination 4° 59′ 40″ N. preceded the Comet 5′ 35″ in Right Aſcenſion, being 5′ 50″ more Southerly. Hence the Comet's Right Aſcenſion was 301° 32′ 45″ and its Declination 5° 5′ 30″ N.

This was the laſt Time that I obſerved the Place of the Comet 'till after the Full Moon; my Affairs calling me to *Oxford*, where I had no Convenience for making ſuch Obſervations.

Dr. *Halley* and Mr. *Graham* continued to obſerve the Comet 'till *November* 20; and according to both their Obſervations that Evening at 7 *h.* 45′ the Comet followed β in *Collo Aquilæ* 6° 33′ 55″ in Right Aſcenſion, being about 4′ more Northerly than the Star. Hence the Comet's Right Aſcenſion was 301° 59′ 50″ and its Declination 5° 48′ 55″ N.

The Light of the Moon daily increaſing, prevented them from making any more Obſervations, the Comet being by this time grown ſo faint, as to become in a manner imperceptible while the Moon ſhone bright. And the faint Appearance which it made before the Moon obſtructed the Sight of it, gave little Hopes of its being to be ſeen again after the Full Moon. Notwithſtanding which on *December* 3. (being then near *Cirenceſter* in *Glouceſterſhire*) I was tempted by the Serenity of the Evening, and the Uſe of a very good Teleſcope of 10 Foot, to look for it again before the Moon roſe; and I found it among ſome ſmall Teleſcopical Stars; but it appear'd ſo faint and dull, as made it doubtful, whether what I took for the Comet might not be a ſmall Star with a little Hazineſs about it. But this Doubt was cleared two Nights after; when I perceived that the Comet was moved from its former Situation, towards a bright Teleſcopical Star, from which I afterwards took its Difference of Right Aſcenſion and Declination, upon my Return to *Wanſted*, on *Dec.* 7. This Star's Right Aſcenſion was then found 303° 39′ 20″ and its Declination 7° 32′ 30″ N. And *Decemb.* 7. 6 *h.* 45′ the Comet followed it 3′ 15″ in Right Aſcenſion, being 14′ more Northerly than the Star. Hence the Comet's Right Aſcenſion was 303° 42′ 35″ and its Declination 7° 46′ 30″ N.

This was the laſt Night that I ſaw the Comet, tho' I belive I might have continued to have obſerved it, had not an interrupted Succeſſion of cloudy Evenings prevented ſo long, that it became uncertain where to look for it.

The forementioned Obſervations are the Principal of all that were made at *Wanſted*; and moſt of them being taken from Stars which are not in the *Britiſh* Catalogue, whoſe Places therefore are here determined, only by comparing them with ſome that were; it cannot be ſuppoſed that the Comet's Places deduced from them are altogether exact. For which Reaſon I have all along ſet down, not only the Place of the Comet and Star where it was known, but alſo the Particulars of the Obſervation, that if any hereafter ſhould be willing to examine the Tract of this Comet more nicely, they may know where to find the Stars from which it was obſerved. The Places of the Stars here ſet down are abundantly ſufficient for that Purpoſe, as will appear from the following Table, which contains the Longitudes and Latitudes of the Comet deduced from the foregoing Obſervations, together with the Places of the Comet calculated from the Theory of Gravity, for the Times of Obſervation on the ſeveral Days therein mentioned, as alſo the Differences between the Obſerved and Computed Places. Thoſe Differences not exceeding one Minute, ſhew that the Obſervations are not only conſonant to each other, but that the Places of the Stars are likewiſe near the Truth, ſince the Comets Places deduced from them are found all along to agree ſufficiently near with the Theory of Gravity; the Truth of which having long ſince been eſtabliſhed by its great Author Sir *Iſaac Newton*, and my worthy Collegue Dr. *Halley*, needs not the Confirmation of ſo ſhort a Series of Obſervations as was made of this Comet. But ſhort as it is, I preſume 'twill be no eaſy Matter to account for the Obſervations with the ſame Degree of Exactneſs any other way, than by that Theory, according to which the following Computations are made.

1723. Temp. Æquat.				Comet. Long. Obſervat.				Lat. Bor. Obſerv.			Comet. Long Comput.				Lat. Bor. Comput.			Differ. Long.	Differ. Latit.
	D.	H.	′		°	′	″	°	′	″		°	′	″	°	′	″	″	″
Octob.	9	8	5	♒	7	22	15	5	2	0	♒	7	21	26	5	2	47	+ 49	− 47
	10	6	21		6	41	12	7	44	13		6	41	42	7	43	18	− 30	+ 55
	12	7	22		5	39	58	11	55	0		5	40	19	11	54	55	− 21	+ 5
	14	8	57		4	59	49	14	43	50		5	0	37	14	44	1	− 48	− 11
	15	6	35		4	47	41	15	40	51		4	47	45	15	40	55	− 4	− 4
	21	6	22		4	2	32	19	41	49		4	2	21	19	42	3	+ 11	− 14
	22	6	24		3	59	2	20	8	12		3	59	10	20	8	17	− 8	− 5
	24	8	2		3	55	29	20	55	18		3	55	11	20	55	9	+ 18	+ 9
	29	8	56		3	56	17	22	20	27		3	56	42	22	20	10	− 25	+ 17
	30	6	20		3	58	9	22	32	28		3	58	17	22	32	12	− 8	+ 16
Nov.	5	5	53		4	16	30	23	38	33		4	16	23	23	38	7	+ 7	+ 26
	8	7	6		4	29	36	24	4	30		4	29	54	24	4	40	− 18	− 10
	14	6	20		5	2	16	24	48	46		5	2	51	24	48	16	− 35	+ 30
	20	7	45		5	42	20	25	24	45		5	43	13	25	25	17	− 53	− 32
Dec.	7	6	45	♒	8	4	13	26	54	18	♒	8	3	55	26	53	42	+ 18	+ 36

In order to determine the Orbit of this Comet, I ſuppoſed it to deſcribe a *Parabola* agreeable to what is delivered in the third Book of Sir *Iſaac Newton's Princip. Math.* and then I found the Inclination of the Plains of the Orbit and Ecliptick 49° 59′. The Place of the Aſcending Node ♈ 14° 16′. The Place of the Perihelion ♉ 12° 52′ 20″. The Diſtance of the Perihelion from the Node 28° 36′ 20″. The Logarithm of the Perihelion diſtance 9.999414. The Logarithm of the Diurnal Motion 9.961007. The Time of the Comets being in its Perihelion, *Sept.* 16. 16h 10′ equal Time. In its Orbit thus ſituated, the Motion of the Comet was Retrograde or contrary to the Order of the Signs.

From theſe Elements, by the Help of Dr. *Halley*'s general Table for Comets (to which they are adapted) I computed the Places in the forgoing Table; which agreeing with the obſerv'd Places as near as the Obſervations themſelves agree with one another, ſhew that it would be a vain Attempt to pretend to determine the true Ellipſe in which this Comet moves, or its Periodical Revolution, from ſo ſmall a Part of its Orbit as that was, which it deſcribed between the firſt and laſt of the forgoing Obſervations; this therefore muſt be left to Poſterity, eſpecially ſince it is certain, that this Comet is not one of thoſe of which Obſervations have hitherto been tranſmitted to us, ſufficient to determine the Situation of their Orbits.

The *Nucleus* of this Comet was very little, for it appear'd but of a ſmall Diameter when I firſt ſaw it, although it was then above three times nearer to the Earth, than the Sun is at its mean Diſtance. Its Tail was then hardly diſcernable with the naked Eye, but through a Teleſcope one might perceive a faint Light extending itſelf above a Degree from the Body.

I have not yet heard that this Comet was ſeen before *October* 6. although it was in a proper Situation to have been obſerved in the Morning, moſt part of *September*, eſpecially from the Time it was in its Perihelion, 'till near the End of that Month. For about that Time it croſſed the Milky-way between the Maſt of the Ship and the Head of the great Dog, paſſing between the bright Stars in the Body and Tail of the great Dog, towards the Head of the Dove, were it was about *September* 29. being by that time got ſo far towards the South-Pole, as not to riſe above our Horizon. From thence it paſſed under the Tail of *Xiphias* within about 15° of the South Pole of the Ecliptick; and moving on between the Head of *Hydrus* and the bright Star in *Eridanus* called *Acarnar*, it went by the Stars in the Body and Neck of the Crane about *October* 5. when it came again above our Horizon. From hence paſſing under the Tail of the Southern Fiſh, and between the Stars in the Shoulder of *Capricorn*, it croſſed the Ecliptick, *October* 8. in about 8° ⅓ of *Aquarius*. From thence it moved on by the Hands of *Aquarius* and *Antinous* towards the Head of the *Eagle*, according to its Courſe before deſcribed.

The Comet was in Oppoſition to the Sun *Octob.* 1. when it had near 74° Southern Latitude, and alter'd its Longitude two Signs in a Day. About *October* 3. it was in its Perigœon, or neareſt Diſtance to the Earth, being then almoſt ten times nearer to it than the Sun is at its mean Diſtance; and its apparent Motion was then about 20° in a Day, and when I laſt ſaw it, 'twas above twice as far off as the Sun.

II. *Extract of a Letter from the Right Honourable the Lord* Paiſley, *F. R. S. to Mr.* George Graham, *F. R. S. With ſome curious Figures of the ſame Comet.*

HIS Lordſhip being at *Witham* in *Eſſex*, where he had the Advantage of a very clear Sky, firſt diſcover'd this Comet on *Friday* the 11th of *Octob.* laſt about 7 in the Evening; it then appeared not much unlike a Star of between the 4th and 5th Magnitudes, but a Hazineſs round the Head, and ſome Light ſtreaming from it on that Side that was oppoſite to the Sun, induced him immediately to look upon it as a ſmall Comet; which his Obſervation the next Evening abundantly ſatisfy'd him of. His Lordſhip was very particular in the Notice he took of its Appearance, and was pleaſed to communicate the three curious annexed Figures [*Fig.* 1, 2, 3.] of it, repreſenting it on three ſeveral Nights, *viz.* the 11th, 13th and 15th of the ſame Month; ſome time after which the Tail became ſo inconſiderable as hardly to deſerve any farther Deſcription; as will be readily judged from the Decreaſe of it between the 11th and 15th Days of the Month. The Tail was viſible on the 11th to near a Degrees Diſtance from the Body, as his Lordſhip found by comparing it with ſome known Diſtances in the Heavens; it was of a dusky Light not unlike a Cloud growing darker and darker towards its Extremity, as is expreſs'd in the firſt Figure, where, as well as in the two following, the white Speck in the Head is intended to expreſs the Brightneſs of a ſmall Star; from the Compariſon of which with the Tail the Brightneſs of the latter may in ſome ſort be collected: The Tail appear'd ſharper, and not ſo much ſpread in the two following Obſervations, and in the laſt did not exceed one third Part of the firſt Length; it was then of a much darker Colour, which made the Difference between that and the Head more obſervable, the Head yet appearing ſufficiently bright. For ſome following Nights his Lordſhip's Obſervations were interrupted by cloudy Weather, after which the Comet was ſo far diminiſh'd, as only to be known by its Motion, its Appearance being no ways diſtinguiſhable from that of a ſmall nebuloſe Star.

III. *Obſervatio ejuſdem Cometæ ab Illuſtriſſimo Domino* Franciſco Bianchini *habita* Albani *Menſe* Octobri, 1723. *& ab eodem Ulyſipponem miſſa* P. Joanni Baptiſtæ Carbone *Soc JESU. Commuuicavit* Iſaacus Samuda, *M.D. Col. Med.* Lond. *L. S. R. S.*

DIE 17 *Octobris*, poſtquam Jovialium Comitum ſitum obſervaſſem fortè in Conſtellationem *Capricorni* oculos conieci; cumque aſtra ſingula percurrerem, in quamdam veluti nebuloſam ſtellam incidi, cæteris ſane grandiorem, quam tamen ibidem loci numquam antea obſervaram. Rei novitate perſcrutaturus, eo Teleſcopium direxi, ſtatimque Cometen eſſe deprehendi; ſiquidem tenuiſſimæ nebulæ globus apparuit, ejuſque in medio veluti lucidus nucleolus. Idem quoque nudis oculis diſcernere licuit; & præter

ter nebulam, ſeu Cometæ atmoſphæram, brevem quoque caudam, quæ ad orientem vergebat, eratque hujuſmodi. [*Fig.* 4.]

Ne me igitur ea occaſio præteriret, conſuetas circa illum obſervationes inſtitui, ut ejus Longitudinem, Latitudinem, propriumque motum deprehenderem.

Et quidem prima nocte, die nempe 17. ſupradicta, tranſiit per Meridianum (qui penè cum Romano coincidit) circa horam ſeptimam 44′ poſt Merid. ejuſque diſtantia à Zenith, 69° 29′.

Hora 8. 11′ 30″ diſtantia Cometæ à *Fomahant Aquarii*, intercepta eſt, 20° 33′ & hora 8. 17′ 30″ diſtabat à Stella β in humero dextero *Aquarii*, 21° 8′. Proindeque verſabatur Cometes in 11° 54′. *Aquarii*, cum Latitudine Auſtrali ab Ecliptica 11° 10′ circiter.

Die 21. erat adeo proximus Stellæ ε in Lino ſupra manum ſiniſtram *Aquarij*, quàm ipſa ε eſt proxima Stellæ μ minori in eodem Lino, conſtituebatque Cometes cum utraque Stella ε, μ rectam Lineam, ſic [*Fig.* 5.] Ex hac igitur obſervatione, & ex Aſcenſionis rectæ nec non declinationis differentiâ inter Cometen, & ſupradictam Stellam ε quam diligentiſſime obſervavi, infertur locus Cometæ fuiſſe in 6° 45′. *Aquarii* cum Latitudine Boreali ab Eclipt. 8° 5′.

Hinc etiam infertur qualis Cometæ motus proprius fuerit, & quale iter ; per planum ſcilicet circuli maximi ſecantis Eclipticam in gradu 9 *Aquarii*, & conſtituentis cum eadem Ecliptica angulum 80 graduum circiter.

Reliquis diebus eadem ſemper proportione movebatur, magiſque in dies elongari à terra viſus eſt.

Parallaxim nullam ſenſibilem, etſi pluries inten taverim, deprehendere potui; proindeque maxima ejus diſtantia à terra credenda eſt.

Hactenus Illuſtriſſimus Dominus *Franciſcus Bianchini* in Mathematicis Scientiis apprimè eruditus, & in obſervando, quoad noverim, accuratiſſimus. Ejus obſervatio à noſtra, mea ſcilicet, ac Prioris Dominici *Capaſſi* vix in uno aut altero minuto quoad latitudinem diſcrepavit, cæterùm omninò conformis. Quapropter nec illam hic arbitror apponendam.

IV. *Obſervations about* Waſps, *and the Difference of their Sexes. By the Reverend Mr.* Derham, *F. R. S. Prebendary of* Windſor.

IN the Beginning of *July* laſt 1723. having frequent occaſion to be on the Top of our *Collegiate Chapel* in *Windſor Caſtle*, I obſerved many *Waſps* flying about it; and particularly frequenting a Covering of Deal Boards, and the Pieces of Timber lying on the Leads. I took notice that moſt of theſe *Waſps* were of a larger ſort than uſual, and thought they came thither to gnaw the Wood, and to carry it away in Mouthfuls to build their Neſts; the Artifice of which I have taken notice of in my *Phyſ. Theol.* particularly *B.* 4. *ch.* 11. *note* 21. and *chap.* 13. *not.* 12. But having caught ſome of them, and amongſt the reſt a large *Queen-Waſp* (which ſort of Waſp, by the by, never engages in any Labours of the Colony) I began to be more ſtrict in my Obſervations, and on *July* 6. I obſerved a Cluſter of only three *Waſps* cloſely embracing each other; one of which was a large *Fe-*

male Waſp, the other two of a leſſer ſort. This more excited my Curioſity. And ſoon after, I found 8 or 10 *Waſps* cloſely hanging together, and divers other ſuch like Parcels. In the midſt of all which was conſtantly a *Queen-Waſp*, and only one; the reſt being always of a different Sort from either the Queen or the common Waſps; which gave me a Suſpicion of their being *Male* and *Female*. And therefore examining another Company of them with greater Strictneſs, I found the *Queen-Waſp*, *in coitu*, with one of the other Waſps, ſo cloſely joined Tail to Tail, that it was ſome Time before they were parted.

After this I caught all the Waſps I could, on the top of our Chapel, but could not ſee one of the *common labouring Waſps* among them; but all were for the moſt part *Male Waſps*, with now and then a *Queen*, or *Female*, among them, and ſhe generally *in coitu*.

And now from this Hiſtory of my Obſervation, it appears, That there are three ſorts of *Waſps*; The *Queens*, or *Females*; the *Kings*, or *Males*; and the *common Labouring-Waſps*; each of them very diſtinct.

The *Queen*, or *Female-Waſp* (by many called the *King-Waſp*) is much longer in the Body, and larger than any other Waſp.

The *Male Waſps* are leſſer than the *Queens*, but as much longer and larger than the *common Waſps*, as the *Queen* is longer and larger than theſe. Theſe *Males* alſo have no Stings, which the *Queens* and *common Waſps* all have. And theſe are thoſe which *Moufet* ſaith Authors call 'Ακέντρȣς, and take to be *Females*, although he is of another Opinion, imagining all Waſps to have Stings; upon his examining a Waſp's

Neſt, at *Ham*, *Anno* 1587. in which he found no Waſps without a Sting. But I wonder how that curious Enquirer miſſed of theſe ſting-leſs Male *Waſps.* Surely he was too haſty in his Examination, and not being aware of the difference, he thought the *Males* (which are but few in number to the *labouring Waſps*) were the ſame and had Stings as well as the reſt; or elſe he made his Enquiry at a Time when perhaps the Males had deſerted the Neſt, which probably they may do, as the *Male* or *Drone-Bees* are forced to do: or elſe the Year 1587. (in which *Moufet* made his Obſervation) might produce fewer *Waſps*, at leaſt fewer *Male Waſps*, than this laſt Summer of 1723. did, in which I made my Obſervation; which was obſerved to have a greater abundance of Waſp-Neſts than hath been known in many Years. And in all the Neſts that I ſearched into, I conſtantly found *Male Waſps*, either many or few, according to the Size of the Neſt, and Number of Waſps therein. And the Part of the Neſt where theſe Males are bred, or at leaſt where I found them moſt to reſide, was chiefly the two uppermoſt Cells, or Partings, between the Combs, but one.

But to return to the Diſtinction of our *Male Waſps.* Another thing by which they may be known from other Waſps, is their *Antennæ*, or *Horns*; which are longer and larger than either thoſe of the *Queen*, or *common Waſps*; and with them they ſeem, in running, to feel more than the others do.

But the grand and chief Difference, are the *Parts of Generation* of theſe *Male Waſps*, quite different from other Waſps. Which I diſſected with all Care, and ſhall give a Deſcription of, as well as I can, without Figures, which I could not get drawn, partly for want of a Deſigner, and partly from my Removals between *Windſor* and *Upminſter*: in which Time the

Parts are ſo dried up, or eaten with Mites, that they cannot be drawn 'till the Return of Waſp time.

For the Diſcovery of theſe Parts, if the *Alvus* be preſſ'ed, an *Horny* or *Shell-like Part* will be thruſt out, of a ſhining black Colour, which conſiſts of two Parts like Shells, ſomewhat reſembling the *Caſtagnets* uſed in Dancing; at the extreme Part of each of which grows an *Hook*, ſomewhat like thoſe of the Earwig's Tail, but much leſſer; in the Middle, between theſe Hooks, appear three Parts, the middlemoſt of which is a ſtiff brown *Tube*, very curiouſly made, with the Fore-part like a Spoon or Ladle, and the other End (within the Body) is neatly branched and braced to each Side within the two Shells I ſpake of. A little above which Branching, is a *φυματιον* or *Swelling*, like that of a Dog's Pizzle, and perhaps ſerves for the ſame Uſe, if this Tube is (as I imagine it is) the *Penis* of the Waſp.

On each Side this *Penis*, lies a ſtiff Part (in Number two) branched at the Top with ſomewhat like Hairs, giving them the Reſemblance of *Bruſhes*. At the Bottom of which are two curious black *Cells*, with an *Opening* on one Side like that of the *Concha Veneris*, with ſmall whitiſh Hairs growing on one Edge thereof. What the Uſe of theſe two Bruſh-like Members may be, I know not, unleſs it be to ſtrengthen and ſupport, or direct the *Penis in coitu*, or provoke therein.

Behind all theſe Parts, which I have deſcribed, more within the Body lies a long contorted white *Veſſel*; which at firſt I took to be the real *Penis*, penetrating the *Ladle-like Tube* I ſpake of. But upon farther Examination, I rather take it to be the *Spermatick Veſſel*.

As to the Uſe of the two little *Hooks* I mention'd at the End of the *Uropygium*, or *Shells*, I take them to be, to catch hold of the *Female's Podex*, and to direct and aſſiſt the Penetration of the *Penis in coitu.*

As for the *Parts of Generation* in the *Queen*, or *Female-Waſps*, nothing was to be ſeen ſo remarkable as in the *Male*; but thoſe Parts are very like what we ſee in the *common Labouring-Waſps:* Indeed, with the moſt accurate Obſervations I could make with my Microſcopes, I could not perceive any Difference at all. For which Reaſon I ſuppoſe it is that moſt of the Writers upon *Waſps* and *Bees*, have been very confuſed and wavering about the Sexes of theſe two Tribes of Inſects. It would be endleſs to cite the Authors and their Opinions, eſpecially concerning the *Bee-Tribe.* I think *Swammerdam* (*a*) (who as he was one of the firſt that rejected Equivocal Generation, ſo was one of the moſt judicious Writers of Inſects) that his Opinion, I ſay, is the moſt juſt, *viz.* That of *Bees*, there are three Sorts, *viz.* 1. Rex, *aut veriùs* Regina, *ſiquidem ſequioris ſexûs eſt.* 2. Fuci, *qui Maſculi proprie ſunt.* 3. Apes Operariæ, *quarum Sexum diſtinguere non poſſumus, cùm in iis nec Maſculas nec Fœminas partes obſervemus: quæ perbellè diſtinguntur in* Fucis *ſeu* Regibus, *&* Reginis, *quæ tralatitio errore* Reges *ſolent ſalutari. In* Reginis *certè invenimus* Ovarium *apud incomparabilem illum Anatomum* Joh. van Horne, *&c.*

As for what is related by *Ariſtotle*, *Pliny*, *Virgil*, or any other ancient Authors, or by our more numerous Moderns, concerning the Production of *Waſps* out of *Horſes*, or *Bees* out of *Oxen* or *young Bullocks*; as alſo of their *Polity*, their *Emperours*, *Kings*, *Dukes*, and *common Subjects*, their exact *diſcipline and Juſtice*, their ſtrict *Temperance*, and other *Virtues*, with a great deal more of ſuch like Stuff: This is ſo very whimſical, that it is not worth while to take any farther Notice of it: But there is a Story ſeriouſly told by *Mouſet*, (*b*) that deſerves our Obſervation, *viz.* That *in the Year* 1582, *being on the higheſt Ridges of the* Cartmel-Hills, (I ſuppoſe in *Lancaſhire*) *he ſaw among the Rocks two Species of Waſps deſperately fighting: That they differed only in Magnitude; that the larger truſted to their Strength; and the leſſer to their Numbers, there being ſix of the leſſer engaged againſt only one of the larger ſize, and that the Battle was not in the Air, but among the Graſs, and laſted for ſome Hours in the hoteſt Sun, not being at an end in two or three Hours ſpace.* The Cauſe of this Engagement *Mouſet* thinks was, that the *great Waſps* are wont to rob the *leſſer* of their Honey and Young, or do them ſome other ſuch like Miſchief; and the *leſſer* being very revengeful, and naturally full of Courage, did outbrave even *Mars* himſelf in aſſaulting their Enemy. But this Engagement I take to be ſuch another, as that which I have given the Hiſtory of, namely one under the Conduct of *Venus*, not of *Mars*.

And as there is no Doubt to be made of its being ſuch, and that the Engagement ſeen by *Mouſet* was on the higheſt Tops of *Cartmel* (*in ſummis Cartmeli montium jugis*) as that I ſaw was on the very Top of our *Chapel*, it may deſerve Obſervation, whether the *Waſps* ever copulate in lower Places, obvious to Diſturbance, and every one's Eye, or only on ſuch Eminencies where they can be more out of Sight, and conſequently in greater Safety: And if at any time they ſhould be found in Copulation, they may all

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(*b*) Mouſet Theat. Inſect. *l.* 1. c. 8.

all with Safety be ſeized with the naked Hand, provided it can be ſecured againſt the *Queen-Waſp*, which is the only one in the Company that is provided with a *Sting*.

For a Cloſe of theſe Obſervations about the Sexes of Waſps, I ſhall take Notice of *Mouſet's* Experiment, (which I try'd) *viz. If you take a Waſp by the Feet, and ſuffer her to buz, that thoſe Waſps, which have no Stings, will fly to her, but not any that have Stings. Which ſome,* he ſaith, *uſe as an Argument to prove that ſome Waſps are Males, ſome Females.* This Experiment I was minded to try with a *Queen-Waſp*, more eſpecially, not knowing but that *Waſps*, particularly the *Males*, might be as fond of their *Queens*, as the *Bees* are of theirs, who will not forſake them, but will live and die with them. But I did not find it to ſucceed ſo among the *Waſps*. For although I put ſome *Queen-Waſps*, and others alſo, near the Entrance of ſome large *Waſp-Neſts*, yet I did not ſee any Flock near them, only now and then one of the common Waſps, for a little while, to ſee their Fellow confined. But indeed the *Queen-Waſps* which I confined were weak, and did not buz long; as alſo the time of Copulation was probably paſt, it being *Auguſt* 12. when I try'd the Experiment.

V. *Ob-*

V. *Obſervationes duæ rariores, ex Literis Viri Doctiſſimi* Johannis Huxham, *M.D. ad* Jacobum Jurin, *R. S. Secr.*

OBSERVAT. I.

Ingens Omentum.

VEterani cujuſdam Militis uxor dolore colico & vomitu diu laboraverat, quibus ſupervenit ventris tumor durus, qui auctus indies magnam in molem excrevit. Jam evomuntur omnia, bilis atra, ſincera, tandem & ipſæ fœces alvinæ. Dolor vero perſæpe immanis in ſiniſtro præſertim hypochodrio, fotus, cathartica, enemata, anodyna, plurima Pharmacopoeio *Collins* adhibentur; incaſſum omnia; permanet dolor, increſſit uſque tumor, digitorum preſſui haud magis cedens quam ſi lignum fuiſſet. Ex hocce tumore plura quaſi tubera enata ſunt, quorum unum alterumve caput puerile, alia pugnum virilem, magnitudine æquabant: eminebat autem maximum in ſiniſtro hypochondrio, ubi graviſſimum perſenſit dolorem, ita ut ſæpius exclamaverit ſe voluiſſe tumorem ibidem loci fore perfoſſum. Increſcente tumore facta eſt valde dyſpnoica. Tanta tamque atrocia per menſes fere 14 perpeſſa fuerat mulier, cum tandem grata venit morborum requies, mors.

Mihi enarravit caſum Pharmacopola, rogavit inſuper ut viſcera inciſo abdomine inſpicerem, quod cum alio medico feci perlubens.

Nudato cadavere montoſum obſervavimus ventrem; reliquum vero corpus ſumma macie confectum. Secto abdomine in conſpectum ſe dedit ingens quaſi ſebi maſſa (niſi quod colore fuit minus alba) totum ventris cavum adimplens, ita ut nec ventriculus, nec jecur, neque inteſtina apparerent ulla; adhæſerat enim omentum hoc enorme peritonæo pluribus in locis, in utroque præſertim hypochondrio, ubi & latior & firmior fuit cohærentia. Secto autem in modum crucis peritonæo, & ab omento ſeparato, conſpeximus illum in pelvim fere detruſum, omentum etiam toti jocinoris cavo annexum, ventriculum hac mole oppreſſum, uti & duodenum, colon & jejunum: cum adipoſo renum involucro (ſiniſtro præcipue) connexum & quaſi confuſum fuit; adeo ut colon, altius paulo quam in rectum deſinit, ſebo hocce concreto fuit penitus involutum, hinc fœcibus præcluſum iter, hinc illæ lachrymæ, hinc dolores illi ſæviſſimi, quibus ante mortem excruciata fuerat mulier: & ut hic obiter notem, per plures dies ante mortem nec ſua ſponte nec vel clyſmatibus acerrimis irritata alvus fœces dejecerat ullas.

His luſtratis ingens ſeparavimus omentum à jecore, ventriculo, ſchirroſo pancreate, inteſtinis, tandem & à meſenterio, & ab interna peritonæi lamina renibus inſtrata. Splen fuit hac maſſa quodam modo demerſus, contractior & quaſi coriaceus. Hunc quaſi ſebi montem exemi denique & per famulum noſtrum domum miſi: erat autem animus omentum hoc morbidum accuratius perſpiciendi, cumjam enim adveſperaſceret defuit opportunitas.

Pendebat omentum hocce ℔ xvi ½ *Avoird.* nec tamen abſtulimus unam ſaltem alteramve libram partibus adhærentem. Pondus hercle maximum! Si conſideremus ex obeſiore homine omentum vix libras pendere tres, majus certe inveni antehac nunquam, quanquam & ſecui & diſſecta vidi plurima hominum cadavera. Fateor equidem longe majus à *Gregorio Horſtio* in ſuis obſervat. memoratum fuiſſe, majus etiam in Ephemerid. *German.* ann. x. veruntamen hoc quoque noſtrum monſtris annumerari merito debet.

Hujus & in extima parte, nec non in interiore ſubſtantia, plura obſervavi vaſa ſanguinea, eaque certe maxime dilatata, quorum aliqua penna anſerina majora, aliqua vero quaſi in aneuriſmatibus terminantia. Ex horum aneuriſmatum (ſi ita dicam) maximo uncias circiter ſex nigricantis ſanguinis extraxi cum quibuſdam grumis albidioribus; anne adipis particulæ à venis epiploicis abſorptæ atque morbo congelatæ?

Ex plurimis lobis conflari mihi videbatur maſſa hæcce, ſibi invicem arcte hærentibus; aliquos tamen ſeparavi, quorum pauci pomo minori fuere magnitudine æquales, forma haud abſimiles. Media hujus pars, cætera durior, cultro haud facile cedebat.

In iſthoc cadavere ſequentia etiam fuere obſervatu digniora. Inferior hepatis pars in ſchirrum abierat. Ex veſica fellea plures exemi calculos, carbonem foſſilem colore referentes, friabiles, aquam innatantes, quos bilem haud improprie nominares ſplendidam; his quippe quum inerant plurimæ particulæ micantes, haud dubio ſalinæ.

Meſenterii glandulæ fuere ſchirroſæ, imo quædam in ſubſtantiam pene lapideam induratæ. Inteſtina tenua fuere inflammata, colon cum cœco fere totum gangrena correptum, etiam proceſſus ipſe vermicularis.

Ren uterque fuit ſanus ſatis: dexter autem emiſit ureteres duos: cum vero renem diſſecuiſſem, percepi facile, quid eſſet in cauſa, cur ureter hic fuit duplex. Renis enim pelvis ſepto quodam fuit diviſa, digitum tranſverſum craſſo, ejuſdem perfectè ſubſtantiæ cum reliquo renis parenchymate.

Hic fuit ren quaſi duplex, cui duplex pelvis, ureterque duplex.

Erant in imo abdomine ſeri ſubcruenti libræ quaſi duæ.

Malum hoc immedicabile penitus viſum eſt, niſi quis morbi principiis obſtitiſſet.

OBSERVAT. II.

Salivæ Color inſolitus.

DOminus *Fox*, quadragenarius, gracilis, bilioſus, per menſem unum alterumve elanguerat, nauſea, ictero & doloribus colicis vexatus: tandem ex epoto largiter vino pomaceo vehementiſſimo colices paroxyſmo correptus me conſulit, Vomitabat omnia, urinæ parum reddebat coloris lixivii, quæ ſedimenti plurimum deponebat ſubviridis.

Ego ſtatim illi potionem ex Ipecac. emeticam propinari jubeo, infuſo dein C. Bened. affatim hauſto plurima viſcoſa, bilioſa, rejecit: tum enemate Terebinth. injecto alvus bis terve reſpondit. Ex ſumpta miſtura anodyna vomitus & tormina comprimuntur; jam de languore ſummo & abdominis diſtentione conqueſtio; paulo etiam poſt recrudeſcit dolor. Bolum præſcripſi ex Jalap. ℈j. Calomelan. gr.viij. Spec. Diambr. gr. vj. Laud. ſolid. gr. j. Syrup. de Sp. Cerv. q. ſ. quam primum ſumend. cum Tinct. ſacr. ℥ij. Hinc omnia pacata. poſt horas 12 ter fundit alvus perliquida, bilioſa. Ex ſumpto dein hauſtu anodyno nox tranquilla. Mane de dolore & faucium tumore queribundus craſſa, ſubfuſca, expuebat; mox abunde effluit ſaliva colore viridiſſima, bili porraceæ quam ſimillima, niſi quod tenuior. Perduravit fluxus hic ſalivæ viridis horas quaſi 40, quo tempore ſputavit quantum vix caperent ſextarii duo: ad flavedinem dein vergebat color fluoris eo uſque dum ad inſtar ſolutionis Gut. Gambæ evaſit: quantitas autem aucta potius quam diminuta.

Perſtitit & hicce color per horas etiam quadraginta, dein ſenſim pellucida facta eſt ſaliva, atque ſubito, uti oborta eſt, evanuit penitus ſalivatio.

Intra biennii ſpatium bis terve in icterum inciderat ante hujus morbi acceſſionem. Decem abhinc annis maxima ſalivatio ſponte erumpens hunc ipſum hominem in ſummum vitæ diſcrimen intulerat; ſalvus autem evaſit ope doctiſſimi expertiſſimique Medici, D. D. *Pyne.* Tunc vero temporis ne vel ☿ii hilum ſumpſerat, nec ullo modo fuerat colorata ſaliva.

In hac hiſtoria obſervatu haud indigna quædam occurrunt. Salivatio nempe ſponte exuſcitans, vel ex tantillo forſan Calomel. excitata. Calomel autem probe paratum novi, quippe qui plurimas ejuſdemet doſes, quarum aliquas ad ſcrupul. integrum, exhiberi juſſeram, ne vel minimo ſequente ſalivationis ſigno.

Occurrit porro, quantum ego ſaltem ſcio, ſalivæ inauditus color. Saliva viridi ita penitus tincti erant dentes & fauces quaſi ærugine obducti fuiſſent: permanſit dentium color viridis quatuordecim poſt diebus quam convaluit.

Notandum præterea fluxum hunc ſalivæ fuiſſe criticum, quo judicatus eſt & icterus & colicus morbus; ab inceptante enim ſalivatione ne vel minimus dolor ventris, & color cutis ſubviridis ſenſim evanuit, urinam etiam reddidit copioſius; ſed ſubnigram. Ille vero, qui ante hunc fluxum valde languidus fuit & jam quaſi moribundus maximum ſalivæ evacuationem εὐφόρως φέρει.

Serum Ictericorum tinctum eſſe bile omnibus notum eſt: nec alicui forſitan videatur magis mirandum ſerum flavum poſſe per glandulas ſalivares excerni quam per ſeroſa cutis vaſcula, quod tamen ictero laborantibus contigiſſe ſæpius obſervavimus, admoto prius epiſpaſtico: de urina bilioſa nil dicam. Unde vero color ſalivæ viridis haud ita facile dictu. Hujus opinor ego cauſam procatarticam eſſe pomacei vini potationem.

Bili ſi miſcueris acidum color oritur viridis. Hinc torminoſæ infantum dejectiones virides, lacte in ventriculo aceſcente. Hinc porracea bilis. Ponamus jam acidum pomaceum a venis lacteis vel & a meſaraicis forſan, abſorptum, ſeroque ſanguinis bilioſo permixtum: quid eveniret inde? Credo equidem flavi ſeri coloris in viridem permutatio. Profiteor hercle, acida quæcumque a vi corporis vitali in alcalinam naturam mutantur, etiam ipſum vitrioli acidum: langueſcentibus autem viribus, concoctione læſa lympha effœta facta, facta & inerte bile, queis adde ſanguinem tardius circulantem, acida parum ſubacta, nec in primis viis, nec in ipſo ſanguine, acidam exuunt naturam. Hoc teſtantur acidi debilium ſudores, aciduſque in ore ſapor a febribus inflammatoriis convaleſcentium, quibus ad reſtinguendam febrem largius fuerat exhibitus acidus potus. Sæpius certe memini me vinum pomaceum largiter potâſſe brevique urinam ipſiſſimum potus odorem ſpirantem reddidiſſe. Curabam nuperrime mulierem ſumma hæmorrhagia, durante diu, correptam, cui inter cætera plurimum fuerat exhibitum Elix. Vitriol. Mynſich. ceſſavit tandem hæmorrhagia, debilis autem inde reddita mulier incidit in leves ſpaſmos, artuumque dolores, ventris tormina diarrhœa comitante, plures excrevit alvus dejectiones viridiſſimas, ſtercoris vaccini ſimillimas, taleſque per plures dies, quanquam Rhabarb. fuerat ter exhibitum ad expurgandas acidi particulas, inteſtinis adhærentes. Hic ſal acidus ſanguini immiſtus, poſt dies plures iterum è ſanguine per inteſtina rejectus eſt, vi vitæ nullo fere modo ſubactus.

Microſcopio olim obſervavit *Leeuwenhoekius*, quod chyli ſales acidi a bile retundantur & comminuantur, quum vero in jecore ſubſiſtat obſtructa bilis, integris viribus vaſa lactea permeat acidum. In ægroto autem noſtro bilem obſtructam fuiſſe, patuit inde, quod alvus fuerat compreſſior, & fœces ante vomitum medicamento commotum reddidit albidas.

Hypotheſis fortaſſe noſtra illuſtretur magis, ſi perpendamus unde oriatur virginum καχεκτῶν color herbeus (ut cum *Plauto* loquar). His enim fructus immaturi, acetum atque id genus alia, ſummæ ſunt cupidiæ, vitiatis autem concoctionis organis, parum immutata ſanguini miſcentur, bilemque & ſanguinem obruunt acido. Hinc glandulæ obſtructæ, hinc facies ex viridi pallida, tumor hinc hydropicus; atque quod non levis eſt momenti jecur ſchirroſum ex chloroſi præmortuarum ſæpe inventum fuit viride. Jam paucis reſtat dicendum unde colori ſalivæ viridi ſucceſſit color flavus, quod inde factum arbitror, quia poſt acidum a ſanguine partim ablegatum, partim & a motu ſanguinis, ob cardiaca exhibita, aucto deſtructum, ſuperfuit ſolum ſuperflua bilis ſanguine expellenda, quod & brevi ſeceſſit, reſeratis bilis poris, & per patulos ſalivæ ductus mirandum in modum excreta.

VI. *Part*

VI. *Part of a Letter from Mr.* Thomas Robie, *Phyſician in* New-England, *to the Reverend Mr.* Derham, *F. R. S. Concerning the Effects of Inoculation; The Eclipſe of the Sun in* November 1722; *And the Venom of Spiders.*

Salem, Tueſday June 4.
1723.

WE don't as yet ſee any ill Effects of Inoculation, but the Inoculated are as yet as well, and ſome of them a great deal better than ever; as for the ill Conſequences that have been in *England*, I can't tell what they may ariſe from, but I can hardly think they are the genuine Effects of Inoculation, but may ariſe from ſome previous Diſpoſition to ſuch Diſtempers as you mention, or for want of due Evacuations after Inoculation, and too ſoon healing the Places of Inciſion; but I dare not pretend to ſay any more, and ſo leave it to the Opinion of better Judges; but only aſſure you I don't know, as yet of any ſuch ill Effects in this Country.

As to the great Eclipſe of the Sun in *November* laſt, I will give you my Obſervations thereof, when I have told you, that at *Yale* College, in *Connecticut* Colony, I have an Account that their Obſervers ſuppoſed it to be but about 8 Digits, when by my Calculation for *Cambridge*, it was to have been above 11. *Yale* College lies about 8′ or 10′ Weſt from *Camb.* in Lat. about 41° $\frac{1}{4}$ North. I never calculated it for that place, but I did for *London*, and it agrees to about

7' or 8' with your Obſervations, and pretty exact as ~~to the Digits.~~ The Gentleman that writes me word of this, tells me he calculated it and could make it ſcarce 7 *dig.* eclipſed, and that it paſſed off from *America* into the Sea, a great deal more ſouthward than I made it, for by my Calculation it was to be central at Cape *Cod*, and ſo to paſs over to the Iſles of Cape *de Verd.* But taking him to be very much miſtaken, I proceed to give you my Obſervations, as far as I was able to make them ; and I think they were well and truly made.

	ſt.	*h.*	'	
Nov. 27. 1722.	1	7	27	morn. I ſaw the Sun riſe Eclipſed, on its ſupreme Vertex to the South, about 4 *Dig.* tho' ſome on the Top of the new College ſaw it 2' or 3' before. The Sun's true riſing this Morn. was 7 *h.* 30', hence the Refraction is about 6' and ſo I have often obſerved it. From this time, till about 8 *h.* 30' or 40' I ſaw no more of the Sun, but then I judge it was eclips'd 6 *dig.* or more.

d.	*h.*	'	''	
2	8	55	15	The Sun was eclipſed 4 ¼ *Dig.* neareſt.
3	9	00	16	4 *Dig.* ½.
4	9	19	45	A little Spot in the Sun emerged.
5	9	25	45	I ſaw the Moon go off the Sun.
	9	25	45	Mr. *Danforth* in a Room juſt by me ſaw the Shadow

go

h.	′	″	
			go off the Paper about 30° from its lower Vertex to the Eaſt.
9	25	20	Mr. *Appleton* ſaw the Shadow go off the Paper fix'd to the College Braſs Quadrant at his Houſe.
			Mr. *Owen Harris*, an ingenious Schoolmaſter in *Boſton*, ſays he obſerved the End at about 26′ *p*. 9.

By the ſecond Obſervation the Sun's Diameter was to the Moon's As 1000 to 972; by the third, As 1000 to 975. At *Boſton* the Eclipſe was obſerved, allowing for its Diſtance, as I obſerved it at the College. And at *Barnſtable*, on Cape *Cod*, there was but a little left of the Sun, and nearer the Head of the Cape there was a Ring of Light quite round the Moon.

The Teleſcope I made my Obſervations by is 24 Feet long. The Teleſcope that Mr. *Danforth* uſed, thro' which the Rays were tranſmitted, was 8 Feet, and the Braſs Quadrant the very ſame Dr. *Halley* uſed at *St. Helena*. If I have been guilty of any Miſtake pardon me, and if, with eaſe, you could tell me where the Shadow would paſs off *America*, I ſhould be glad, for I made it to be about Cape *Cod*. Taking its Latitude to be 40° North, or 40° 10′ and Eaſt from the College 10′ or 15′ I forget which.

I ſhall now give you an Account of a remarkable Accident relating to the Venom of Spiders. *Sept*. 13. 1722 one *Nat. Ware* of *Needham* was bit by a ſmall Spider, which he could not give an exact Deſcription of, cruſhing it to Pieces between his Stocking and Leg; the Account he gave is this; *viz.* That getting up early in the Morning, and putting on his Stocking he preſently felt ſomething bite his left Leg a little above his Ancle, about ¼ an Hour after he felt a Pain in that Leg, and about ½ an hour from his firſt perceiving Pain in his Leg, he felt a Pain in his Groin, and at the ſame Time a creeping Pain in the Calf of his left Leg; and about one Hour after it got into the Small of his Back, and then round him, and in his Stomach, and in his right Thigh, and afterwards Numbneſs in his Head; the Pains were not conſtant and fixed, but erratick and very acute. His Pulſe was very low and heavy. He came to *Camb.* to a Phyſician there, and I was alſo deſired to go and ſee him, which I did, and he gave me this Account.

Sept. 14. In the Morning the Man aboveſaid came to ſee me, and was much better tho' he had but little Sleep in the Night. The Means the Doctor uſed were only *Sp. Car. Cerv. & Sal Vol. Corn. Cerv.* with *Vinum Viperin.* and Onions or Garlick externally applied to the Place where the Wound was. Theſe things raiſed his Pulſe, and ſo, I ſuppoſe, aſſiſted Nature to throw off the Venom.

VII. *Obſervations made in* Italy *of a Lunar Eclipſe, which happened the 8th of* Sept. 1718. *Extracted from the* Giornale di Literati *of* Venice.

I. O*Bſervations made by Signor* Giovanni Poleni *and* Giovambatiſta Morgagni, *at* Padua, *in the Houſe of Signor* Pietro Bembo, *Noble of* Venice.

Tempus Apparens poſt Meridiem.

H.	′	″	
			Sub initium Eclipſis nubes lunam obtexere.
6	54	25	Umbra appellit ad partem ortivam maris humorum; diſtat ab Ariſtarcho diametro ejuſdem maculæ, parique intervallo a Keplero.
7	5	5	Appellit umbra ad Copernicum.
	12	56	Umbra appellit ad Tychonem.
	18	10	Appellit ad Platonem.
	22	31	Tegit Manilium totum.
	30	55	Tegere incipit partem ortivam maris nectaris.
	41	53	Appellit ſecundam partem ortivam ſuperiorem ad mare Criſium.
	46	58	Penumbra extremum diſci attingit.
	49	4	Vix quidqam immerſionis ſupereſt.
			Toto integræ immerſionis tempore luna videri potuit commixta colore quodam obſcuro & ſubrubro. Principio poſt immerſionem lunæ pars orientem verſus erat obſcurior.

H.	′	″	
8	33	3	Obſcurior erat lunæ pars prope diſci medium: minus obſcuræ erant circumquaque partes diſci extremæ.
9	30	49	Stellula quædam, nudis oculis inconſpicua, vix decem ſecundis diſtare videbatur a lunæ diſco e regione *Lansbergii.*
	32	9	Penumbra fit clara in diſci extrema parte ortiva.
	36	4	Initium emerſionis ex ortiva plaga.
	40	39	Grimaldus jam emerſit ab umbra a qua diſtat ſui ipſius minori ſpatio.
	44	38	Stellulæ ante viſæ a luna occultatio: tamen incerta.
	49	34	Gaſſendus emergit.
	50	49	Mare humorum extra umbram totum.
10	00	3	Copernici emerſio.
	5	55	Plato emergere incipit.
	14	41	Eudoxus exit.
	19	12	Menelai emerſio.
	27	7	Mare nectaris totum emerſit.
	36	28	Umbra dividit mare criſium bifariam ſecundum ipſius majorem diametrum.
	39	12	Incipit umbra fieri tantillum rarior.
	41	2	Viſus eſt eſſe umbræ finis.
	42	57	Et finis penumbræ.

II. *Obſervations made in the Palace of the* Iſtituto delle Scienze, *at* Bologna; *by Signor* Geminiano Rondelli, Giuſeppe-Antonio Nadio, *and* Giulio-Cæſare Pariſi.

Temp. ver. p. m.

H.	′	″	
			Initium eclipſis non eſt obſervatum.
6	51	36	Mare humorum ad umbram.
	56	22	Capuanus ad umbram.

Mare

H.	′	″	
6	56	37	Mare humorum totum in umbra.
7	1	7	Bullialdus ad umbram.
	2	52	Bullialdus totus ſub umbra.
	3	37	Copernicus totus ſub umbra.
	11	22	Tycho ad umbram.
	12	52	Totus Tycho ſub umbra.
	15	37	Plato ad umbram.
	16	27	Totus Plato ſub umbra.
	19	22	Manilius ad umbram.
	19	52	Mare ſerenitatis.
	23	57	Mare tranquillitatis.
	35	8	Meſſalla ad umbram.
	36	8	Totus Meſſalla ſub umbra.
	36	38	Mare fœcunditatis ad umbram.
	37	23	Promontorium ſomni.
	39	23	Cleomedes ad umbram.
	39	53	Mare criſium.
	44	8	Mare fœcunditatis totum.
	44	43	Mare criſium totum.
	47	18	Totalis obſcuratio lunæ, juxta D. *Nadii* æſtimationem.
	47	53	Totalis obſcuratio, juxta D. *Pariſium.*
9	33	40	Initium emerſionis lunæ.
	36	35	Grimaldus totus extra umbram.
	40	54	Galilæus extra umbram.
	42	34	Sidus quoddam a luna tegitur in eodem proxime verticali cum centro lunæ.
	47	50	Mare humorum extra umbram.
	52	10	Bullialdus extra umbram.
	54	25	Centrum Tychonis extra umbram.
	55	12	Tycho totus extra umbram.
	58	46	Mare nubium extra umbram.
10	4	2	Plato ad umbræ terminum.
	5	33	Totus Plato extra umbram.

H.	'	''	
10	17	12	Inſula ſinus medii extra umbram.
	23	47	Meſſalla ad terminum umbræ; ſimul totum mare ſerenitatis extra umbram.
	27	58	Mare tranquillitatis extra umbram.
	30	12	Cleomedes extra umbram.
	32	8	Mare criſium umbræ terminum attingit.
	34	7	Mare fœcunditatis totum extra umbram.
	36	19	Mare criſium totum extra umbram.
	37	36	Finis eclipſis.

III. *Obſervations made in the Suburbs of* Bologna *ſouthwards*; *by Signori* Euſtachio *and* Gabbrielo Manfredi.

H.	'	''	
6	31	48	Nunc primum luna e collibus aſſurgere incipit, penumbra atmoſpheræ jam infecta.
	42	13	Initium veræ eclipſis, quantum judicare patiebatur ſubdubius umbræ terminus. Paulo pòſt nubecularum atque arborum objectu lunæ tegebatur.
	52	48	Umbra per Ariſtarchum & Keplerum protenditur, atque una mare humorum tangere videtur.
7	2	23	Umbra per medium Bullialdi, ſimul tangens Copernicum.
	4	2	Umbra per medium Copernici.
	5	4	Totus Copernicus latet.
	7	58	Umbra Pitatum attingit.
	10	54	Attingit Tychonem.
	12	19	Medium Tychonis latet.
	13	9	Totus Tycho latet.
	15	34	Umbra ad Platonem.
	16	7	Ad medium Platonis.

H.	′	″	
7	16	54	Totus Plato latet.
	20	9	Manilius tegitur.
	20	34	Umbra tangit mare ſerenitatis.
	23	44	Menelaus tegitur.
	24	36	Dionyſius tegitur.
	27	34	Plinius tegitur.
	29	49	Umbra ad Catharinam, Theophilum, Cyrillum.
	30	36	Umbra tangit Fracaſtorium.
	31	44	Medium Fracoſtorii tegitur.
	32	34	Promontorium acutum umbram ſubit.
	35	15	Promontorium ſomni latet.
	37	57	Taruntius latet.
	39	39	Umbra tangit mare criſium.
	42	16	Umbra per medium mare criſium.
	44	5	Totum mare criſium in umbra conditur.
	47	50	Totalis immerſio lunæ in umbram.
			Toto tempore eclipſis luna clariſſimè in ſudo ſpectabatur, colore rubeſcenti, ea parte denſiori, qua altius in umbram immergebatur.
8	38	50	Hoc tempore, & deinceps aliquot minutis, omni ex parte æque obſcura apparebat lunæ facies, ut facile conſtaret eam prope umbræ centrum verſari.
9	27	50	E regione Grimaldi, qua parte emerſio imminebat, inſignis fulgor ſpectari cœperat.
	29	20	Dubitari cœptum de emerſionis initio.
	33	20	Proculdubio emerſio jam inceperat.
	35	21	Grimaldus ab umbra ſe ſubducere incipit.
	35	55	Centrum Grimaldi emergit, totus Ricciolus jam detectus erat.
	36	26	Totus Grimaldus exit ab umbra.

H.	′	″	
9	39	28	Galilæus exit.
	41	22	Umbra tangit mare humorum.
	42	31	Stellula quæ diu prope limbum lunæ inferiorem (qui teleſcopio ſuperior apparebat) morata fuerat, nunc demum ſub lunam conditur, circa plagam Tychonis, adhuc eclipſi laborantem. Aliæ Stellulæ lunam ſubituræ videbantur, ſed poſtquam unus vel alter digitus lunaris faciei illuſtrari cœpit, præ fulgore omnes evaneſcebant.
	43	53	Umbra per medium mare humorum.
	45	41	Ariſtarchus emergit.
	47	23	Keplerus emergit.
	52	6	Emergit Bullialdus.
	53	31	Tycho detegi incipit.
	54	9	Medium Tychonis detegitur.
	55	1	Totus Tycho detectus: quo tempore nondum ſtellula e luna ſe ſubduxerat.
	55	21	Copernicus emergere incipit.
	56	6	Medium Copernici emergit.
	57	15	Totus Copernicus.
10	0	1	Stellula quæ paulo antea ſub luna delituerat, jam ſpectabatur a lunæ limbo nonnihil diſtans, ut appareret eam ante 4 vel 5 minuta temporis emerſiſſe. Verſabatur autem e regione partis obſcuræ inferioris limbi lunæ, neque longe ab umbræ termino.
	4	51	Umbra per medium Platonis.
	5	36	Totus Plato detegitur.
	13	6	Manilius emergit.
	16	31	Dionyſius emergit.
	16	41	Menelaus emergit.

H.	′	″	
10	20	31	Fracaſtorius totus jam emerſerat.
	23	51	Snellius & Furnerius totaliter emergunt.
	24	5	Promontorium acutum detegitur.
	25	11	Meſſalla totus apparet.
	31	11	Proclus emergit.
	31	51	Mare criſium emergere incipit.
	34	3	Medium mare criſium emergit.
	36	7	Totum mare criſium extra umbram.
	38	51	Circa hoc tempus umbra vera lunam deſerere videtur, penumbra adhuc ad multum temporis perdurante.

In the *Ephemerides* publiſhed in the Year 1715. from *M. Caſſini*'s Tables for the Uſe of the *Iſtituto Bologneſe delle Scienze*, the Beginning of theſe Eclipſes was marked at *6h.* 41′, the total Immerſion at *7h.* 46′, the Beginning of the Emerſion at *9h.* 33′, the End at 10*h.* 38′, which Times ſcarcely differ one or two Minutes from the Times obſerved.

IV. *Obſervations made by the Marquis* Antonio Ghiſilieri, *at* Bologna, *on the Obſervatory in his own Houſe.*

H.	′	″	
6	40	23	Initium eclipſis dubium.
	51	23	Mare humorum ad umbram.
	55	46	Capuanus ad umbram.
7	1	13	Bullialdus ad umbram.
	28	14	Mare nectaris totum ſub umbra.
	32	30	Promontorium acutum ad umbram.
	36	45	Promontorium ſomni ad umbram.
	38	45	Mare criſium ad umbram.
	46	37	Totalis immerſio lunæ.
9	33	50	Initium emerſionis.

H.	′	″	
9	35	39	Grimaldus totus extra umbram.
	54	17	Tycho totus extra umbram.
10	15	6	Plinius totus extra umbram.
	32	39	Mare criſium emergere incipit.
	37	42	Finis eclipſis.

VIII. *Experimenta & Obſervationes de Congelatione aquæ in vacuo factæ* a D. G. Fahrenheit, R. S. S.

INter plurima admiranda Naturæ Phœnomena aquarum congelationem non minoris momenti eſſe ſemper judicavi; hinc ſæpe experiundi cupidus fui, quinam effectus frigoris futuri eſſent, ſi aqua in ſpatio ab aere vacuo clauderetur. Et quoniam dies ſecundus, tertius & quartus *Martii*, (Styli V.) Anni 1721. ejuſmodi experimentis favebat, hinc ſequentes obſervationes & experimenta a me ſunt factæ.

Antequam autem experimentorum recenſionem aggrediar, neceſſe erit, ut paucis quædam de thermometris, quæ a me conſtruuntur, eorumque ſcalæ diviſione, ut & de methodo evacuandi, qua uſus ſum, mentionem faciam. Duo potiſſimum genera thermometrorum a me conficiuntur, quorum unum ſpiritu vini & alterum argento vivo eſt repletum: Longitudo eorum varia eſt, pro uſu, cui inſervire debent: Omnia autem in eo conveniunt, quod in omnibus ſcalæ gradibus concordent, interque limites fixos variationes ſuas abſolvant. Thermometrorum ſcala, quæ meteorologicis obſervationibus ſolummodo inſerviunt, infra a Zero incipit & 96to gradu finitur. Hujus ſcalæ diviſio tribus nititur terminis fixis, qui arte ſequenti modo parari poſſunt; primus illorum in infima parte vel initio ſcalæ reperitur, & commixtione glaciei, aquæ, & ſalis Armoniaci vel etiam maritimi acquiritur; huic mixturæ ſi thermometron imponitur, fluidum ejus uſque ad gradum, qui zero notatur, deſcendit. Melius autem hyeme, quam æſtate hoc experimentum ſuccedit. Secundus terminus obtinetur, ſi aqua & glacies abſque memoratis ſalibus commiſcentur, impoſito thermometro huic mixturæ, fluidum ejus triceſimum ſecundum occupat gradum, & terminus initii congelationis a me vocatur; aquæ enim ſtagnantes tenuiſſima jam glacie obducuntur, quando hyeme liquor thermometri hunce gradum attingit. Terminus tertius in nonageſimo ſexto gradu reperitur; & ſpiritus uſque ad hunc gradum dilatatur, dum thermometrum in ore vel ſub axillis hominis in ſtatu ſano viventis tam diu tenetur donec perfectiſſime calorem corporis acquiſivit. Si vero calor hominis febri vel alio morbo fervente laborantis inveſtigandus eſt, alio thermometro utendum, cujus ſcala uſque ad 128 vel 132 graduum prolongata eſt. An autem hi gradus ferventiſſimo calori alicujus febris ſufficiant nondum expertus ſum, vix tamen credendum, quod cujuſdam febris fervor gradus memoratos excedere debeat. Thermometrorum ſcala, quorum ope ebullientium liquorum gradus caloris inveſtigatur, etiam a zero incipit & 600 continet gradus, hoc enim circiter gradu Mercurius ipſe (quo thermometron repletum eſt) incipit ebullire.

Ut autem quoque thermometra ab omnibus mutationibus caloris celeriter afficiantur, loco globulorum cylindris vitreis ſunt prædita, eo enim modo ob majoris ſuperficiei quantitatem citius a variatione caloris penetrantur.

Poſtquam breviter mentionem feci de conſtructione thermometrorum meorum, adhuc deſcribendus erit modus evacuandi, quo in experimentis initio memoratis uſus ſum. Globulus vitreus (*Fig.* 6.) A tubulo BC duorum vel trium pollicum longo in extremitate C attenuato præditus ſupra ignem caleſit, quo facto tubuli extremitas aquæ immergitur, & tam diu in aqua relinquitur donec refrigefactione aëris in globulo contenti, guttulis aliquibus aquæ ſit repletus, deinde iterum ſupra flammam latiorem lampadis cujuſdam, vel ſupra prunas ope forcipis parvæ tenetur, donec aqua in globo contenta incipit ebullire, & vapor aquæ impetu inſtar Æolipilæ erumpit: Hæc ebullitio aquæ aliquantulum continuatur, quo facto globulus ab igne removetur, & extremitati ejus flamma candelæ appropinquatur. Refrigereſcente globulo vapor ab igne rarefactus etiam ſucceſſive condenſatur, vaporumque egreſſus paulatim diminuitur, qui poſtquam plene ceſſavit, in ipſo etiam momento extremitas tubuli colliqueſcit, globuluſque hermetice ſigillatus & ab aere evacuatus redditur. An vero hoc modo probe ab aere evacuatus ſit, experire poteſt, ſi nempe extremitas tubuli ſub Mercurio diffringitur, totus enim globulus Mercurio replebitur, ſi diffractio caute ſine introitu aeris externi fuerit peracta. Diffractio extremitatis etiam ſub aqua perfici poteſt, ſed licet ſummâ curâ peragatur, globus tamen non tam perfecte aqua replebitur; dum enim aqua globum evacuatum intrat, aer, qui ſemper in aliqua quantitate aquæ commixtus eſt, ab illa in minutiſſimis bullulis ſeparatur, quæ poſtquam coiverunt ſub ſpecie bullulæ majoris in globulo apparent. Eodem modo globus ab aere evacuari poteſt, ſi tertia, dimidia vel major pars globi aqua repleta deſideratur; deſiderata enim quantitate aquæ prius impletur & deinde poſt ebullitionem aquæ hermetice clauditur. His explicatis ad recenſionem experimentorum pervenio.

Globulum vitreum, cujus diameter uni circiter pollici æquabat, memorato modo ab aëre evacuatum, & aqua pluviatili fere ad dimidiam partem repletum, die ſecundo *Martii* Anni 1721, frigoris rigori exponebam. Aeris temperies in thermometro appoſito quindecimo gradu notabatur. Elapſo horæ ſpatio, aquam adhuc fluidam in globulo reperiebam, cujus rei cauſam eſſe arbitrabar, quod nempe aqua nondum bene a frigore eſſet penetrata, ut autem dubii omnis tolleretur ſcrupulus, globum per totam noctem aëri expoſitum relinquebam. Sequenti die tertio *Martii* ad horam matutinam quintam aquam adhuc fluidam inveniebam & liquor thermometri eundem adhuc notabat gradum, cujus improviſi phænomeni cauſam aëris abſentiæ attribuebam. Ut autem hujus conjecturæ veritas mihi innoteſceret, diffringebam tubuli extremitatem, ut ſpatium vacuum globuli iterum aere repleretur, quo facto tota aquæ maſſa celerrime tenuiſſimis glaciei lamellis permiſcebatur. Placebat mihi autem antequam experimenti repetitio fieret, alio experimento explorare, an hæ lamellæ glaciales aquæ innataturæ eſſent, quapropter globum diffringebam partemque aliquam glaciei aquæ vitreo poculo contentæ injiciebam, & illam aquæ innatare conſpiciebam.

Dum autem forte oculos admodum brevi temporis intervallo in alium quendam direxeram locum, aſpiciendo iterum poculum, totam aquam glacialibus lamellis permixtam cernebam, manente tamen adhuc in interſtitiis lamellarum plurimâ aquæ parte fluidâ. Thermometron huic mixturæ impoſitum, trigeſimum ſecundum notabat gradum. Attentiori autem animo & oculo hæc phænomena contemplari cupidus, experimentum duobus aliis globulis repetere reſolvebam:

poſtquam igitur priori modo præparati erant, illos per horæ ſpatium aëri externo exponebam, liquor autem thermometri interea jam vigeſimum attigerat gradum. Elapſa hora aquam in ambobus globulis adhuc fluidam inveniebam, poſtquam autem ſpatium vacuum globuli aëre iterum repletum erat, citiſſime etiam aqua (ut in priori experimento) lamellis glacialibus permiſcebatur, illarumque generatio tam ſubitanea erat ut vix oculis aſſequi poterat. Et quoniam lamellarum generatio, quæ in poculo vitreo erat facta, obſervationem meam effugerat, hinc adhuc maxime curioſus eram, illarum generationem paulo attentius contemplari. Priusquam autem globulorum alterum diffringebam, aquam memorato poculo contentam a lamellis glacialibus ſeparabam quo facto globulum diffringebam, glaciem in globo generatam aquæ injiciebam. Glacies injecta quidem aquæ innatabat, ſed lamellarum generatio in poculo fruſtra a me expectabatur. Ob negotiorum quorundam neceſſitatem, experimentorum continuationem advenienti dedicabam nocti. Quæ poſtquam advenerat, iterum hora undecima tres globulos ſævienti gelu exponebam. Horum duo ad dimidiam circiter partem, iterum aqua erant repleti, manente reſidua parte globulorum vacua, in tertio vero ſolummodo quarta circiter pars globuli erat vacua. Aeris temperies in thermometro appoſito vigeſimo ſexto notabatur gradu. Hora quarta matutina eundem ſtatum temperiei aëri adeſſe thermometro deprehendebam, & aquam in duobus globulis, qui tantum pro dimidia parte aqua modo erant repleti, adhuc fluidam inveniebam: in tertio autem aqua congelata, atque globus diffractus erat. Glacies minutiſſimis ſed admodum paucis permixta erat bullulis, pelluciditaſque ejus maxime perturbata apparebat & confuſæ cryſtallizationi alicujus ſalis ſimillima erat. Hujus experimenti contrarium ſucceſſum inviſibili cuidam fiſſuræ attribuebam, quâ aër externus introitum invenerat atque ita congelationem aquæ procreaverat.

Quoniam autem magno adhuc flagrabar deſiderio, lamellarum generationem in poculo vitreo attente contemplari, idcirco vas vitreum e cubiculo in illam afferebam cameram ubi hæc experimenta fiebant, dum autem ſcalas paucas, quæ ad illam ducebant cameram, aſcendere volebam, deficiebam ſcalam aliquam pede bene attingere, quo facto aqua vitro contenta ruditer commovebatur, atque eo ipſo momento tota ejus maſſa plurimis permixta apparebat glacialibus lamellis. Hoc autem caſu infortuito edocebar, glaciem in aqua ſatis frigida agitatione produci poſſe; curioſus inde eram experimento explorare, an congelatio aquæ etiam in ſpatio vacuo agitatione futura eſſet. Poſtquam igitur globulum aliquantum agitaveram, magna animi voluptate eundem phœnomeni eventum cernebam, ſimulque judicii errorem agnoſcebam, quod nempe abſentiæ aeris fluiditatem aquæ attribuiſſem. Interea e thermometro agnoſcebam, gelu multum langueſcere, liquor enim jam ad vigeſimum octavum aſcenderat gradum, cito igitur manu diſſolvebam glaciem, globulumque unum iterum aëri exponebam (erat autem alter caſu infortuito diffractus). Relicto globo per dimidium circiter horæ ſpatium, gelu adhuc magis remittere obſervabam, thermometri enim liquor jam ad gradum trigeſimum ſecundum pervenerat. Et quoniam verebar, ne remiſſione frigoris experimenti repetitio vana futura eſſet, ſi diutius globulus aëri relinqueretur expoſitus: hinc ipſo tempore, agitatione globuli aquæ congelationem procreare tentabam; ſed licet fortiter agitaretur, non minima tamen congelationis apparebant indicia. Cum vero hoc modo omnis congelationis ſpes evanuerat, adhuc experiri volebam, an congelatio ſucceſſura eſſet, ſi ſpatium vacuum globuli iterum aëre repleretur. Diffracta igitur extremitate tubuli, minutiſſimæ glaciei ſpiculæ per totam aquæ maſſam diffuſæ generabantur, quæ circumrotatione aquæ ſuperficiem petebant, amœniſſimumque ſpectaculum reflectione luminis ab earum politis ſuperficiebus præbebant. Quoniam autem hujus hyemis gelu hoc die finiebatur & cum illo experimentorum continuationi finis imponebatur, quam vero tempori opportuniori aliis excogitatis experimentis inſtituere mihi proponebam. Hyems anni 1722. ita mitis in *Hollandia* erat, ut per totam hyemem vix aquæ ſtagnantes glacie aliqua obducebantur. Et licet hyems initio anni 1723. multo ſeverior erat, attamen negotiorum copia, aliorumque experimentorum major neceſſitas continuationem eorum prohibebant. Cogitationes aliquas de horum phænomenon cauſis quidem apponerem, ſed ob experimentorum inſufficientem copiam illas relinquo, ſufficietque mihi experimenta & obſervationes memoratas recenſuiſſe, an forſan viri perſpicaciſſimo ingenio præditi, illas contemplationibus ſuis dignari vellent.

FINIS.

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ERRATA.

PAG. 46. lin. 17, *read* uninterrupted.
Pag. 60. lin. 11. *read* hypochondrio.

Numb. 383

PHILOSOPHICAL TRANSACTIONS.

For the Months of *May* and *June*, 1724.

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I. *Part of a Letter from the Reverend Mr.* Waſſe, *Rector of* Aynho *in* Northamptonſhire, *to Dr.* Mead, *concerning the difference in the Height of a Human Body, between Morning and Night.*

I Have obſerv'd ſeveral Soldiers diſcharged for being a little under the Standard, and having experienced the difference of a Man's Height in the Morning and at Night, I mentioned it to an Officer, and thereby kept ſome Perſons from being turn'd out of the Service. Since that Time I have meaſured Sir *H. A* . . . . , Mr. *C* . . . . . , and a great many ſedentary People, and Day-Labourers, of all Ages and Shapes, and find that Difference to be near an Inch. I try'd my ſelf, when ſitting, and found it in like manner ; particularly, *Auguſt* 21. 1723. Weather warm, no Wind, at Eleven in the Morning I ſat down, and fixed an Iron Pin ſo as to touch it, and that but barely. After that, I fatigued my ſelf for half an Hour with a Garden-Roller, and the Conſequence was, that at 12 Ho. 30 Min. I could not reach the Nail ſitting, by about 5 Tenths of an Inch, or the Breadth of one of the Rods of *Hunt*'s Sliding-Rule. At 2 the ſame Day I wanted near ſix Tenths of an Inch. On the 21ſt, at 6 Ho. 30 Min. in the Morning, I touched the Nail fully ; and after the above-mentioned Exerciſe for only a Quarter of an Hour, at 7 Ho. 14 Min. I fell ſhort almoſt as much as before. On the 27th, having ſat up late with ſome Friends, I was faint, and felt my ſelf heavy upon the Ground, and without any Spring, and at 9 that Morning I did not reach the Nail, though I had us'd no

Exerciſe. I rid out, but could not reach it that Day. On the 28th I rid about four Miles; and whereas at 6 that Morning I reached the Nail, I had loſt 6 tenths of an Inch by 8. *September* 19th I came from *Oxford* a little tired, and next Morning at 8 I wanted half an Inch. If I Study cloſely, though I never ſtir from my Writing-Desk, yet in 5 or 6 Hours I loſe near an Inch. All the Difference I find between Labourers and ſedentary People is, that the former are longer in loſing their Morning Height, and ſink rather leſs in the whole than the latter. I cannot perceive, that when the Height is loſt, it can be regained by any Reſt that Day, or by the Uſe of the cold Bath.

I have not informed my ſelf ſo well about Horſes; my Mare comes Seven this Graſs, is ſtrong, ſhort back'd, and well legg'd. I meaſured her before and after riding 20 Miles, *April* 30th, and could not perceive the leaſt Difference in her Height.

The Alteration in the human Stature, I imagine, proceeds from the yielding of the Cartilages between the *Vertebræ*, to the Weight of the Body in an erect Poſture.

AYNHO, *May* 16, 1724.

This curious Obſervation has been confirmed by ſeveral Members of the Royal Society, and others, who have purpoſely tried the Experiment.

II. *Some Remarks upon the Obſervation mentioned in the foregoing Letter. By Mr.* William Beckett, *Surgeon, F. R. S.*

THE remarkable Difference in the Stature of human Bodies, in the Space of a few Hours time, taken Notice of in the Reverend Mr. *Waſſe*'s Letter, read before the Society this Day Sevennight, I have ſince found to be Fact, by ſeveral Experiments made with the greateſt exactneſs I have been capable of. Only this I have farther obſerved, that in thoſe Perſons who have been young, the Alteration has been more conſiderable than in thoſe that have been aged. The Trials equally ſucceeding in a ſitting as in a ſtanding Poſture, will naturally lead us to believe, that it muſt neceſſarily be from the Trunk of the Body, or ſome of its Parts, that this remarkable Alteration is brought about. Now every Body knows, that the Standard of a Perſon's Stature has been always looked upon to be determined by the whole *Compages* of the Bones, adjuſted by the Divine Architect according to the ſtricteſt Rules of Geometry.

But there is ſomething ſo wonderful in the Structure and Diſpoſition of the Spine, that nothing but ſuch a peculiar Contrivance could ſo curiouſly have fitted it for the reſpective Uſes and Purpoſes it was ordained for The thickneſs and ſhortneſs of the Bones, with the intervening Cartilages, aſſiſted by the bony Proceſſes, diſpoſe it to a Motion peculiar to it ſelf. Whereas had the Bones been of any conſiderable length, upon bending the Body, the Articulations muſt have made a large Angle upon their inmoſt Edges, and the ſpinal Marrow have been continually liable to be injured; or had the Cartilages been entirely wanting, it would have been as uſeleſs as if it were but one Bone, whereby we being rendered uncapable of bending the Trunk of the Body, it muſt have always remained in an erect Poſture. But by the preſent Diſpoſition of its Parts, 'tis not only abſolutely ſecured againſt any ſuch Inconveniencies, but, although ſo ſmall a Pillar as it is, is capable of ſupporting, without Hazard, ſuch prodigious Weights, as we are not wanting in our Accounts of.

Another Particular, which beſpeaks the utmoſt Wiſdom and Deſign in the Contrivance of this Part, is the remarkable Difference there is in the thickneſs of the Cartilages, placed betwixt the Bones of the Spine; the *Vertebræ* of the Breaſt requiring but little Motion, the Cartilages are there but thin, in compariſon of thoſe of the Loins, which being very thick, the loweſt more eſpecially, the Motion is there vaſtly greater; and the Cartilages being abundantly thicker before than behind, this is the Reaſon that we bend our Bodies ſo much more forward than backward. And by this admirable Method of diſpoſing of the thicker Parts of the Cartilages forward, it is, that in all violent Exerciſes, the Parts contained in the Belly and Breaſt are in a great meaſure ſecured from any Damages they might have been liable to, becauſe by the pliableneſs and elaſticity of theſe Cartilages, they break the violent Shocks the *Viſcera* muſt otherwiſe have neceſſarily ſuſtain'd upon ſuch Occaſions.

From what I have here remarked, in relation to theſe peculiar Properties of the Cartilages placed between the Bones of the Spine, we may reaſonably ſuppoſe them to be certain compreſſible, dilatable, elaſtick Bodies, which like other Bodies, endued with the ſame Qualities, will naturally yield to any incumbent Weight, which is ſufficient to force the Particles of Matter of which they conſiſt, into a more ſtrict and cloſe Union, and that when this compreſſive Power is removed, they will of themſelves recover that State they enjoyed before they were obliged to give Way to that Preſſure. Now I am here particularly to obſerve, that the loweſt of all the Cartilages of the Loins, is the thickeſt, and ſo conſequently that it contains a greater Quantity of Matter than any of the reſt; by which means it becomes more diſpoſed to have its thickneſs diminiſhed, and that all of them gradually become thinner, even to the top of the Spine. Now all ſuperiour Bodies, if they come to an immediate Contact, preſſing upon their inferiour, it muſt neceſſarily follow, that the whole Weight of the Body, except the lower Limbs, muſt preſs upon and be ſuſtained by the loweſt *Vertebræ* and their Cartilages; but theſe Cartilages, as has been obſerved, being much thicker in this Part than the other, and the incumbent Weight bearing harder upon them, they muſt be unavoidably compreſs'd more than the other; and ſo, conſequently, when this Weight is removed, their Expanſion, from their natural Elaſticity, will be greater alſo.

This being the natural State and Diſpoſition of theſe Parts, during the whole ſpace of Time we are uſually employed about our neceſſary Avocations till we diſpoſe our ſelves to Reſt, the Cartilages of the Spine will, by their compreſſible and yielding Properties, become more cloſe and compact from the perpendicular Preſſure they ſuſtain, and ſo conſequently the Spine, the only ſupport of the Trunk of the Body, will become ſhorter. But when this ſuperiour Weight ſhall be entirely removed, by placing the Body in a horizontal Poſture, as it always is when we are in Bed, the compreſs'd Cartilages will, by their natural elaſtick Power, begin gradually to enlarge themſelves, till they recover the ſame expanded State they enjoyed before they were forced to give Way, and yield to the incumbent Preſſure; and ſo conſequently it will produce a conſiderable Alteration in a Perſon's Stature, agreeable to the determined Times mentioned in the preceding Letter. For if we only conſider, that the before-mentioned compreſſive Power will leſſen the thickneſs of all the Cartilages, in proportion to the Quantity of Matter they contain; and that there are uſually reckoned about twenty four in Number, it will be no difficult Matter for us to apprehend, that their natural Expanſion being recovered by our cuſtomary Repoſe, the Aggregate of the whole of the Expanſions may amount to about an Inch. Now if the Alteration be ſo conſiderable as this, occaſioned only by the bare incumbent Weight of the ſuperiour Parts of the Body, without any additional Force applied to compreſs the Cartilages yet cloſer, how much more may we reaſonably imagine it would be, were the Experiment tried on ſuch Perſons, whoſe uſual Employment it is to carry heavy Burthens. I have only this one Particular farther to obſerve upon this Head, which is, that this Alteration is not to be expected to be the ſame in aged Perſons as in thoſe that are younger, becauſe the Cartilages, as we advance in Years, gradually grow harder and harder, till many of them arrive to the ſolidity of a Bone; that is, by Degrees they loſe their Spring or expanſive Power, and at length continue in a compreſs'd State of Reſt. And this is without doubt one principal Cauſe, why old People not only ſeem to have loſt ſomewhat of their former Height, but are actually ſhorter.

III. *A Catalogue of Fifty Plants from* Chelſea *Garden, preſented to the Royal Society for the Year* 1723, *by the Company of Apothecaries of* London, *purſuant to the Direction of Sir* Hans Sloane, *Bart. Preſident of the College of Phyſicians.*

51. A *Butilon Carolinianum, reptans, Alceæ foliis, gilvo flore.*
52. *Ambroſia, maritima, Artemiſiæ foliis inodoris, elatior.* H. L. Bat.
53. *Alyſſon Veronicæ folio.* T. Inſt. 217
Burſa Paſtoris major loculo oblongo. C. B. prod.
54. *Aſter peregrinus, Ciſti folio non crenato, flore magno, luteo.* Pluk. Phyt. T. 16. f. 2.
55. *Aſter montanus, Salicis glabro folio.* C. B. 266
56. *Bidens Indica, Hieracii folio, caule alato.* T. Inſt. 462.
57. *Blitum monoſpermum, Indicum, aculeatum,* Breyn. prod. 18.
58. *Blitum quod Amaranthus Græcus, ſylveſtris, anguſtifolius.* T. Cor. 17.
59. *Calcitrapoides pumila, ſupina, tenuifolia, calyce turbinato.* D. Vaillant. Act. Ac. Par.
60. *Caſſia decaphylla, Orobi Pannonici foliis mucronatis.*
61. *Cataria major Vulgaris.* T. Inſt. 202.
62. —— *anguſtifolia minor.* Ib.
63. —— *quæ Nepeta Pannonica major & elatior.* H. Ox. 3. 415.

64. —*Hiſ-*

64. *Cataria Hiſpanica, Betonicæ folio anguſtiori, flore albo.* T. Inſt. 202.

65. ———*Luſitanica, erecta, Betonicæ folio, tuberoſa radice.* T. Inſt.

66. ——— *quæ Horminum Spicatum, Lavendulæ flore & odore.* Boccon. rar. 48.

67. ———*Orientalis, Teucrij folio, Lavendulæ odore, Verticillis florum craſſiſſimis.* T. Cor. 13.

68. *Chryſanthemum tenuifolium, flore bullato aureo.* Barrel. Ic. 450.

69. ———*flore luteo, aphyllo minore.* Barrel. Icon. 452

70. *Cirſium humile, ramoſum, foliis integris.*

71. *Helianthemum Anglicum, vulgari ſimile, flore ſtellato.*

72 ———*ampliore folio, flore roſeo.* D. Sherard.

73 ———*Halimi folio, anguſtiore, acuto. An Ciſtus Halimi folio, 2.* Cluſ. Hiſt. 71.

74. *Hieracium Pyrenaicum Blattariæ folio, minus hirſutum.* Schol. Bot.

75 ———*Caſtorei odore noſtras. Hieracium Cichorei, vel potius Stæbes folio, hirſutum.* Raij Cat. Cantab.

76. *Lychnis Or. longifolia, nervoſa, flore purpuraſcente.* T. Cor. 24.

77. *Malva Sinenſis, annua, erecta flore minimo. Kitſaitſe dicta.*

78. ———*Luſitanica, annua, flore parvo ſtriato.*

79. ——— *Sylveſtris, foliis ſinuatis minoribus, floſculis minimis, Anglica.* Boerh. Ind. 2. 268.

80. ———*Or. erecta, major, flore vix conſpicuo.* D. Sherard.

81. ——— *Erectior, flore magno, ſuave rubente.* T. Cor. 2.

82. *Malva Syriaca, Ela Haur taunal dicta. Malva Cretica, annua, altiſſima, flore parvo, ad alas umbellato.* T. Cor. Ib.

83. *Mimoſa Jamaicenſis Zanon.* Hiſt 149.

84. *Rhagadiolus alter.* Cæſalp. 511.

85. ——— *Lampſanæ foliis.* T. Cor. 36

86. *Salvia Or. Abſinthium redolens, foliis pinnatis, flore carneo, elatior.* D. Sherard.

87. *Scabioſa annua, Centauroides fiſtuloſa.* H. Cath.

88. ——*Altiſſima, annua, foliis Agrimoniæ nonnihil ſimilibus* H. L. B. 539.

89. *Stachys Cretica.* C. B. 236.

90. —— ——*foliis ſub verticillis latioribus, acuminatis, flore majore.*

91. *Stachys major Germanica.* C. B. 236.

92. ——— *Folio obſcurè virente, flore ferrugineo.* H. Ox. 3. 382.

93. ———*Salviæ folio, flore luteo.* D. Juſſieu.

94. ———*Elatior flore flaveſcente punctato.*

95. *Vicia, Craccæ majori Germanicæ ſimilis, per omnia minor Anglica.*

96. *Vicia ſupina, latiſſimo folio, non ſerrato.* T. Inſt. 397.

97. —— —— —— —— *Serrato.* Ib.

98. *Sylveſtris, incana, major, & præcox, Pariſienſis, flore ſuave rubente.* T. Inſt. 397.

99. ———*Sylvatica maxima.* Phyt. Britan. 129.
———*Multiflora maxima.* Raij. Syn. 189.

100. ———*Multiflora, Caſſubica, fruteſcens, Lentis ſiliqua.* Breyn. Prod. 52.

IV. *An Account of Obſervations made of the Variation of the Horizontal Needle at* London, *in the latter Part of the Year* 1722, *and beginning of* 1723. *By Mr.* George Graham, *Watch-maker, F. R. S.*

THE Figure of the three Needles, with which the Experiments were made, was priſmatick; their Lengths were nearly 12,2 Inches; their Ends, which pointed to the Diviſions, being filed to an Edge, which made a fine Line perpendicular to the Horizon. The Caps of two were of Chryſtal, the other of Glaſs; they were well poliſhed on the Inſide, in that Part which touched the Pin they moved upon. The Box was Braſs, and of a Breadth ſufficient to admit of 20° on each Side the middle Line, and covered with a piece of ground Glaſs. The circular Arches at the Ends were raiſed ſo much above the Bottom of the Box, as to have their upper Surfaces, upon which the Diviſions were cut, lie in the ſame Plane with the Needle, and at ſuch a Diſtance from each other, that the Needle might play freely between them. A few of the Degrees at the North End were divided into ſix equal Parts, each Diviſion being 10′. It was eaſy, by the help of a Convex Glaſs, to determine the pointing of the Needle to leſs than a Quarter of theſe Diviſions, or to about 2′ of a Degree. The Pin, upon which the Needle moved, was of Steel hardned, and ground to a fine Point; and by a Spring placed in the Box, the Needle might be raiſed from off the Point, and let down again at Pleaſure, without removing the

Glaſs, or diſturbing the Box. By this means both the ſharpneſs of the Point, and poliſh of the Cap were better preſerved from injury, when there was occaſion to move the Box. A ſmall piece of Braſs was made to ſlide upon that End of the Needle which pointed to the South, for readily bringing it to an horizontal Poſition; for according to the different ſtrength of the Touch, the North End of the Needle will dip more or leſs. The bottom Plate of Braſs was a little broader and longer than the Box, and its Edges made Lines exactly parallel to the middle Line of the Diviſions; and for the greater ſecurity of placing the Box in a right Situation, there was a Braſs Ruler of thirty Inches long, having its Edges even and parallel, except part of that Edge which was applied to the Side of the Box which was a little filed away on the Middle, that the Side of the Box near its Ends only might touch the Ruler. By this Contrivance the two Points of Contact were as far aſunder as the length of the Box would admit of, and the other Edge of the Ruler making a longer Line than the Side of the Box, afforded a better Direction for giving it the ſame Situation.

For determining the Quantity of the Variation, I got a Meridian Line ſtretched upon the top of the Houſe, between the Rails of the Leads, which were above fifteen Foot aſunder, and the Line was a little more than thirty-nine Inches above the Leads. As this Line was faſtned to two Pieces of Braſs that were fixed in the Rails, and was above fifteen Foot long, no ſenſible Error could ariſe in putting it up at any Time. The Compaſs-Box was placed upon a Wooden Stool, with three Feet, that had nothing of Iron about it, and its Top ſet level by a Plumb-Rule. But finding

ing that in the open Air the Wind gave ſome Diſturbance, I put up another Line, after the ſame manner, in a Room two pair of Stairs high ; this Line was about the ſame length with the other, and thirty-nine Inches above the Floor. Some time after I put up a third Line, of the ſame Length, in the Room over this. By the Method made uſe of in fixing theſe Lines they could not differ above 2′ of a Degree from the Meridian, or from one another. Before I had made any Trials, I imagined no other Difference would ariſe than what might be occaſioned by the Friction of the Needle upon the Point it was to move upon, and having found that conſiderable in all the Needles that I had taken notice of, I took more than ordinary Care to provide againſt it, and ſucceeded beyond my Expectation. For I have ſeveral times obſerved all the three Needles return ſo exactly to the ſame Place, that I could not perceive the leaſt Difference ; as likewiſe all three to agree very nearly about the ſame Time, when they have been placed in the ſame Box immediately one after another, the Box remaining unmoved. The firſt Needle I made, was a little above three tenths of an Inch broad, about ,06 in thickneſs, and weighed about an Ounce Troy, the Cap of Chryſtal. After ſome Trials with this Needle, it was made narrower, not to exceed half a tenth of an Inch, and it then weighed five Peny Weight and five Grains. The ſecond Needle was at firſt about three tenths of an Inch broad, and ,04 thick, the Cap of Glaſs ; and after ſeveral Trials, it was made ſo much narrower, that its Breadth was a little leſs than its Thickneſs, and it weighed two Peny Weight and five Grains. The third was nearly of the ſame Dimenſions with the ſecond, and weighed two Peny Weight and three Grains.

Wh

When the two firſt Needles were made narrower, care was taken that the Files made uſe of for filing the North Ends, touched not the South Ends; and after they were made lighter, I tried them both, before they were freſh touched upon the Stone, and found no ſenſible Difference in their Direction. The reaſon of making the two firſt Needles ſo heavy, was to try whether they would return more conſtantly to the ſame Situation than lighter ones. But notwithſtanding each of them would ſettle very exactly in the ſame Place, for a great Number of Trials made immediately one after another, yet I found them at different Times to differ conſiderably from their former Directions.

This occaſioned my making them narrower, fearing their Breadth had been ſome way concerned in this Irregularity. But after the Alteration, I found the ſame thing happened, though I could find nothing of it to proceed from any Friction upon the Point. This made me prefer the lighter Needles, as leſs apt to injure the Point they moved upon, and as exact in returning to the ſame Situation. After many Trials, I found all the Needles I made uſe of, would not only vary in their Direction upon different Days, but frequently at different times of the ſame Day; and this Difference would ſometimes amount to upwards of half a Degree in the ſame Day, ſometimes in a few Hours. And this Alteration I obſerved, whether the Needles were drawn aſide immediately before the Obſervation, or ſuffered to remain undiſturbed. For I have left the Box ſtanding for ſeveral Days together, without ever diſturbing the Needle, only have taken notice what it pointed at, and the Time of the Day, and I could ſometimes perceive in a few Minutes a very ſenſible Alteration. But whether it ſtood near its greateſt or leaſt Variation, or whether I drew the Needle to one Side with a Key a few Degrees or a greater Number, it would conſtantly return to the ſame Place it ſtood at immediately before. Sometimes I have taken the Needle out of the Box, and put it in again, and this I have repeated ſeveral times in the ſpace of an Hour. At other times I have taken down the Box from off the Stool, and put it up again, but have found no Alteration in its Direction; ſo that I found it of no Conſequence, whether the Needle was drawn aſide or let alone, the ſhaking of the Floor by walking upon it, or the trembling of the Houſe by the Coaches in the Street, was ſufficient to overcome the ſmall Friction upon the Point. When I made the Obſervations, I was very careful to have no Keys, nor Iron about me, that could affect the Needle.

The Box was placed in the Room above the Diſtance of ſix Foot from the neareſt Wall, and above thirteen Foot from the Grate in the Chimney, and no Iron could at any time be brought near it without my Knowledge. Yet, after all, I am not ſatisfied that it was out of the reach of Iron, and that the Variation ſhewn by it is the true Quantity; but I am very ſure there was no Change of Circumſtances in the Room that could affect it, for if there were any ſuch Materials in the Wall, or Floor, their Diſtances and Situations continued the ſame. But for a farther Confirmation of this Irregularity, I put one of the Needles into a Wooden Box, with a few Degrees divided as the other, and placed it at the ſame Meridian Line, at the Diſtance of three Foot and a half from the other, and found both Needles nearly agreed in their Alterations. The Needles were all touched by that excellent Loadſtone preſented to the Society by the Right Honourable the Lord *Paiſley*. It may not be improper to take Notice, that the Needles were not touched upon the naked Stone, but with its Armour on, generally upon that Part of the Capping neareſt the Poles ; but I could not find a Difference in the Direction, by touching upon another Part. I may add, that when I have obſerved the Needle increaſing, or decreaſing in its Variation, I have very frequently, with a Key, drawn it the contrary Way ſeveral Degrees, and then, by letting it return very gently, till it has been within a Degree, or leſs, of the Place it ſtood at immediately before, I have there ſtopt it for ſome time, by holding the Key at a proper Diſtance; and withdrawing my Hand gradually, have tried to make it ſtand ſhort of its former Place, but could never ſucceed. By this Method, and ſeveral others made uſe of, I am well aſſured theſe Changes in the Direction are owing to ſome other Cauſe than the Friction of the Needle upon the Pin ; but what that Cauſe is I cannot ſay, for it ſeems to depend neither upon Heat nor Cold, a dry or moiſt Air, clear or cloudy, windy or calm Weather, nor the Height of the Barometer. The only thing that has any appearance of Regularity, is, that the Variation has been generally greateſt, for the ſame Day, between the Hours of Twelve and Four in the Afternoon, and the leaſt about ſix or ſeven in the Evening.

March 8. 1722.

This Day a piece of Braſs was fixed to a Wooden Box, and a few Degrees were divided into 10′ each, as in the Braſs Box, to try if both Needles would be alike affected in the ſeveral Alterations. This Wooden Box was placed at the ſame Meridian Line, and about the Diſtance of 3½ Feet from the other.

Braſs Box.	Needle 2=5.	h.	Needle 5=5
March 8.	14°=30′—	3=00′	14°=25′+
	14=20	3=15	14=20
	14=15 +	4=00	14=10
	14=20	4=15	14=15
	14=25	5=00	14=20
	14=25	5=30	14=20
	14=15	5=45	14=10
	14=00	5=57	14—
	14—	6= 8	13=55
	13=50	6=15	13=40
	14=20	6=38	14=15 +
	14+	6=48	14=00
	14=00	6=54	14—
	14= 5	7= 5	14+
	14=10	7=15	14= 5
	14+	12=00	14+

Braſs Box.	Needle 2=5.		Needle 5=5.
March 9.	14° = 10′	9^{h} = 30′	14° = 10′
	14 = 10 +	10 = 00	14 = 10 +
	14 = 10	10 = 15	14 = 10
	14 = 10 +	10 = 30	14 = 10 +
	14 = 15	11 = 00	14 = 15 −
	14 = 00	8 = 15	14 = 00
	14 = 00	11 = 50	14 = 00
March 10.	14° = 10′ +	10^{h} = 00′	14° = 10′
	14 = 15	11 = 00	14 = 10 +
	14 = 15	12 = 00	14 = 10 +
	14 = 15 +	12 = 45	14 = 10 +
	14 = 15 +	1 = 00	14 = 10 +
	14 = 15 +	1 = 30	14 = 10 +
	14 = 15 +	1 = 45	14 = 10 +
	14 = 15 +	2 = 00	14 = 10 +
	14 = 15	3 = 30	14 = 10
	14 = 15 +	4 = 00	14 = 10 +
	14 = 15 −	5 = 30	14 = 10 −
	14 = 10	6 = 00	14 = 5
	14 = 00	6 = 15	14 = 00
	14 −	6 = 30	14 −
	14 +	7 = 30	14 +
	14 = 5	7 = 45	14 +
	14 +	12 = 00	14 +

March 30.

The Needle 2=5 which was in the Braſs Box, was this Day put into the Wooden Box, and a new Needle put into the Braſs Box, Weight 2=3.

No remarkable Change happened to either Needle till *April* the 5th.

Needle 2=3 in the Braſs Box.			Needle 2=5.
April 5.	14° = 5′	9^h = 00′	14° + 00′
	14 = 10	1 = 30	14 = 5
	14 = 10 —	5 = 30	14 +
	14 —	8 = 15	14 —
	13 = 50 +	8 = 37	13 = 45
	13 = 55 +	9 = 45	13 = 45 +
	14 —	10 = 25	13 = 50
	14 = 00	10 = 45	13 = 55
	14 +	11 = 00	14 = 00

The firſt Column ſhews the Variation of the Needle in the Braſs Box. The third the Variation of that in the Wooden Box. The ſecond Column ſhews the Time, by the Clock, when the Obſervations were made.

April 15. 1723.

14° = 30′ —	$9^{h} = 00'$
14 = 30 —	10 = 00
14 = 30	11 = 30
14 = 30 +	12 = 30
14 = 30 +	1 = 30
14 = 30	3 = 30
14 = 30	4 = 10
14 = 30 —	5 = 30
14 = 20	6 = 18
14 +	7 = 8
14 = 00	7 = 50
14 = 00	8 = 15
14 = 15 +	8 = 20
14 = 15 +	8 = 40
14 = 15 +	12 = 15
14 = 00	12 = 27
14 = 00	12 = 32
14 = 00	12 = 35
14 = 00	12 = 43

Wind at S. W.

April 16.

14° = 30′ —	$9^{h} = 30'$
14 = 30	11 = 00
14 = 30	12 = 00
14 = 30 +	1 = 10
14 = 30 +	1 = 40
14 = 30	2 = 45
14° = 30′	$5^{h} = 00'$
14 = 30 —	6 = 00
14 = 25	6 = 20
14 = 20	6 = 30
14 = 15	6 = 35
14 = 10	6 = 40
14 = 10 —	6 = 45
14 = 5	6 = 49
14 = 00	6 = 57
14 = 00	7 = 10
14 +	7 = 20
14 = 5 +	7 = 30
14 = 10	7 = 45
14 = 15	8 = 00
14 = 15 +	8 = 20
14 = 20 —	8 = 30
14 = 25 +	9 = 00
14 = 25	12 = 12
14 = 25	12 = 21

Day warm, cloudy in the Morning, Evening clear.

April 19.

14° = 30′ —	$8^{h} = 35'$
14 = 30 —	9 = 00
14 = 30 —	1 = 30
14 = 30 —	2 = 00
14 = 30	3 = 30
14 = 30	4 = 00
14 = 20	5 = 00
14 = 25	5 = 38
14 = 25 +	5 = 45

14° = 30′ —	6^{h} = 00′
14 = 30 —	6 = 45
14 = 25	7 = 00
14 = 20 —	8 = 00
14 = 20	9 = 00
14 = 20 +	10 = 00
14 = 25	11 = 00
14 = 25	11 = 15

Day warm, Wind at Eaſt, ſome Thunder in the Afternoon.

May 2.

14° = 25′	9^{h} = 30′
14 = 30 +	10 = 30
14 = 35 —	11 = 30
14 = 35 +	1 = 00
14 = 35 +	1 = 52
14 = 40 +	2 = 30
14 = 20	3 = 30
14 = 25	3 = 45
14 = 30 —	4 = 00
14 = 25 +	6 = 45
14 = 20	7 = 00
14 = 30 —	7 = 35
14 = 20 +	12 = 50

Day cold, Wind at Eaſt.

May 3.

14° = 10′	9^{h} = 30′
14 = 15	11 = 10
14 = 15 +	12 = 40
14 = 15 +	2 = 20
14 = 10 —	5 = 20
14 = 10	6 = 5
14 = 10	6 = 45
14 = 10 —	7 = 5
14 = 5	7 = 15
14 +	7 = 30
14 = 00	7 = 42
14 +	8 = 00
14 +	9 = 38
14 +	10 = 15
14 +	11 = 00

Day cold, Wind Eaſterly.

May 4.

14° = 5′	9^{h} = 15′
14 = 5	9 = 30
14 = 10 +	1 = 35
14 = 10 +	3 = 17
14 = 10 +	3 = 50
14 = 10	4 = 55
14 = 10	6 = 00

14 =

$14^\circ = 00'$	$8^h = 15'$
$14 +$	$1 = 00$

Windy at Eaſt.

May 5.

$14^\circ = 10' +$	$9 = 30'$
$14 = 15$	$10 = 45$
$14^\circ = 15' +$	$12^h = 30'$
$14 = 20$	$1 = 57$
$14 = 20$	$2 = 45$
$14 = 20$	$3 = 25$
$14 = 20 -$	$4 = 35$
$14 = 15 +$	$5 = 30$
$14 = 15 +$	$6 = 10$
$14 = 15 -$	$12 = 7$

Day clear, Wind at Eaſt

All theſe Obſervations are of the lighteſt of the three Needles, the Compaſs Box remaining unmoved the whole time. From *February* 6. 1722. to the 10th of *May* following, I made above a thouſand Obſervations in the ſame Place; and the greateſt Variation Weſtward, was $14^\circ = 45'$, and the leaſt — — $13^\circ = 50'$. It was ſeldom leſs than 14°, or greater than $14^\circ = 35'$.

V. *Some Obſervations upon Dr.* Eaton's *Styptick. By Dr.* Sprengell, *R. S. S. Coll. Med. Lond. Lic.*

THE Method of curing freſh Wounds in a few Days, without Suppuration, where neither Nerves, large Veſſels, Bones, or any of the *Viſcera* were concerned, has been long ago obſerved. *Purman*, a famous Surgeon of *Breſlaw*, in his *Chirurgia Curioſa*, tells us of a Mountebank, who gave himſelf thirteen Wounds, by Inciſion, in the upper Part of his Left Arm, and thereupon applied his *Noſtrum*, and with the help of a good Roller, was cured in two Days Time. Next he mentions a martial Styptick, which ſtopped bleeding incomparably, and healed freſh Wounds (as he ſays) in two Days time, eſpecially if the Patient took withall a few Drops inwardly. This has likewiſe been mentioned by *Blegny* near thirty Years ago.

When I came into *France*, I found that many little Trials were made there alſo, with a Styptick Ball mixed with *French* Brandy, by ſtriking a Cock thro' the Head, opening the Crural-Artery of a Dog, or chopping off a Dog's Leg, &c. But I found that it did not amount to any thing of Conſequence; yet I have ſtill a Ball, made above twenty Years ago, of Filings of Iron, and an equal Quantity of Tartar, mix'd well with *French* Brandy upon a Marble. This, with ſome Alteration, was afterwards publiſhed by the famous *Helvetius*, Phyſician to *Lewis* XIV of *France*, in a Book called, *Recueil des Methodes pour la Gueriſon des diverſes Maladies*, which was reprinted in *Holland*

Holland in the Year 1710. This Preparation was then ſold by *Pierre Rottermond*, Apothecary at the *Hague* in *Holland.*

The *Recipe* for his Medicinal Ball I have tranſlated, and is as follows:

Take four Pounds of the Filings of Steel, and eight Pounds of Tartar, well powdered; mix theſe well together, and put them in a new Earthen Pot, and pour thereupon as much *French* Brandy as will make it into a Poulteſs. Let this ſtand fermenting in a Cellar for four Days, and ſtir it between whiles. Then put it in *Baln-Mar*, and diſtill it *S. A.* with a moderate Fire, to draw off the Brandy. When you find that nothing but the Phlegm comes over, then take it from the Fire, and take out the Maſs, ſtamp it very fine, that not the leaſt Lump may remain; then mix it again as before with a ſufficient Quantity of Brandy, and put it in the Cellar to ferment, as before, and then diſtill it a ſecond Time. This Operation may be reiterated ſeven or eight times; but the laſt Time mix your Maſs well upon a Marble, and form it into two Ounce-Balls. One of theſe Balls is ſteeped in a Pint of good *French* Brandy, a little warmed, and hung only in it by a Wire, till the Brandy has received the Colour of the Ball. But if you are in haſte, then grate a ſufficient Quantity of the Ball in ſome Brandy, ſtir it well, and you may uſe it that very Inſtant.

No doubt but the Author thought by often grinding, fermenting, and diſtilling this Maſs, to comminute and ſubtilize its Particles, ſo as to make it more fit to contract the Fibres and Veſſels of a Wound, and to prevent Stagnations of the Fluids, both within and without, upon Contuſions; but the Succeſs did did not anſwer, and therefore it was laid aſide. Neither did *Helvetius* ever recommend it as an Univerſal

Styptick, aſtringent, or conſolidating Medicine, but meerly in freſh Wounds, and that only for a firſt Dreſſing, and where People lived at a Diſtance, and could not get immediate Aſſiſtance from a Surgeon. Beſides, he makes ſeveral Exceptions where it ſhould not be uſed; and, in general, adviſes it where Chalybeat Medicines may be made uſe of according to Experience.

But laſt Year a Balſamick Styptick was publiſhed by one Dr. *Eaton*, good to ſtop all manner of Bleeding without or within, and that without any manner of Exceptions. This made me deſirous to ſee it, and as it happened that ſoon after I had an Opportunity to examine it: I found preſently that this was the ſame old Medicine, which was got hither alſo, after other Countries had diſcarded it. But I neglected it at that Time, as not worth my notice, till I ſaw lately a Treatiſe of Conſumptions, publiſhed by Sir *Richard Blackmore*, Kt. giving Dr. *Eaton*'s Styptick the greateſt *Encomium*, that ever was given to any Invention whatſoever. For, ſays he, *Dr.* Eaton'*s Balſamick Styptick bids fair for the Credit of a certain Remedy in ſtopping of Blood outwardly or inwardly, where the* Craſis *of the Blood is not entirely ruined; and will be of more Service to Mankind, than all the Diſcoveries made by Galenical Compounders of Drugs, and Syſtematical Methodiſts.*

Finding this Remedy recommended in ſo extraordinary a manner, by ſo eminent a Phyſician, I began now to think, that poſſibly I might have been miſtaken, and therefore deſired Mr. *Winterbottom*, an Apothecary in *Bow-Lane*, immediately to prepare the *Recipe*, as deſcribed by *Helvetius*. When this was ready, I ſent for a Bottle of Dr. *Eaton*'s Styptick, and tryed them both with Galls, before ſeveral Gentlemen; the Tincture was the ſame, a deep Purple. I then precipitated the Contents with old Hock, and found the precipitated Matter to be the ſame in both. Not contented with my own Enquiry, I ſent ſeveral ſmall Quantities to others, and went my ſelf to Mr. *Godfrey*, a famous Chymiſt in *Covent-Garden*. They all told me, that there was no Difference betwixt them. I tryed ſeveral ways to find out its Balſamick Quality, from whence it has its Name, but found none. Then, I confeſs, it ſurprized me, that a Man who had a mind to vend a thing as a Secret, had not done ſo much as to alter it either in Taſte, Smell, or Colour; and yet this might very well have been done, without robbing it of its Virtue in the leaſt.

My next Buſineſs was to try theſe two upon the Crural-Artery. Having got a good middle-ſiz'd Dog, Mr. *Ranby*, Surgeon, laid the Artery bare, and open'd it with a Lancet the length Way of the Artery, for near half an Inch. The old Trick uſed to be, to cut the Artery croſs ways, and then there was no neceſſity of a Styptick at all, nor indeed here neither. But at firſt *Helvetius* his Tincture was applied, and ſtopped the Bleeding; then we opened the Artery again, and tryed Dr. *Eaton*'s, with the ſame Succeſs. I then had the Artery opened in the other Thigh, and tryed it only with *French* Brandy, which, I found, did as well as the other two. I opened the Artery again, and had diſſolved in *French* Brandy a little *Sal Martis* and *Saccharum Saturni*, and applied that, and it anſwered in the ſame manner. This made me immediately conjecture, that there was but little Virtue in either of them, but only that the Brandy, by its great Heat, did meerly contract the Fibres of the Artery, which, no doubt, might be a little aſſiſted by the *Cha-*

lybs; but this could not be much. I then reflected upon the ſmallneſs of the Crural Artery in a Dog, and that it was no more to be compared to the Crural Artery of a Man, than a Cock's-Head to a Man's Head, and that a little Pledget of Lint might ſtop the Blood without more to do, as well as the Temporal Artery when opened with a Lancet, which we did, and the Pledget of Lint ſtopped it. We then untied the Dog, and ſent him going, who ran directly Home to the Crown Tavern in *Bloomſbury*, from whence we had taken him. The Miſtreſs of the Houſe tore off the Pledgets, and had the Dog well waſh'd with Butter and Beer warmed, ſhe not knowing what had been done to him, upon which the Dog fell a Bleeding again, though not much, and the Blood ſtopp'd of it ſelf: Mr. *Ranby* and I ſaw the Dog in the Evening very well, but he ran away at the Sight of us. Thus far as to its outward Uſe, and I could wiſh it were as harmleſs within. If only, according to *Helvetius*, it had been ordered to be taken inwardly, in freſh Wounds and Contuſions, a Body might have let it paſs; but when, without Exception, Dr. *Eaton* recommended it,as alſo even Sir *Richard*, in all inward Bleedings, I then thought it high Time to make ſome Animadverſions upon it. For Sir *Richard* himſelf ſays, in his Treatiſe of Conſumptions, *p*. 99 and 101, That in Spitting of Blood there is an Orgaſm, or ſtimulating Ferment: What is this but a Feveriſh Indiſpoſition? And is there any Hemorrhage without? Now, if ſo, will not Brandy and Chalybeats heighten this? Which by their Heat and *Stimuli* brace and irritate the Fibres, and accelerate the Blood's Motion. And will not then the Blood take up more Room, and preſs harder againſt the Sides of the Veſſels, and whatſoever oppoſes it?

Is not this the Way to make an Orgaſm, and cauſe an Hemorrhage?

Dr. *Eaton* tells us himſelf in his Book, *p.* 57, That it did very much over-heat a Gentlewoman, and that her Bleeding ſtill continued after the taking of it, and ſhe might have periſhed if a Surgeon had not given her a cooling and aſtringent Apozeme. And but juſt before, *p.* 47, he complains of a Phyſician, that was not willing that his Patient ſhould take it, who had a Hectick Fever upon her, becauſe he was afraid that it was too hot.

Since the former Tryals, on the 10th of *June* laſt, I deſired Mr. *Ranby*, Surgeon, to open the Carotide Artery of a Dog, thinking that this Artery might give me more Satisfaction than the Crural Arteries had done, to try the Styptick Quality of *Helvetius* and *Eaton*'s Tinctures. Having laid bare the Jugular Vein, divided, and tied it, that its Bleeding might not hinder us from finding the Carotide Artery, we were obliged to cut ſome of the Muſcles through likewiſe, till, with ſome Difficulty, we found the Artery; which being opened with a Lancet, the Blood ſpouting forth, I applied to it *Helvetius*'s Tincture, upon which the Blood ſtopp'd. I took it off in leſs than a Minute, and made it bleed again, but it bled but little, and then I applied Dr. *Eaton*'s Styptick; we fill'd up the Wound with Lint, and ſtitched up the *Cutis*, then untied the Dog and let him run down Stairs, where, after ſome Time, I ſaw him again, and found he had bled a good deal, and was ſtill bleeding. I was very well ſatisfied that, the Artery being ſo very ſmall, he would not bleed to Death, and if he had had nothing but Lint upon it, it would have done as well. It muſt be obſerved, that the Carotide Arteries are largeſt in Proportion in Human Bodies, and that this Artery of the Dog was but a ſmall matter bigger than the Crural Arteries of the former Dog. This ſhews, that the Styptick Quality of theſe Tinctures is very inconſiderable; and that *Helvetius*'s Tincture is rather better than Dr. *Eaton*'s, if there is any Difference at all; though that, I believe, was owing to the Brandy, for my Brandy was ſtronger than Dr. *Eaton*'s.

I enquired for the Dog the next Morning, and found him alive and well, only hanging his Head on one Side, which proceeded from the Muſcles being cut through.

VI. *Materiarum quarundam gravitates Specificæ, diverſis temporibus ad varios ſcopos exploratæ a D. G.* Fahrenheit, *R. S. S.*

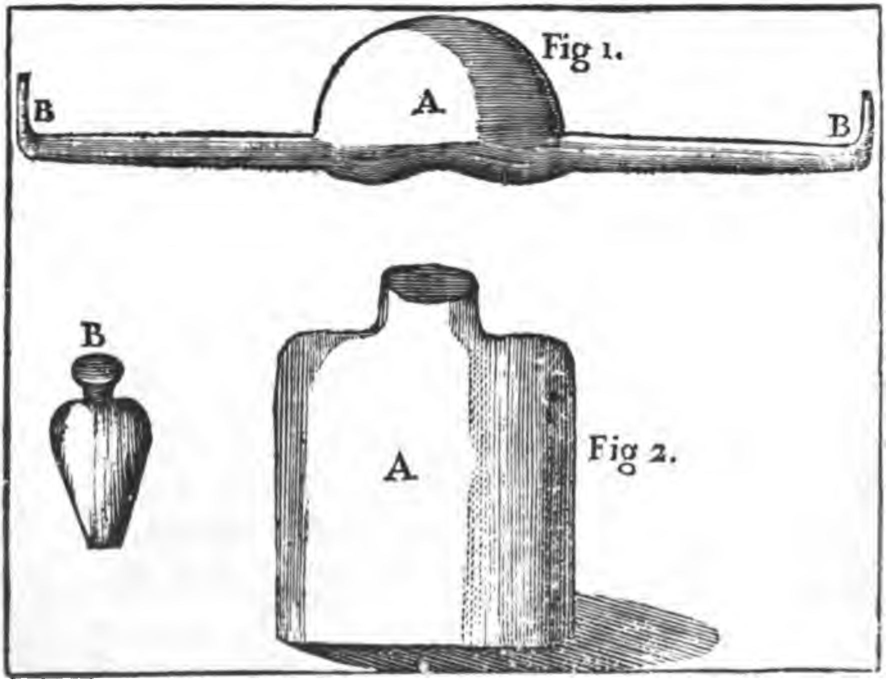
Aurum	19081
Mercurius	13575*
Plumbum	11350
Argentum	10481
Cuprum Suecicum	8834
Idem Japonenſe	8799
Ferrum	7817
Stannum provinciæ Indiæ Orientialis vulgo dictæ Malacca	7364
Stannum Anglicanum	7313
Marcaſita alba	9850
Regulus Antimonii	6622
Aurichalcum	8412
Cryſtallus de rupe	2669
Pyrites homogeneus	2584

Cinis clavellatus ſordibus, ſaleq; neutro quodam (quod fere ſemper magis vel minus in cinere illo reperitur) depurgatus	3112
Sal illud neutrum	2642
Sal maritimum	2125
Nitrum	2150
Alumen	1738
Saccharum albiſſimum	1606$\frac{1}{2}$
Oleum Vitrioli	1877$\frac{1}{2}$ *
Lixivium cineris clavellati ſale quantum fieri potuit impraegnatum	1563 *
Idem alio tempore præparatum	1571$\frac{1}{5}$ *
Aqua fortis melioris notæ	1409 *
Spiritus nitri	1293$\frac{1}{7}$ *
Aqua pluviatilis	1000 *
Oleum Raparum	913
Alcohol vini	826
Idem magis dephlegmatum.	825

Experimenta variis ſunt facta modis. Corpora enim fixa, ut vulgo fieri ſolet, prius bilancis accuratioris ope in aere & deinde in aqua pluviatili ſunt ponderata. Salium pondus, prius in aere, & tunc in liquore idoneo quodam eſt exploratum, & deinde calculo ad gravitatem aquæ comparatum. Liquorum gravitates, interdum Aræometro quodam ſingulari (cujus deſcriptionem alio tempore dabo) aliquando autem vaſibus hic delineatis ſunt indagatæ.

Globus Vitreus concavus A (*Fig.* 1.) ad Lampadis flammam ſatis magnus conficitur, duobus tubulis vitreis ſibi oppoſitis B & B præditus. Tubulorum extremitates ſunt apertæ, attenuatæ, & aliquantiſper incurvatæ, ne liquor effluere poſſit. Globulus præterea in inferiori loco aliquantulum eſt applanatus, ut eo commodius bilanci imponi poſſit.

Icones inſtrumentorum quæ inſervierunt ad facienda experimenta de gravitate ſpecificâ liquorum.



Ampulla A (*Fig.* 2.) e tenuiſſimo vitro ad lampadis flammam paratur, collo ſatis largo prædita, cujus apertura operculo B, intus concavo tam accurate, quam fieri poteſt, clauditur.

Ope hujus ampullæ, etiam ſalium gravitates ſpecificæ explorari poſſunt, & quidem hoc modo. Ampulla prius liquore quodam idoneo (in quo nempe ſal, cujus gravitas exploranda eſt, non ſolvitur) impletur, & poſtquam liquoris innotuit pondus, liquor effunditur, atque vas probe exſiccatur. Hoc facto, ſale fere totum vas impletur, & ſalis pondus inquiritur; hoc noto, interſtitia ſalis liquore replentur, ponderiſq; incrementum a liquore addito quæritur. Si hocce incrementum ponderis a pondere toto liquoris ſubtrahitur, reſiduum exprimet gravitatem liquoris a ſale deturbati.

Sal neutrum cineris clavellati in ſpiritu nitri nullam facit ebullitionem. Mercurium in ſpiritu nitri ſolutum albo colore præcipitat. Carbonibus ſuperimpoſitum, crepitu in minores partes diſrumpitur & diſpergitur.

Nitrum in tigillo ſupra ignem fuit liquefactum, ut eo modo ab omni humiditate depurgaretur, ſpatiaque nonnulla aëre alias plena nitro ipſo replerentur.

Gravitates liquorum, quæ aſteriſco notatæ ſunt, ad gradum quadrageſimum octavum meorum thermometrorum calculo ſunt revocatæ, & nonnullorum jam in* experimentis de gradu ebullitionis liquorum quorundam facta eſt mentio.

Modus ſimpliciſſimus ad inveſtigandam gravitatis differentiam, quæ a diverſo temperamento fluidorum originem ſuam trahit, eſt, ut prius liquore minus calido (cujus tamen gradus ope Thermometri notus eſſe debet) vas aliquod repleatur, & ponderetur, deinde illud vas iterum calidiori liquore impleatur, & ut prius ponderetur. Si in hoc ſecundo experimento gradus caloris iterum eſt notatus, habebitur differentia gravitatis liquoris, a calore inter hos gradus effecta, quæ deinde ope calculi facile unicuique gradui attribui poteſt.

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* *Phil. Tranſ.* No. 381.

Experimenta in aere ſunt facta: addenda ergo erit unicuiq; numero gravitas aeris, ut habeatur materiarum gravitas in vacuo. Eſt autem aeris gravitas ſpecifica ad illam aquæ, fere ut 1 ad 1000, utpote Philoſophiæ naturalis cultoribus abunde patet.

VII. *Some Conſiderations about the Cauſe of the univerſal Deluge, laid before the Royal Society, on the 12th of* December 1694. *By Dr.* Edmond Halley, *R. S. S.*

THE Account we have of the univerſal Deluge is no where ſo expreſs as in the Holy Scriptures; and the exact Circumſtances as to point of Time, do ſhew that ſome Records had been kept thereof more particularly than is wont in thoſe things derived from remote Tradition, wherein the Hiſtorical *Minutiæ* are loſt by length of Time. But the ſame ſeem much too imperfect to be the Reſult of a full Revelation from the Author of this dreadful Execution upon Mankind, who would have ſpoke more amply as to the Manner thereof, had He thought fit to lay open the Secrets of Nature to the ſucceeding Race of Men; and I doubt not but to all that conſider the 7th Chapter of *Geneſis* impartially, it will paſs for the Remains of a much fuller Account of the *Flood* left by the Patriarchs to their Poſterity, and derived from the Revelation of *Noah* and his Sons. It muſt be granted, that there are ſome Difficulties as to the Conſtruction of the *Ark*, the Reception and Agreement of the *Animals* among themſelves, and Preſervation of it in ſo immenſe and boundleſs an Ocean, during that *Wind* which God ſent to dry the Waters away, eſpecially when it firſt came on Ground: But it muſt alſo be allowed, that length of Time may have added, as well as taken away many notable Circumſtances, as in moſt other Caſes of the Story of remote Times and Actions.

This we may, however, be fully aſſured of, that ſuch a Deluge has been ; and by the many Signs of marine Bodies found far from and above the Sea, 'tis evident, that thoſe Parts have been once under Water: or, either that the Sea has riſen to them, or they have been raiſed from the Sea; to explicate either of which is a Matter of no ſmall Difficulty, nor does the ſacred Scripture afford any Light thereto. All that it ſays to help us is, that all the Fountains of the great Deep, תְהוֹם רַבָּה, were burſt, or broken up; that the Windows, or Cataracts of Heaven were opened, and that it rained inceſſantly forty Days and Nights. Now the Rain of forty Days and Nights will be found to be a very ſmall Part of the Cauſe of ſuch a Deluge; for ſuppoſing it to rain all over the Globe as much in each Day, as it is now found to do in one of the raineſt Counties of *England* in the whole Year, *viz.* about forty Inches of Water *per Diem*, forty ſuch Days could cover the whole Earth with but about twenty two Fathom Water, which would only drown the low Lands next the Sea, but the much greater Part would eſcape. What is meant by the Fountains of the *Abyſſe* being broken up, and the opening of the Windows of Heaven, ſeems not ſo eaſy to be underſtood, but is intended to indicate the *Modus* of the Deluge, which was, according to the *Moſaic Philoſophy*, from the letting in of the Waters above the Firmament, mentioned *Geneſis* 1. 7. by the Windows of Heaven; and the riſing up out of the Ground of the Waters under the Earth, ſpoken of in the ſecond Commandment: Or, (if you will underſtand that by the תְהוֹם רַבָּה is meant the great Ocean) by the overflowing of the Sea, riſing upon the Land, which is expreſs'd by the breaking up of the Fountains of the great Deep. So that we may reaſonably conclude, that by the one of thoſe Expreſſions is meant an extraordinary fall of Waters from the Heavens, not as Rain, but in one great Body; as if the Firmament, ſuppoſed by *Moſes* to ſuſtain a *Supra-aerial Sea*, had been broken in, and at the ſame Time the Ocean did flow in upon the Land, ſo as to cover all with Water.

By an extraordinary Encreaſe of the Waters this could not be effected, for that at this Time there is not Water ſufficient of itſelf to cover any more of the Earth than now it doth; and to ſuppoſe a Creation and Annihilation of Water on purpoſe to deſtroy the Earth, is by much the moſt difficult Hypotheſis that can be thought of to effect it. A change of the *Center* of *Gravity*, about which Center the Sea is formed, ſeemed not an improbable Conjecture, till it appeared that this Center of Gravity was the neceſſary Reſult of the Materials of which our Globe conſiſts, and not alterable whilſt the Parts thereof remained in the ſame Poſition: And beſides this Suppoſition could not drown the whole Globe, but only that Part thereof towards which the Center of Gravity was tranſlated, leaving the other Hemiſphere all dry.

I ſhall ſay nothing of Dr. *Burnet*'s Hypotheſis, nor of the many Inſufficiencies thereof, as jarring as much with the Phyſical Principles of Nature, as with the Holy Scriptures, which he has undertaken to reconcile.

cile. Dr. *Hook*'s Solution of this Problem, as he has not fully diſcovered himſelf, I cannot undertake to judge of; but his Compreſſion of a Shell of Earth into a *prolate Spheroide*, thereby preſſing out the Waters of an Abyſs under the Earth, may very well account for drowning two extream oppoſite Zones of the Globe; but the middle Zone, being by much the greater Part of the Earth's Surface, muſt by this means be raiſed higher from the Center, and conſequently ariſe more out of the Water than before; and beſides, ſuch a Suppoſition cannot well be accounted for from Phyſical Cauſes, but require a preternatural *digitus Dei*, both to compreſs, and afterwards reſtore the Figure of the Globe.

But the Almighty generally making uſe of Natural Means to bring about his Will, I thought it not amiſs to give this Honourable Society an Account of ſome Thoughts that occurr'd to me on this Subject; wherein, if I err, I ſhall find my ſelf in very good Company.

In Num. 190. of theſe Tranſactions, I have propoſed the caſual *Choc* of a *Comet*, or other tranſient Body, as an Expedient to change inſtantly the Poles and Diurnal Rotation of the Globe; at that Time only aiming to ſhew how the *Axis* of the *Earth* being chang'd, would occaſion the Sea to recede from thoſe Parts towards which the Poles did approach, and to encreaſe upon and overflow thoſe Parts wherefrom the Poles were departed; but at that Time I did not conſider the great Agitation ſuch a *Choc* muſt neceſſarily occaſion in the Sea, ſufficient to anſwer for all thoſe ſtrange Appearances of heaping vaſt Quantities of Earth and high Cliffs upon Beds of Shells, which once were the Bottom of the Sea; and raiſing up Mountains where none were before, mixing the Elements into ſuch a Heap as the Poets deſcribe the *old Chaos* ; for ſuch a *Choc* impelling the ſolid Parts would occaſion the Waters, and all fluid Subſtances that were unconfined, as the Sea is, with one *Impetus* to run violently towards that Part of the Globe where the Blow was received ; and that with Force ſufficient to rake with it the whole Bottom of the Ocean, and to carry it upon the Land ; heaping up into Mountains thoſe earthy Parts it had born away with it, in thoſe Places where the oppoſite Waves balance each other, *miſcens ima ſummis*, which may account for thoſe long continued Ridges of Mountains. And again, the Recoil of this Heap of Waters would return towards the oppoſite Parts of the Earth, with a leſſer *Impetus* than the firſt, and ſo reciprocating many times, would at laſt come to ſettle in ſuch a Manner as we now obſerve in the Structure of the ſuperficial Parts of the Globe.

In this Caſe it will be much more difficult to ſhew how *Noah* and the *Animals* ſhould be preſerved, than that all things in which was the Breath of Life, ſhould hereby be deſtroyed. Such a *Choc* would alſo occaſion a differing Length of the Day and Year, and change the Axis of the Globe, according to the Obliquity of the Incidence of the Stroak, and the Direction thereof, in relation to the former Axis. That ſome ſuch thing has happened, may be gueſs'd, for that the Earth ſeems as if it were new made out of the Ruins of an old World, wherein appear ſuch Animal Bodies as were before the Deluge, but by their own Nature and Defences from the Weather, have endured ever ſince, either petrified, or elſe entire in *ſtatu naturali*. Such a *Choc* may have occaſioned that vaſt Depreſſion of the *Caſpian Sea*, and other great Lakes in the World ; and 'tis not unlikely, but that extream Cold felt in the

North-Weſt of *America*, about *Hudſon*'s-*Bay*, may be occaſioned by thoſe Parts of the World having once been much more Northerly, or nearer the Pole than now they are; whereby there are immenſe Quantities of Ice yet unthaw'd in thoſe Parts, which chill the Air to that degree, that the Sun's warmth ſeems hardly to be felt there, and of which the Poet might juſtly ſay, *Frigus iners illic habitat pallorque tremorque —Ac jejuna fames.*

If this Speculation ſeem worthy to be cultivated, I ſhall not be wanting farther to inſiſt on the Conſequences thereof, and to ſhew how it may render a probable Account of the ſtrange Cataſtrophe we may be ſure has at leaſt once happened to the Earth.

VIII. *Some farther Thoughts upon the ſame Subject, delivered on the 19th of the ſame Month. By the ſame.*

I HAVE been adviſed ſince the laſt Day, by a Perſon whoſe Judgment I have great Reaſon to reſpect, that what I then advanced, ought rather to be underſtood of thoſe Changes which might have happen'd to the Earth in Times before the Creation, and which might poſſibly have reduc'd a former World to a *Chaos*, out of whoſe Ruins the preſent might be formed, than of the Deluge whereby Mankind was in a manner extinguiſhed about 4000 Years ſince; that being much more gradually brought to paſs, and with ſome Circumſtances that this Hypotheſis cannot admit of, which abler Pens, perhaps, may account for: What I have advanced, I deſire may be taken for no more than the Contemplation of the Effects of ſuch a *Choc* as might poſſibly, and not improbably, have befallen this Lump of Earth and Water in Times whereof we have no manner of Tradition, as being before the firſt Production of Man, and therefore not knowable but by Revelation, or elſe *a poſteriori* by Induction from a convenient Number of Experiments or Obſervations, arguing ſuch an Agitation once, or oftner, to have befallen the Materials of this Globe. And perhaps, in due Periods of Time, ſuch a Cataſtrophe may not be unneceſſary for the well-being of the future World; to bury deep from the Surface thoſe Parts, which by length of time are indurated into ſtony Subſtances, and become unapt for vegetable Production, by which all Animals are either immediately or mediately ſuſtained: the ponderous Matter in ſuch a Mixture ſubſiding firſt, and the lighter and finer Mould remaining for the latter Settling, to inveſt the exterior Surface of the New World. This may, perhaps, be thought hard, to deſtroy the whole Race for the Benefit of thoſe that are to ſucceed. But if we conſider Death ſimply, and how that the Life of each Individual is but of a very ſmall Duration, it will be found that as to thoſe that die, it is indifferent whether they die in a Peſtilence out of 100000 *per Ann.* or ordinarily out of 25000 in this great City, the Peſtilence only appearing terrible to thoſe that ſurvive to contemplate the Danger they have eſcaped. Beſides, as *Seneca* has it,

Vitæ eſt avidus quiſquis non vult
Mundo ſecum pereunte mori.

N.B. *The foregoing Papers having been read before the Society thirty Years ſince, were then depoſited by their Author in their* Archives, *and not publiſhed; he being ſenſible that he might have adventured* ultra crepidam; *and apprehenſive leaſt by ſome unguarded Expreſſion he might incur the Cenſure of the Sacred Order. Nor had they now been printed, but at the Deſire of a late Committee of the Society, who were pleaſed to think them not unworthy the Preſs.*

Here the Reader is deſired to obſerve, that Mr. William Whiſton's *Book, entituled,* A New Theory of the Earth, *was not publiſhed till about a Year and a half after the Date hereof, and was not preſented before* June 24, 1696. *to the Royal Society.*

F I N I S.

ERRATUM.

Pag. 118. *lin.* 25, 26. *read* Relation.

ADVERTISEMENT.

ALL Perſons concern'd in the Practice of *Inoculating the Small Pox*, are deſir'd to ſend to Dr. *Jurin*, Secretary to the Royal Society, any time before the End of *February* next, an Account of the Name and Age of every Perſon *inoculated* by them, from the Beginning to the End of the preſent Year, the Place where it was done, the Manner of the Operation, the Days of Sickening and of the Eruption, what Sort of Small-Pox was produc'd, and the Event.

Where the true Small-Pox was not produced by Inoculation, it will be of uſe to take particular Notice, whether the Patient had any other kind of Eruption, what Symptoms preceded, or attended it, whether the Inciſions inflam'd and run, and for what Time their running continued.

In caſe of any Perſon's dying after Inoculation, either in the Courſe of the Small-Pox, or after they are gone off, it is deſir'd that a particular Relation of the Caſe may be ſent, atteſted, if it be judged neceſſary, by the neareſt Relations of the Party deceaſed, or by other credible Perſons, that were Witneſſes to the Fact.

Such Gentlemen as have not already ſent in their Accounts for the preceding Year, are now requeſted to communicate them.

Lincoln's-Inn Fields,
Decemb. 7, 1724.

LONDON: *Printed for* W. *and* J. INNYS,
Printers to the Royal Society, *at the Weſt End of*
St. Paul's *Church-yard*.

Philosoph: Transact: N°. 334.

plate. 3

Fig 8.

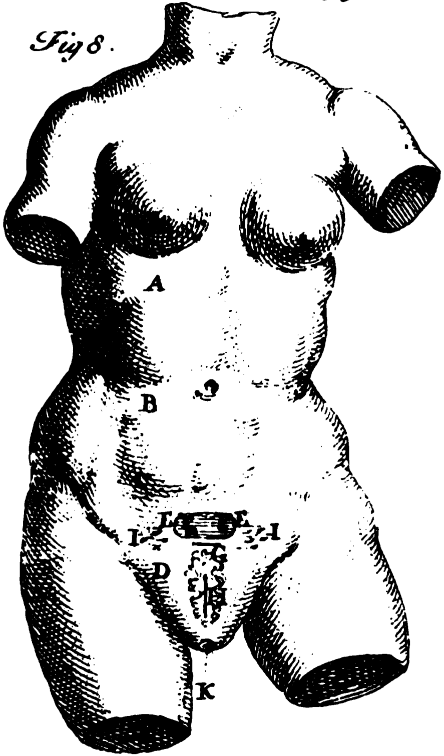
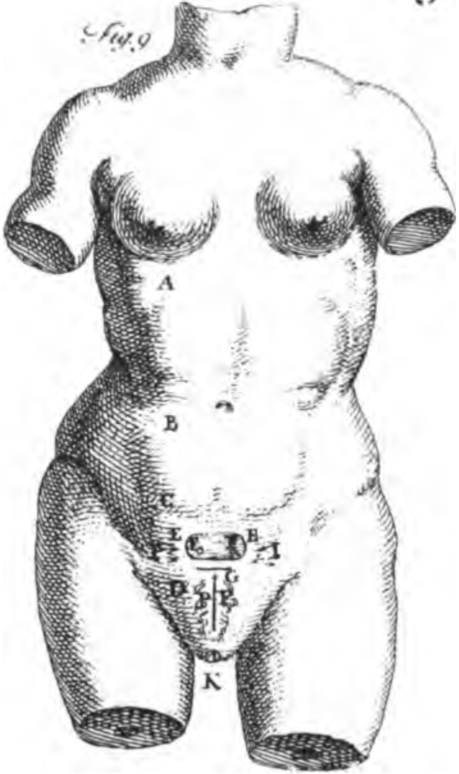
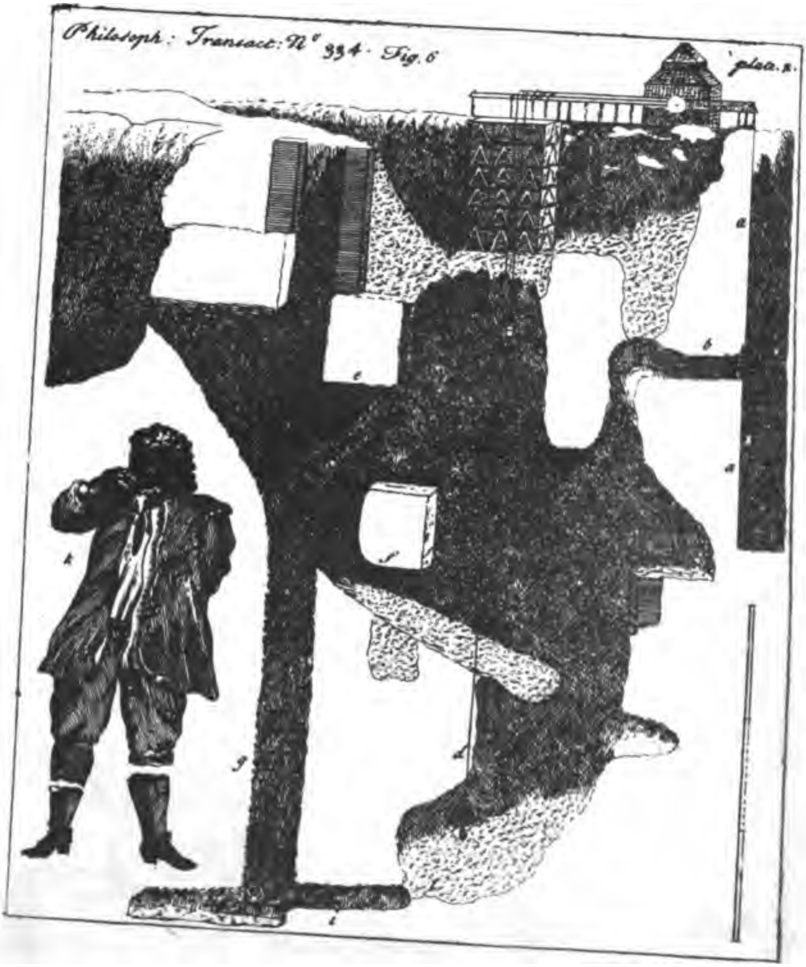


Fig. 9





Philosoph: Transact: No 334 · Fig. 6

plate. 2.

Numb. 384.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *July*, *Auguſt* and *September*. 1724.

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I. *An Account of ſome new Experiments, relating to the different, and ſometimes contrary Motion of the Sap in Plants and Trees, made by* Thomas Fairchild, Gardener *at* Hoxton.

I HAD the Honour, ſome Years ago, to ſhew ſome Experiments before the *Royal Society*, and they were pleaſed to allow the Experiments to be new and uſeful; which encouraged me to try further, and bring more Experiments, in order to ſhew the Courſe of the Sap; which I find, by Experience, will be ſo uſeful, that I can make barren Trees fruitful, and decaying Trees healthful, and render the Syſtem of Gardening and Planting more uſeful to the Publick.

The firſt Plant I ſhewed was the *Laureola*, grafted upon the *Mezereon*, and the *Evergreen Oak* of *Virginia* upon the common *Engliſh Oak*; both which hold their Leaves all the Winter, and are in good State and flouriſhing, though grafted on Plants that drop their Leaves in Winter; which plainly proves that the Juices riſe upwards, in Winter, in thoſe Plants that drop their Leaves, otherwiſe the *Evergreens* that are grafted on them would ſoon periſh.

I believe by grafting the Variety of foreign Oaks on the *Engliſh*, we might make the Timber more firm and laſting, than it is in its own Nature, when raiſed from foreign Acorns: For as the Crab-Stock maketh the Wood of the Apple-Tree more firm and laſting than that on the Apple-Stock, and the Peaches and Almonds, budded on Plums, are more laſting than thoſe on Peach-Stocks; ſo by the contrary Rule, all firm Timber, grafted on ſpungy Stocks, would be made worſe than it would be on its own Bottom. For Example, If that which is called the *Engliſh Elm*, ſhould be grafted on that which is called the *Dutch*, it would partake of abundance of the ſpungy Juices of the Stock, whereby the Timber would become unfit for the Purpoſes it is now uſed for.

The firſt Experiment, I have to offer to your Obſervation at preſent, is made on the *New England Cedar*, or rather *Juniper*, grafted on the *Virginia*; and what is remarkable in it, is, That the Branch, which is grafted, is left ſeveral Inches below the Grafting, which Part continues growing as well as the upper Part above the Grafting.

The ſecond Plant is the *Viburnum*, with the Top planted in the Ground, which is become Roots; and the Roots turned up, which are become Branches. But whether the ſame Veſſels, which fed the Branches, have changed their Courſe, or whether the Juices go up and down in the ſame Veſſels, I ſhall leave to better Judgments; but I find the Plant in as good State of growing, as it was in its natural State.

The third Experiment is made on a Pear-Tree, which I enarched upon two Pear-Stocks, in *March* 1721-2. which is now in a good flouriſhing State, with a Branch in Bloſſom, and receiveth no Nouriſhment but by the two enarched Branches, the Root being out of the Ground; and tho' it was done above two Years ago, it is now ſhooting Suckers out of the Root, which proveth that the Branches are as uſeful to ſupport the Roots, as the Roots the Branches; and it is therefore no Wonder that ſo many Trees miſcarry in Planting, when there are no Branches left on the Head.

Philosoph: Tra

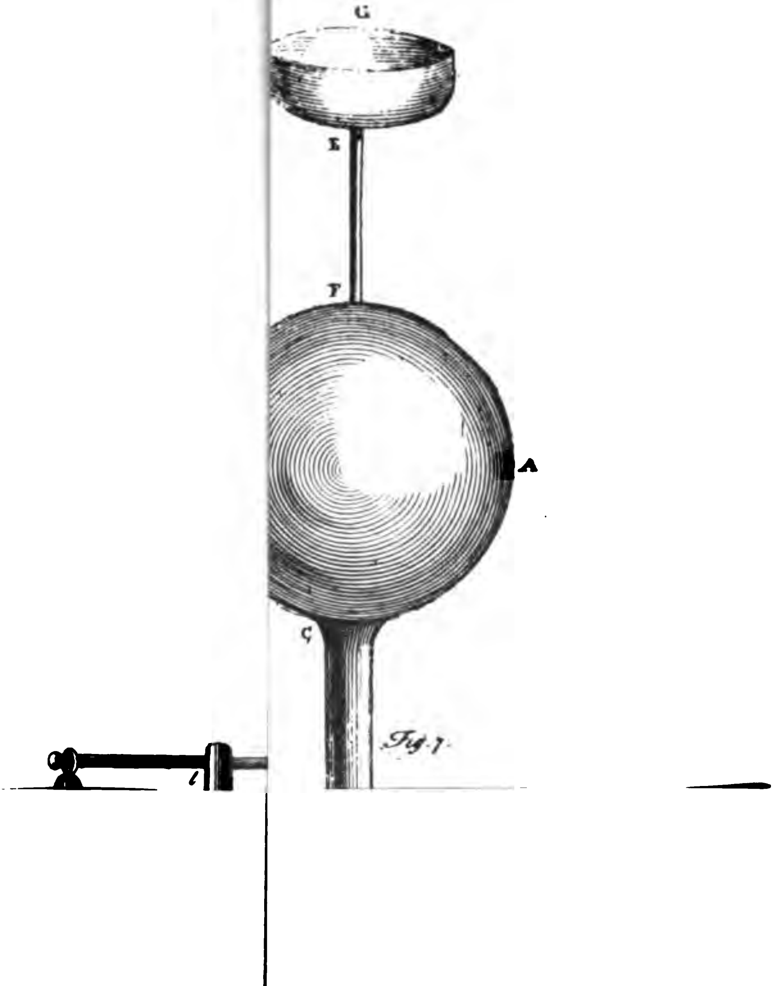

Fig. 7.

the Turff was put into the Oven, the Doctor rubb'd

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The fourth Experiment is made on the Cedar of *Libanus*, grafted on the *Laryx*, which drops its Leaves in the Winter; yet maintains the Cedar in as flouriſhing Condition, as if it had been on a Tree that held the Leaves all the Winter; and the Part of the Graft, left below the Grafting, is in as good Health as the Part above the Grafting.

II. *An Account of an extraordinary Cure by Sweating in Hot Turff; with a Deſcription of the* Indian Hot-Houſes; *by the Honourable* Paul Dudley, *Eſq*; F. R. S.

IN the Year 1704. *Peter Coffin*, Eſq; of *Exeter* in *New England*, being then ſeventy four Years of Age, had taken a great Surfeit, as 'twas thought, by drinking cold Water, in a very hot Day, and when he had heated himſelf in the Woods. This Surfeit ſettled principally in his right Side, but gave him a racking Pain all over his Body, and particularly depriv'd him of the Uſe of his right Arm: In this Condition he kept his Houſe and Bed for nine Weeks, and his Recovery, conſidering his Age, was deſpaired of; when a Son of his, from whom I had the Relation, making a Viſit to his Father, propoſed the Sweating of him in Turff: The Father readily agreed to it, having uſed many Medicines, from other Phyſicians, without any Effect. Immediately Orders were given to cut a large Oven full of Turff; the Pieces might be to eighteen Inches ſquare. The Turff it ſelf was of *Engliſh* Graſs, and only the Sward, or Top of the Earth, with the Graſs. Before the Turff was put into the Oven, the Doctor rubb'd

the

the graſſy Side of the Turff, with ſome Spirit, or Oyl, and then doubled the Graſs-ſides together, and ſo ſet them in. When they were well baked, which was in about two Hours, he took them out, and made a Bed of them upon the Floor, (the Place for the Head raiſed); as ſoon as that was done, he ordered his Father to be taken out of his Bed without his Shirt, but wrapp'd up in a Sheet, and laid upon the hot Turff, and then proceeded to cover him over with the reſt of the Turff, more eſpecially on his Side, where the Seat of his Pain was, but they laid none on his Breaſt or Head; then they cover'd him with Blankets to keep the Heat in: While the Father was in this Bath, the Son gave him warm Cordials, to prevent fainting, which he was in great Danger of; after he had lain thus about three Quarters of an Hour, which was as long as he could bear it, he was put naked into the Bed very well warm'd, where, in a few Minutes, he fell aſleep, and ſweat to that Degree, that it run, thro' his Pillow and Bed, upon the Floor. After about two Hours Sleep they dry'd him, and put him on warm Cloaths, and the old Gentleman found himſelf much eaſed and refreſhed: This was in the Morning; and before Night, he walked about the Houſe comfortably, his Pain being in a manner all gone; the next Day the Doctor repeated his Cordials, and the fourth Day he ſweat his Father a ſecond Time, in the ſame manner as above; and the next, *viz.* the fifth Day, he went abroad about his Buſineſs, and lived eleven Years afterwards in perfect Health, and free from Pain. The Doctor tells me, great Care muſt be taken that the Patient do not lie too long in the Turff, and that even a Quarter of an Hour may be ſufficient for ſome Perſons; and when e'er the Patient begins to fetch his Breath ſhort, or faint, he muſt be put to Bed immediately, and the Phyſician, or Operator, muſt by no means omit his Cordials. I ſhould have been glad to have made this Account yet more perfect, by acquainting you, what the Specifick was the Doctor put upon the Turff, before he ſet them into the Oven, but I could not poſſibly prevail upon him to tell me. As to the Matter of Fact, or Story of curing the old Gentleman, in this new and wonderful Manner, it was fam'd throughout the Country in the Day of it; but the Particulars I lately had an Opportunity of having from the Son, who ſo happily made the Experiment.

Houſes, to ſweat in, were common among the *Aborigines*, when the *Engliſh* firſt came into *New England*, tho' now but little uſed. A Gentleman of the Iſland of *Nantucket*, where the *Indians* ſometimes practiſe it, even at this Day, or very lately, gives me the following Relation.

The Cave was uſually four Foot high, and to eight Foot Diameter; the Roof ſupported with Sticks or Boards, covered with Earth, and they dug it in the Side of a Hill, and, as near as could be, to ſome River, Pond, or Place of Water: The Entrance into this Cave was ſmall, and the Door (when any Perſon was ſweating) was covered with a Blanket or Skin; near the Cave they make a good large Fire, and heat a Parcel of Stones, to the Quantity of five hundred Weight, and roll them in red-hot, piling them up in the middle of the Cave; when this is done, the *Indians* go in naked, and ſet round the heated Stones as many as pleaſe; as ſoon as they begin to grow faint, which may be in a Quarter of an Hour, they come out, and plunge themſelves all over in the Water for a Minute or two, and then in again, as long as they can well bear it, and ſo in the Water a ſecond Time, and then dreſs themſelves. This has been uſed with Succeſs for Colds, Surfeits, Sciatica's, and Pains fixed in the Limbs; and even the *Engliſh*

liſh have many times found Relief by it. I don't underſtand, but that it may be practis'd at any Time of the Year, without Hazard or Inconvenience. The *Indians* often uſed it before, and after long Journies, Hunting or Voyages, to ſtrengthen and refreſh themſelves.

III. *An Attempt to account for the riſing and falling of the Water of ſome Ponds near the Sea, or ebbing and flowing Rivers; where the Water is loweſt in the Pond, at the Time of high Water in the Sea or River; and the Water is higheſt in the Pond, at the Time of low Water in the Sea or River. As alſo for the increaſing or decreaſing of the Water of ſuch Pools and Brooks as are higheſt in the dry Seaſons, and loweſt in the rainy Seaſons: With an Experiment to illuſtrate the Solution of the* Phænomena. *By the Reverend* J. T. Deſaguliers, *L. L. D. and R. S. S.*

HEro Alexandrinus, and other Hydraulick Writers, have deſcrib'd a Cup (call'd a *Tantalus*, from its Effect) which will hold any Liquor very well, when it is not fill'd above a certain Height mark'd in the Cup; but if it be fill'd higher, not only the Liquor above the Mark will run out, but the whole Liquor that was in the Cup. This is perform'd by a Syphon in the Cup, which is ſometimes conceal'd to make the Effect the more ſurpriſing.

The Cup, A B (*Fig.* 1.) has a viſible Syphon C E D in it; the Cup, (*Fig.* 2.) has the ſame, conceal'd by the Figure of a Man, to repreſent *Tantalus* in the Fable; and the Cup of *Fig.* 3. has its Syphon more conceal'd, as it is carried up into the Handle. Any of theſe Cups will hold Water very well, provided they are not fill'd up above the Line F G; for then not only the Liquor that is above F G will run out, but all the Liquor in the Cup as low as D, the Orifice of the ſhort Leg of the Syphon.

EXPERIMENT. Fig. 4.

In the Veſſel *a b c d* is plac'd an open wooden Box A B C D fill'd with Water as high as the Line L M. Another Box or Plug E F G H made tight, and containing Weights to ſink it, is made to let down into the Water between the Partition I K and the End A B of the Box above mention'd; but when it is not to preſs the Water up to I O, (as it does when let down) it is drawn out of the Water by the Weight *m*, which pulls it up by the Bar *i k* faſten'd to a Leaver moving round the Center *l*.

When, by means of the Plug, the Water in the Space A B K I is puſh'd up to I O, by paſſing under K; it runs out thro' the Spout P Q (whoſe Paſſage is gaged by a little Sluce P *p*) and falls into the Veſſel R S made of an oblong Figure like a Fiſh-Pond, and having a Syphon at S, ſo as to make it a *Tantalus*, or in the Nature of the Cups above mentioned.

Let the Weight *m* pull up the Plug E F G H, and the Water, having fill'd R S, will run down below the Orifice P to M.

The *Tantalus* R S, beginning to run out as ſoon as full, will for the Reaſons above given, continue to run till it is all emptied; and as it diſcharges it ſelf into another *Tantalus* T V (whoſe Syphon is at V); this laſt *Tantalus* will alſo, when full, begin to run out, and its Water go down to *x* Y *o*.

If the Plug be let down gradually, as ſoon as the Water begins to run out of the laſt *Tantalus* T V, (and the firſt *Tantalus* R S be cover'd ſo as to be conceal'd from Sight) it will appear to the Lookers on, That the Cavity T V, repreſenting a Pond near an ebbing and flowing River (as I am credibly inform'd there is ſuch an one at *Greenhive* in *Kent*, between *London* and *Graveſend*) always riſes, whilſt the Water at N O (or the Tide) falls to L M; and always ſinks whilſt the Water at L M (or the Tide) riſes to O L.

EXPERIMENT II.

Let the Water in the Box A B C D not be made uſe of; only the Veſſel Z be fill'd every half Hour: It will empty it ſelf in the Space of a Quarter of an Hour, falling like Rain, and dropping alſo thro' the Leaden Platform *e f* into the hidden *Tantalus* R S, which will not begin to run till this artificial Rain is over: Then in a Quarter of an Hour more, the *Tantalus* R S will have emptied it ſelf into the viſible *Tantalus* T V, which will be filling all the Time after Z has done running; (or in the dry Seaſon) and as ſoon as T V is full, it will begin to run out thro' its Syphon V, at the End of the half Hour, when the Veſſel Z or Sieve runs again; that is, at the Return of the rainy Seaſon.

This laſt Experiment may eaſily be applied to thoſe Ponds, or thoſe Brooks, that are high in dry Weather, and low in wet Weather; of which Kind, I am told, there is a Brook at *Lambourn* in *Berkſhire*.

If it be objected, that ſuch Ponds are full for ſome time, which a *Tantalus* cannot be, becauſe it begins to run out as ſoon as full; that may be eaſily ſolv'd, by ſuppoſing the hidden *Tantalus*, (or intermediate Cavity between the River and Pond) to contain more Water than the viſible one, provided it does not contain ſo much as not to be emptied, before the Return of the Tide

The ſame Solution will ſerve for wet and dry Seaſons, only ſuppoſing the Cavities larger.

If it be asked, where the Water of the viſible *Tantalus*, near a River, can run; it may be anſwer'd, that all this may happen, tho' the ſecond, or loweſt *Tantalus* ſhou'd have its Bottom higher than low Water-Mark in the River. And for the Syphons, which are of a particular Make in the Cup; tho' ſuch be not ſuppos'd in the Earth, yet any long Paſſage, riſing in the Middle, will anſwer the End. See *Fig*. 5. where ABCD repreſents the Channel of a River, AD high Water-Mark, and GH low Water-Mark; ZI a Paſſage from the River to the Cavity IKLMN, or firſt, or hidden *Tantalus*; LMQ the Syphon of the firſt *Tantalus*, running into the ſecond *Tantalus*, or viſible Pond OQRP, which by its Syphon RSV runs out into low Grounds that may be above the low Water-Mark GH; and the Bottom KL of the firſt *Tantalus* may be above the Top of the laſt, whoſe Level is the Line WW.

ABCDYOQRPVH is the Section of the Surface of the Earth.

IV. *Nobiliſſimi Viri ADAMI LEYEL Reg. Colleg. Metallici Aſſeſſoris NARRATIO accurata de CADAVERE HUMANO in fodina Cuprimontana ante duos annos reperto. Ex Actis Literariis Sueciæ. Ann. 1722. Trimeſt. I.*

ANimalcula quævis, inſecta, piſces, cochleas & conchylia, fructus quoſcunque, arbores, ſemina, & herbas in lapides abire, & in ſaxeam mutari duritiem, experientia quotidiana, & innumeræ res, lapidea rigiditate aſperæ, tam intra Svioniam, quam apud exteros paſſim obviæ teſtantur. Hæc petrificatio, ſ. ſolidum in lapidem transformatio, originem ducit a tenuiſſimo halitu calcario, aut alio calce gravido minerali vapore, qui ſe fumi inſtar in animalium quorumvis & vegetabilium poros ingerit, ſedem ibi figit, ac vi ſubſalſæ duritiei lapideſcere cuncta cogit. Neque enim aliunde arceſcenda eſt cauſa, cur pleræque tam indigenæ quam peregrinæ ſic dictæ petrificationes, in montibus inprimis calcariis, aut etiam terra, calcarius ubi lapis effoditur, in conſpectum prodeant, & ex iis eruantur: qua de re tam quæ in Germania & Helvetia, quam Anglia & Italia, lucem videre Lithographorum ſcripta, evolvi conſulique merentur.

Veram hancce, & ſolidam mutationem imitatur alia, ſed rudior & imperfectior, & quæ mutationem mentiatur, petrificationis ſpecies, incruſtatio dicta, quam efficit ſaxea in ſubtilem viſcum ſoluta ſubſtantia, qui occurrentium corporum ſuperficiei tenacius ubi adhæſit, lapideam iis cruſtam paulatim induit; haud ſecus ac fruges variæ, ſemina, corticeſque condiri, ſaccharoque induci ſolent. Cujus generis in Bohemia, Germania, Anglia, aliiſque regionibus non pauca occurrunt, ſed inferiore tamen loco, quam vera petrificata, cenſenda.

Porro varia etiam ſalium genera, alumen puta, ſal culinare, & vitriolum, ſimili donata virtute deprehenduntur. Hæc corpora quidem non in ſolidum convertunt lapidem, ſalſitudine tamen ſua, & conſtringendi vi, obvia quævis ita condenſant & indurant, ut nativa poſita mollitie, in lapideam duritatem deflectant.

Vitrioli autem, inter omnia ſalium genera, vaporem proclivem maxime & idoneum eſſe ad duritiem rebus quibuſcunque hoc modo conciliandam, alia ut prætercam, varia probant hominum membra, manus, pedeſque, quæ fodina Fahlunenſium cuprea, in aqua vitriolo fœta, ſubinde occurrentia præbet conſpicienda; quæque vigore ac inſita facultate vitrioli adeo induruiſſe patet, ut putredinis neſcia corruptionem omnem morentur.

Illuſtre inprimis hujus veritatis documentum dedit in laudata modo Fahlunenſium fodina repertum haud ita pridem humanum cadaver, ante 50 circiter annos infelici ruina obrutum ibi contumulatumque: ſed quod erutum integrum, ac ſive carnem, ſive cutem reſpicias, mutationis omnis expers comparuit, præterquam quod corneam contraxiſſet duritatem.

De incorrupto hocce cadavere Novellis Hafnienſium Literariis * narratiunculam ante aliquod tempus inſertam vidimus, quæ tamen quum circumſtantias nonnullas omittat, nonnulla rurſus tradat, quæ cum rei veritate minus congruant, hinc rem non ingratam facturum me ſpero, ſi poſt luſtratum rite cadaver, & collectas fide dignas loci incolarum relationes, facti hiſtoriam ſequentem in modum cum eruditis communicavero, ſchemaque adjunxero, quod non fodinæ minus faciem, quam oppreſſi ruina hominis habitum adumbret.

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* Extrait des nouvelles du mois de Septembre 1720. p. 207.

Anni 1719. menſe Decembri, cum Wrediani putei cuniculus (Wred-*Schachtz Stollen*, vid. Lit. I, Fig. 6.) in eum, qui (*Mårdskins-Schachtz korgeſtaden*, Lit. D.) vocatur, agendus ducenduſque eſſet, in puteo eodem (*Mårdskins Schachtet*) 82. orgyias profundo, loco quem Lit. L. deſignat, in rupis ipſius confinio, ſub aqua & 5 orgyiarum ruina, defuncti hominis corpus in conſpectum venit. Utrumque crus, cum brachio dextro & capite, avulſa petræ moles contuderat: facies vero, corpuſque reliquum cum veſte, integra plane & intacta cernebantur; habituſque totus viri, collariæ faſciæ orâ extremâ, ſiniſtræ manus ope, os obturantis, is erat, quem Lit. K. exhibet ac demonſtrat. Crumenâ, quam gerebat, pyxis ex orichalco oblonga, pyxide vero tabacì condebatur fruſtulum, utroque illæſo & integro; ductitium autem ferrum, quo ad pyxidem annecti operculum volvique ſolet, aqua tincta edaci vitriolo totum abſumſerat. Caro hominis cutiſque, aſperæ licet & duræ palpantibus viderentur, non tamen lapidis ea erat durities, ſed corneæ aut ungulinæ, etiam ſpecie, ſuppar, quippe quæ cultro cederet ſcindique poſſet.

Poſt extractum tumulo, fodinaque corpus, diligenti examinatione quæſitum eſt, ecquis eſſet, qui agnoſcere poſſet defunctum, aut quando periiſſet, ſcire? cum *Magnus Johannis*, metallicus in *Korſgården*, probe illum a ſe de facie, quippe quæ lineamenta omnia illibata ſervet, agnoſci, idque coram conſeſſu Metallico, profiteretur; ſubjungens etiam nomen, vocatum eum ajebat *Matthiam Iſraëlis*, alias, ob proceriorem paulo ſtaturam, *Matthiam Magnum* ſeu *Procerum*, qui in Boda Swerdſiöenſis parœciæ pago editus, operam *Jonæ Petri* in *Dijkarebacken* locaſſet. Succurrit porro *Matthiam* hunc *Iſraëlis*, poſtquam anno 1670, & tempore quidem autumnali, ſolus capſula vectus deſcendiſſet in fodinam, deſideratum, dubioque procul ruina ſuffocatum fuiſſe. Dictis fidem fecere idem affirmantes *Ericus Michaëlis* Prætor Metallicus, & *Ericus Petri* Reſtiarius. His accedebat vetula, quacum vivus adhuc *Matthias* ſponſalia contraxerat, quæ, veteris & jam reviviſcentis amoris jure, exanime corpus ſibi concedendum, aut terræ ſaltem mandandum poſtulabat. Aderant & alii plures, illum qui agnoſcerent, & narrationis hujus veritatem confirmarent.

Quadraginta novem adeo annorum ſpatio, ab anno videlicet 1670. ad annum 1719. ſub terra delituerat *Matthias* hicce *Procerus*, e qua in lucem protractum, in perpetuam rei memoriam, vetus ædes fodinæ publica excepit: in qua hodieque integer, & tam quod ad veſtimentum & lintea, quam carnem, cutem, capillum, & ungues, incorruptus, fœtoriſque omnis expers, oculis intuentium ſiſtitur; ſolo aquæ vitriolo abundantis miniſterio exſiccatus, & contra putredinis vim munitus.

Ex fideli hac certaque, quantum quidem ex loci ipſius incolis hauriri potuit, notitia haudquaquam petrificatum cadaver hoc, aut in lapidem mutatum, ſed aquæ tantum vitriolo ſcatentis beneficio induratum eſſe liquet. Quid quod vitrioli naturæ & ingenio magis nil, quam ejuſmodi petrificandi vis repugnat: quippe quod nil unquam in ſaxum convertat; tenuiſſimi vero vaporis vegeto motu omnia perrumpat, ſtringat, ac a putredine interituque tueatur.

V. *Aræometri*

V. *Aræometri novi deſcriptio & uſus a D. G.* Fahrenheit, *R. S. S.*

NOTUM eſt liquorum gravitates ſpecificas, duobus potiſſimum modis, inveſtigari poſſe, librarum nempe, vel Aræometrorum ope. Ambo nonnullis difficultatibus ſunt obnoxii, quominus exacte ſæpe experimenta fieri poſſint. Difficultas prioris modi in eo præcipue verſatur, quod cum libræ accuratiſſimè elaboratæ adhiberi debeant, earum vero tenor, in quibuſdam ſolummodo punctis conſiſtat, ea temporis tractu, vel etiam alio modo, facillime depraventur, eoque modo experimenta non eâ, qua par eſt, accuratione perfici poſſint. Aræometra autem accuratiora, quorum ſtructura hucuſque innotuit, hoc præcipue laborant incommodo, quod uno eodemque inſtrumento non omnium liquorum gravitates explorari poſſint, ita ut mutatis liquoribus, etiam aliud Aræometron adhibere oporteat. His conſideratis, uſumque Aræometri perpendens, difficultates memoratas ſtructurâ ſequentis inſtrumenti tolli poſſe animadverti.

Globulo, A, Fig. 7. ſatis magno, (quo major Globulus eo melior) tubi ſibi oppoſiti CD & EF annectuntur, tubulo gracillimo EF receptaculum G additur, mediumque tubuli puncto a minutiſſimo, ſatis tamen viſibili, denotatur. Extremitas altera tubuli CD globulo B prædita eſt, qui receptaculi loco ponderi inferiori (quo nempe inſtrumentum aggravatur) inſervit. Diſtantia globuli B a centro globi A triplo major ſit, quam diſtantia receptaculi G ab eodem centro. Inſtrumento ita præparato, globulus B tantâ mercurii quantitate repletur, ut ſi Aræometron liquori leviſſimo, exempli gratia, ſpiritui vini bene dephlegmato, vel ſpiritui Terebinthinæ immergatur, illud in liquore fere uſque ad punctum a deſcendat; quo facto, tubulus prope E hermetice ſigillatur, & inſtrumentum bilance accuratiori ponderatur; eritque pondus inſtrumenti etiam ipſiſſimum liquoris ab inſtrumento deturbati pondus, utpote ſatis hydroſtatices peritis notum eſt. Si autem graviores inveſtigandi ſunt liquores, exempli gratia, aqua, lixivia, vel ſpiritus acidi, eorum gravitatis differentia invenitur, dum nempe inſtrumentum in receptaculo G tanto pondere oneratur, ut illud iterum ad punctum a ſubſidat. Hoc pondere gravitati inſtrumenti addito, illorum liquorum gravitates ſpecificæ (ſi pondera ſint minutiſſima) ſatis exacte habebuntur: & ſic de cæteris.

Dixi quod inſtrumentum in memoratis ſpiritibus fere ad punctum a ſubſidere debeat; melius enim erit, ut non perfecte liquor illud punctum attingat, & ut differentia parva ponderibus minutiſſimis adjuvetur: hoc enim modo, ſi forſan adhuc liquores leviores darentur, vel etiam ſi liquorum memoratorum gravitas a calore ſpecifice levior redderetur, adhuc inſtrumento explorari poterunt, quod alias non ſuccederet, ſi illud perfecte ad punctum a in nominatis ſpiritibus ſubſideret.

Dum autem experimenta fiunt, cavendum eſt, ne ſuperficies, tam inſtrumenti, quam liquorum aliquâ pinguedine, vel aliis particulis heterogeneis ſint imbutæ; aliter enim experimenta nunquam ſatis accurate peragentur; ut optime Vir acutiſſimi ingenii, hujuſque inclytiſſimæ Societatis Membrum, inter diſſerendum de hoc inſtrumento monuit.

VI. *A Letter from Mr.* John Bonnet, Surgeon *at* Fowye *in* Cornwall, *to* Claudius Amyand, *Eſq*; Serjeant-Surgeon *to his Majeſty*, F. R. S. *concerning the preternatural Structure of the* Pudenda *in a Woman, deſcrib'd in* Phil. Tranſact. N° 379.

AS there appears to be ſome Difference in the Accounts of the remarkable Structure of the *Pudenda*, &c. of the Woman of *Lanteglaſs*, near *Fowye*, publiſh'd in the *Philoſophical Tranſactions* N° 379. I take the Liberty of giving you a particular Account of what I obſerv'd, with reſpect to this Matter. This I am the rather embolden'd to do, as I repeatedly obſerv'd the wonderful Structure of the Parts, before the Birth approach'd, and before I was oblig'd to make the Inciſion.

This Woman was about 23 Years old when ſhe was married, and ſome time after conceiv'd. As ſhe was conſcious of the preternatural Structure of the Parts, and her Mother apprehenſive of the Danger that wou'd attend the Delivery under ſuch unhappy Circumſtances; they apply'd to me about the 7th Month, in order to engage my Aſſiſtance.

On viewing the *Abdomen*, I made the following Obſervations; There was no Sign of the *Umbilicus*; but about three Inches lower, than that is regularly plac'd in Perſons naturally form'd, there was a ſpongy, fleſhy Extuberance, nearly of the Shape and Bigneſs of an Hen-Egg: Not, as is ſaid in Mr. *O* ....'s Account, compos'd many Lobules invelop'd by diſtinct Membranes, but entirely reſembling that luxuriant Fleſh which is thrown forth in ill-digeſted Wounds, and is therefore commonly call'd proud Fleſh. This was exceeding tender, and on it ſhe cou'd not bear the leaſt Touch. On the lower Part of this Excreſence, I perceiv'd two ſmall Orifices, the one diſtant from the other about an Inch. Thro' theſe the Urine drops continually, nor is ſhe able to retain it; but by violent Efforts can make it ſpout out near a Foot. What is ſaid in Mr. *O*....'s Account of its being render'd *multis rivulis*, is certainly erroneous: The two Orifices, by which it is in deed diſcharg'd, being now very evident, and will eaſily admit a ſmall Probe.

About ¼ Inch below this Protuberance, was a tranſverſe Orifice, much reſembling the *Anus* of a Cock. Through this the *Menſtrua* regularly flow'd: By this ſhe was impregnated. 'Twas with ſome Difficulty I thruſt my Finger into this Orifice, in order to reach the *Os Tincæ*, which, however, I could by no means feel, it lay ſo deep; but I plainly felt a thick tranſverſe Membrane ſeparating this Paſſage from an Orifice, which is ſituated about two Inches below that already deſcrib'd. This lower Orifice ſeem'd to be ſituated, exactly where the *Symphyſis* of the *Oſſa Pubis* is, in Women regularly form'd, ſomewhat above the Place where the natural *Hiatus* ſhou'd have been. I cou'd but juſt enter the Tip of my Finger into this. There were a few Hairs ſcatter'd up and down irregularly about this Orifice. The *Anus* terminated as uſual, with a Sphincter, about two Inches below this lower Orifice, much more forward than uſual.

So that the upper Orifice, which may be properly call'd the Orifice of the *Vagina*, was about ¼ Inch below the umbilical Excreſcence: The lower oblong Orifice (or another Paſſage to the Womb) was about two

Inches below that of the *Vagina*: Contrary to what is ſaid in Mr. *O* . . . . 's Account, who makes the ſuperior Orifice to be near four Inches below the Excreſcence, and the lower Orifice four Inches from the ſuperior; and then ſays, the *Anus*, which he (rightly enough) makes to uſurp the Place of the *Foſſa magna* in Women naturally conformed, was five or ſix Inches below this. According to this Proportion, the Woman muſt be really gigantick; whereas ſhe is a ſhort, little Woman: But however, Worthy Sir, you may depend upon the Account and Meaſure I have given; nor do Mr. O . . . . 's Cuts any way repreſent the Caſe.

This Woman had no *Os*, or *Oſſa Pubis*; indeed there was an *Apophyſis* jutting out from the lower Part of each *Os Ilium*, but they were far from being join'd, as uſual, by *Synchondroſis*.

July 18. 1722. I was ſent for late at Night. I found the Woman with true Travail-Pains upon her. The Throws were exceſſive violent, and the continued Agony had almoſt quite exhauſted the poor Woman's Spirits; but the Orifice of the *Vagina* was no way ſenſibly dilated, tho' the *Anus*, thro' the Violence of the Throws, open'd extremely wide. In vain were all Endeavours to relieve her, by thruſting up the Child, and putting the Mother in a proper Poſture. Vain were her own Throws and Agonies. Convulſions now had ſeiz'd her, and Nature ſeem'd to have deny'd a longer Life to the Mother, or an Entrance into it to the Child.

I was in the utmoſt Perplexity what to do under theſe Circumſtances. On the one Hand I conſider'd, that if there was not a Paſſage made for the Child, and that by Inciſion, both Mother and Child muſt ſpeedily periſh. On the other Hand I foreſaw the Danger and Hazard of an Inciſion, and the unavoidable Cenſure of having kill'd the Woman, if ſhe ſhould die under the

Operation.

Operation. At laſt Humanity ſo far prevail'd with me, as to try a doubtful Method of preſerving Life, rather than none.

I told her Mother, and the other Perſons in the Room, that Death was inevitable, without making the Paſſages wider by Inciſion, and ſo attempting a Delivery; withal apprizing them of the Uncertainty of the Event of the Operation. When they ſaw her (as they thought) juſt expiring, they deliver'd her into my Hands, to do with her what I thought fit.

I immediately thruſt my Scalpel into the inferior oblong Orifice, and directly cut into the Orifice of the *Vagina*; ſo brought them into one: Then preſently with my Sciſſors, ſnipp'd the tranſverſe Membrane. This being done, I eaſily introduced my Hand, felt the Head of the Child, and with my Finger thruſt into its Mouth, drew forth a Female Infant, living and well form'd, to my own Surprize and Admiration of all. Thus was this *Gordian* Knot, at laſt, happily cut, that cou'd not be unty'd.

Ever ſince ſhe was deliver'd, in the Manner recited, ſhe hath ſuffer'd a *Prolapſus uteri*, upon the leaſt ſtanding or walking. I propos'd to remedy this by a Suture, as is practis'd in the Caſe of the *Vulva* breaking into the *Anus*; but ſhe wou'd by no means admit of it: So that ſhe almoſt continually labours under a *Procidentia uteri*, and the Body of the Womb and *Vagina* are ſo corroded by the Acrimony of the Urine, that there are four or five Ulcers form'd upon them. Beſides this Inconvenience, ſome of the thinner Parts of the Excrement are diſcharg'd at the Bottom of this large Cleft, and by introducing my Finger at the Bottom of the Cleft, I can eaſily thruſt the Top of it thro' the *Anus*: which is an Argument that there cou'd not be 5 or 6 Inches between the oblong Orifice and the *Anus*, as is aſſerted

ed by Mr. O. . . . . , the Diſtance being ſcarce above two Inches.

Fowey, June 23, 1724.

FIGURARUM EXPLANATIO.

FIGURA Octava.

A. Epigaſtrium.
B. Regio umbilicalis.
C. Hypogaſtrium.
D. Regio pubis.
E E. Spongioſa oſſa.
F F. Meatus urinarii, a partu ſatis viſu faciles.
G. Orificium tranſverſale.
H. Oblongum orificium.
II. Cicatrices duæ, ex Urinæ, ut videtur, acrimonia.
K. Anus, paululum anteriùs locatus.

FIGURA Nona.

P P. Fiſſura magna quatuor pollices oblonga.
L. Procidentia uteri.
M. Os Tincæ.

VII. *Caſus*

VII. *Caſus rariſſimus. Viſus objectorum partialis, communicante Domino* Abrahamo Vater, *Prof. Med.* Wittemb. *R. S. S.*

FOemina mediæ ætatis, anno ſuperiori, a retro-pulſa, per balneum, & ſubſecutam refrigerationem, Coryza, unica nocte incidit in cataractam nigram, ſeu guttam ſerenam: nam mane e ſomno experrecta, uſu oculorum orbatam ſe ſentit, nullo extrinſecus in oculis vitio apparente. Poſt uſurpata vero medicamenta varia laxantia & ſanguinem purificantia, interpoſitis ſimul veſicatoriis, una cum decenti Diæta, viſus ſenſim rediit, ita tamen ut notatu dignæ viſus viciſſitudines, ac ſingularia in eo phænomena occurrerent. Primum enim omnes ipſi obvios homines, capite quaſi truncatos vidit, ſolo corpore abſq; capite in oculos incurrente. Succeſſu temporis, ceſſante hac obſervatione, objecta quidem tota, ſed quaſi per nebulam aut per rete vidit, & hoc ceſſante, maculas tamen & flocculos oculis obverſari percepit. Tandem affectus hic in viſum partialem, quo hoc ipſo tempore adhuc laborat, degeneravit. Nunc enim, utroque oculo aperto, & verſus objectum directo, illud quidem totum conſpicit: alterutro vero eorum clauſo, pars objecti, macula aut nebula rotunda tecta, videtur, quæ macula tanto major apparet, quo longius oculus ab objecto removetur. Prout autem oculus ſive dexter, ſive ſiniſter, occluditur, ſemper quidem media objectorum pars, at diverſa, obſcuratur. Ita ſi, exempli gratia, has tres voces in charta ſcriptas (*Ego ſum cæca*) intueatur oculo ſiniſtro tantum, pupilla verſus mediam vocem *Sum* directa, hæc in viſum non incurrit, ſed tantum duæ voces *Ego* —— *cæca* conſpiciuntur. Pupilla vero verſus vocem *Ego* directa, hæc evaneſcit, ita ut tantum —— *Sum cæca* videat. Quod ſi e contrario oculum ſiniſtrum claudat, & oculo dextro inſpiciat objectum, tunc medium quidem pariter obſcuratur, ita tamen, ut tantum pars quarta viſum fugiat, tribus partibus clare conſpectis. Ita ſi, exempli gratia, oculo huic objiciat has quatuor voces, in charta ſcriptas, (*Ego opto eſſe ſana*) tunc pupilla ad medium directa, omnes voces videt, præter vocem *opto*, quæ macula itidem rotunda obſcuratur, & videt ægra ita tantum, *Ego* —— *eſſe ſana.* Quoniam ergo ex hoc caſu apparet, maculam illam rotundam, qua pars objecti obſcuratur, eſſe fixam quidem, ſed pro varia pupillæ directione locum mutare, dubium videtur, utrum vitium in humore cryſtallino, an in tunica retina hæreat? In tunica quidem cornea nulla obſcuritas aut macula patet. Majoris vero momenti, ac explicatu difficilius videtur antecedens phœnomenon, ubi ægra totum hominis corpus, excepto tantum capite, conſpexit. Quamobrem hunc caſum Illuſtriſſimæ Societati Regali exponendum eſſe duxi, quoniam hic viam mihi parare videtur, ut multa utilia ad viſum illuſtrandum ex eo deduci queant. Dabam Wittembergæ Saxonum D. XX. Mart. MDCCXXIV.

VIII. *Sectio Oculorum duorum Cataractâ affectorum. Epiſtola Domini* *ad* Samuelem Molyneux *Armigerum, Sereniſſimo Principi* Walliæ *a Secretis, R. S. S.*

JOhannes *Wright*, ſenex mileſque emeritus, ſuffuſione ſeu Cataractâ utriuſque oculi laborans, viſu tandem omnino privatus eſt. Curæ ſe commiſit Domini *Caywood*, Oculiſtæ *Dublinienſis*, qui acu punctoriâ oculum utrumque perforavit ad Cryſtallinum è ſede ſuâ deprimendum. Operatione verò in oculo dextro minus feliciter ſuccedente, ſiniſtri uſum ſolummodo recuperavit. Paucis menſibus poſt operationem peractam, in hoſpitium regale militum emeritorum prope *Dublinium* receptus eſt, ubi per annos octo vel novem manſit, &, per totum iſtud tempus, viſus aciem ſatis acutam habuit, adeo per urbem ſolus ambularet, & ſodalium facies diſtinguere poſſet. Mortuus eſt tandem 5° die Aprilis 1722. ſuffocatus per caſei morſulum hærentem in gula, quam adeo arctam habuit, ut eundem mortis aditum paucis abhinc annis bis ægre evaſerit. Oculi ejus, e cranio exempti, miſſi ſunt per Noſocomii Chirurgum ad D. *Thomam Molyneux* Medicinæ Doctorem, Collegii Med. Regalis in *Hibernia* Socium, & in Academia *Dublinienſi* Medicinæ Profeſſorem. In ædibus ejus cultro Anatomico aperti ſunt, per Oculiſtam ſupradictum, meque, ut intereſſem operationi, accerſere dignatus eſt. In oculo ſiniſtro (cujus aciem recuperatam fuiſſe ſupra notavimus) ſublatâ corneâ cum portione tunicæ ſcleroticæ, ne vel minimum invenimus rudimentum pelliculæ in aqueo humore fluctuantis, aut limbo iridis adnatæ, ſicut ſe obſervaſſe ſcribit *Wolhouſius*. Quod vero multo magis mirandum eſt, nullæ prorſus reliquiæ inventæ ſunt humoris Cryſtallini, cujus opacitati morbum adſcribit Doctor *Briſſeus*. Humor Vitreus, quoad ſitum & conſiſtentiam, nihil præternaturale oculis noſtris exhibuit; tunica choroidea & retina fuſci coloris erant tam intus quam extra. In oculo dextro eadem omnia obſervavimus; nullum nempe pelliculæ aut Cryſtallini rudimentum occurrebat. Tunica cornea rugoſa erat & flaccida, quod aqueus humor per puncturam extravaſatus, numquam renovatus ſit: adeoque iſtius oculi uſum amiſit penitus. Cryſtallini vero defectum quod attinet, res eſt prorſus inaudita, neque ab ullo Anatomico, quantum ſcio, defuiſſe obſervatus eſt, vel leviter ſuſpectus. Militem hunc Cryſtallinum habuiſſe in utroque oculo ante operationem, nullus dubito; cum vero acu a ſede ſua detruſus eſt, ligamentis ciliaribus vaſiſque, quibus nutrimentum hauſit, penitus diſruptis, eum ſenſim contabuiſſe credo, tandemque in auras diſſipatum fuiſſe. Summopere vellem hanc rem experimento ſæpius iterato confirmatam eſſe: ſuſpicor enim idem ſemper evenire ſtato tempore poſt depreſſum Cryſtallinum.

Hoc certo conſtat ex hac hiſtoria, hominem nempe abſque Cryſtallino videre poſſe, contra opinionem *Wolhouſii* Oculiſtæ *Pariſienſis*, eâque amplius confirmatur ſententia D. Doctoris *Briſſei* de Cataracta, quem conſulas quæſo ſuper hac re.

IX. *An Account of a Book entituled,* Dominici Bottoni, de immani Trinacriæ Terræ Motu Idea Hiſtorico-Phyſica, in qua non ſolum Telluris concuſſiones tranſactæ recenſentur, ſed noviſſimæ Anni 1717. Meſſanæ 1718 8°. *By* J. G. Scheuchzer, *M. D. R. S. S. Coll. M. L. Lic.*

THIS Treatiſe contains an accurate hiſtorical Account of the ſeveral violent Earth-quakes, which happen'd in the Kingdom of *Sicily*, in the Years 1693, 1694, and 1717. interſpers'd with ſome philoſophical Digreſſions concerning the Cauſes and Effects of Earthquakes in general. I ſhall ſomething enlarge upon the former, as being the more material, as well as the more curious Part of the Book; avoiding, however, with all poſſible Care, a tedious and unneceſſary Repetition of what hath been already mention'd in another Account of thoſe two dreadful Succuſſions, which happen'd the 9th and 10th of *January*, 1693. written by *Vincentius Bonajutus*, a *Sicilian* Nobleman. This Account was communicated to the *Royal Society* by the late learned *Marcellus Malpighius*, and is inſerted in the *Philoſ. Tranſact.* N° 207. p. 2.

The Summer Seaſon, in 1692. was exceedingly hot and tempeſtuous, with frequent Thunders, Lightnings and Rains. About the middle of *September* fell ſuch profuſe Showers, that all the Rivers and Torrents increaſed to ſuch a Degree, as to overflow their Banks in ſeveral Places, and cover large Pieces of Ground with Water. This, join'd to the continual blowing of South erly Winds, during the *Autumn*, put the Inhabitants under great Apprehenſion of future Miſchiefs. And indeed, the diſaſtrous Fate, which befel *Sicily* about the Beginning of 1693, too manifeſtly prov'd, that this ominous Fear was no way groundleſs. For on *Friday* the 9th of *January*, about the 5th Hour (according to the *Italian* Way of counting) after a warm, ſerene and calm Day, the Earth began trembling all of a ſudden, chiefly about *Catania*, and in ſome neighbouring Places, for the Time required to ſay the Lord's Prayer. This firſt Shake was accompany'd, as generally happens in Earthquakes, by a hollow, thundering Noiſe, and and ſucceeded by another ſmall Trembling, obſerv'd only by ſome few People on *Saturday*, early in the Morning. Theſe two Succuſſions, though violent enough, were but a Prelude of the third, which happen'd the 11th of the ſame Month, by 4 of the Clock in the Afternoon, of which the Apprehenſion was ſo much the greater, becauſe all the 10th and 11th, between the firſt and third Shake, the Air was more than uſual dark andcloudy. This laſt was ſtupendous beyond human Imagination, and it would be a Task too difficult for even the ableſt Pen, to deſcribe all its dreadful Effects; the violent toſſing and dancing of the Earth; the hollow, thundering Noiſe threatening the whole Iſland with its entire Diſſolution; the fiery Eruption of the burning *Ætna* throwing out a prodigious Quantity of Flames, Stones and Aſhes; the Terror and Confuſion of the diſtracted Inhabitants running up and down the Streets, uncertain where to provide for their Safety, or how to eſcape the Fury of all the raging Elements, which ſeemed to have conſpired their Ruin. There was ſcarce one Place all over the Kingdom left without ſome particular Misfortune, *Catania Syracuſa*, *Agoſta*, *Meſſina*, *Noto*, *Raguſa*, *Leontini*, *Ibla*

Chiarumonte, *Carleontino*, *Caltagirone*, *Soctino*, *Francofonte*, *Bontello*, *Militello*, *Occhiali*, *Aydono*, *Motica Maſcali*, were all, if not entirely deſtroy'd, at leaſt miſerably ſhatter'd, many Churches and ſtately Buildings, up and down the Country, violently thrown down, and above 60000 Inhabitants buried under the Ruins, of which about 16000 periſhed only at *Catania.*

In many Places the Earth gap'd prodigiouſly. Such an Opening was obſerved near *Meſſina*, in the very Bottom of the Sea; another near a Village call'd *Botto d'Aceto*, 250 Paces long, and near 8 Palms broad; another at *Caltaniſeta*, near the Jeſuits-College, 2000 Paces long and 2 Palms broad; another, at the Top of a Hill near *Leontini*, full wide enough to hold a Man; another upon the Road between *Catania* and *Leontini*, which ſwallow'd up ſome Mule-keepers, then, to their great Misfortune, happening to travel that way, along with their Mules and whole Baggage, that not the leaſt Foot-ſtep of them remain'd. Silently to paſs over a great many more, but of leſs Conſideration.

Out of all theſe Openings ſprung forth a great Quantity of Water, which drown'd the neighbouring Places. This Water was in ſome Places hot, with a ſtrong ſulphureous Smell, which laſted, even after the Earthquakes were over, and induc'd ſome of the Inhabitants, not without Succeſs, to make uſe of it in curing of Ulcers, and other cutaneous Diſeaſes, for which chiefly a hot Well near *Lazaretto* became very famous. Out of ſome of theſe Gapings of the Earth iſſued a thick Stench and Smoke, very troubleſome to all the Neighbourhood. This happen'd, amongſt other Places, upon a Mountain called *S. Theodor*, as alſo near *Mena*. Near *Agoſta* it was preceded by a ſulphureous red Flame.

Juſt at the Time of the ſecond Shock, the Sea retir'd from the Land all along the Coaſts, leaving its Bottom dry'd up for a conſiderable Diſtance, and within few Minutes return'd again with great Fury, and overflow'd the Shores. By this Accident the *Malteſe* Gallies, lying at Anchor in the Harbour of *Agoſta*, were in danger of being loſt; for the Sea ſunk down all of a ſudden, ſo that they came to ſit almoſt upon the Ground, and immediately after bubbled and ſwell'd up again with ſo great an Emotion, that they run the Hazard of having their Cables broke, and being driven away.

And it ſeemed that the Earth itſelf was in ſome Places conſiderably lower'd, and the Tops of the Mountains depreſs'd. Of this they had a remarkable Inſtance at *Paternione*. The Hills, between this City and the Shore, hinder'd it from having any View of the Sea, which ſince the Earthquake diſcovers itſelf towards the Eaſt very plainly.

In other Places the Earth actually ſunk down, and inſtead of it appear'd great Lakes, ſome of which were large enough to become navigable. By the breaking forth of ſuch a Lake between *Noto* and *Syracuſa*, a large Piece of Ground was tranſported for about 50 Paces, where it now ſtands as firm, as if it had always ſtood there.

The Looſening and Fall of two great Rocks between *Terula* and *Caſſero* is already ſufficiently deſcrib'd in the abovementioned Account of Signor *Bonajuti*: Such Looſenings and throwing down of great Rocks happen'd every where up and down in the Country, to the great Terror of the neighbouring People. The ſame was alſo obſerv'd, according to *Kircher*, and ſeveral other Authors, in ſome Earthquakes in the Kingdom of *Naples*. Two very high Rocks near *Ibla*, with all the Trees growing thereon, were by the Violence of the Fall, quite inverted, that the Top came to ſtand upon the Ground.

About two Miles off from the City of *Mena*, lies a Lake full of a bituminous, ſulphureous and combuſtible Matter, formerly famous under the Title of *Lacus Palicorum*. Upon this Lake there was obſerv'd, the very Day of the Earthquake, about a Quarter of an Hour before the ſecond Shock, a great red Flame, like a fiery Column of above three Yards perpendicular Height, which, during the Earthquake itſelf, on a ſudden diſappear'd.

At *Agoſta*, the Misfortune was ſo much the greater, becauſe, beſides all the dreadful Effects of the Earthquake itſelf, the Powder-Magazine, in the Caſtle, by ſome ill Accident, and perhaps by the violent Fall over one another of the Stones of ſome ruined Buildings (or by the breaking out of ſome ſubterraneous Flame) unluckily took Fire, by which the whole Caſtle was blown up with ſuch a Fury, that ſome of the Stones were carry'd as far as the Iſland *Thapſus*, which is near 8 *Italian* Miles diſtant from *Agoſta*. By this Accident periſh'd 1840 Inhabitants.

There remains ſtill one thing worth obſerving, and that is the very Riſe and Progreſs of this terrible Succuſſion. It aroſe in the South, and proceeded from thence towards the North. For it was firſt obſerved in the Iſland of *Malta*; then in the Southern Parts of *Sicily*; and laſt, always with ſome Difference as to the Time, in the Northern Parts of the ſame Kingdom. But the Shakes were leſs violent, the more it approach'd to the North. For the reſt, it extended itſelf ſo far, that not only the Iſland of *Malta*, but alſo *Calabria*, and ſome Parts in the Kingdom of *Naples*, participated of its Fury.

Nor was this the End of all the Miſeries which befel this noble Kingdom: for the Earth continued trembling for ſeveral Months after, during the whole Year of 1693. In the remaining Part of *January*, and from that Time to the Beginning of the Summer, the Shakes came ſtrong and thick, with hollow terrible Noiſes, and frequent Eruptions of *Ætna*. The moſt conſiderable were obſerv'd *February* the 15th in the Morning; *March* the 1ſt; *March* the 18th, by one of the Clock; *May* the 10th; *May* the 26th in the Morning. Towards the End of the Summer, the Shakes were obſerv'd to loſe a great deal of their Force, and *Ætna* to throw out Flames and Aſhes in leſs Quantity, when on the 4th of *September*, this ignivomous Mountain, having been ſome Days before very quiet, trembled and crack'd all of a ſudden, with ſo loud and thundering a Noiſe, as if ſome Thouſands of Guns were fir'd all at once. This was ſucceeded by a new Opening, about 1000 Paces diſtant from the old Mouth, out of which immediately iſſu'd a thick Stench and Smoke, follow'd by a great Flame. The ſame Mountain open'd itſelf in two other Places, with the like Noiſe, and Eruption of Smoke and Fire, the 25th of *September*, 1693, and the 1ſt of *April*, 1694. Since that Time the Shakes became viſibly weaker and weaker, and at laſt entirely waſted.

As to the Earthquake which happen'd *April* the 22d, 1717. early in the Morning, and of which the Author hath given a ſhort Account by way of Appendix; I ſhall not inſiſt upon its Effect, being much the ſame with the former, though far inferior as to the Degree of Violence.

Thus far, what I could gather out of this Treatiſe, relating to the Hiſtory of the *Sicilian* Earthquakes in 1693 and 1717. I proceed now, with all poſſible Brevity, to run over the ſeveral Obſervations, which our Author naketh about the Cauſes and Effects of Earthquakes in general: He obſerveth,

That ſuch Countries as abound much with Foſſil-coals, as *England*, ſome Parts of *Germany*, and even *Sicily* itſelf, are more ſubject to Tremblings of the Earth, than any other.

That peſtilential and other Diſeaſes, which frequently follow great Earthquakes, are rather to be aſcrib'd to noxious and infected mineral *Effluvia*, which upon ſuch an Occaſion iſſue out of the Earth through the ſeveral Openings, than to the Putrefaction of dead Bodies, which periſh'd in the Earthquake, and were buried under the Ruins of deſtroy'd Buildings.

That amongſt the Minerals hidden in the Entrails of the Earth ſome are combuſtible, ſome not; that of the combuſtible Ones, as Sulphur, Bitumen, Alum, Vitriol, Salt-Petre, &c. ſome take Fire quicker than others; that others, as Coals, are with greater Difficulty to be inflamed, but keep the Fire ſo much the longer; that the Heat and ſulphureous Smell of the Waters, which broke firſt through the Openings of the Earth, probably depends upon the Mixture and Fermentation of ſome of theſe Minerals.

That *Baccius*, *Kircher*, and others, which draw the Origin of the abovementioned ſulphureous Lake near *Mena*, and of ſeveral others in other Places from the ſubterraneous Cavities of the Mountain *Ætna*, are much in the wrong, there being not the leaſt Neceſſity of its being derived ſo far, ſince the Country every where abounds with combuſtible Minerals, which rather ſeem to entertain and nouriſh the fiery Eruptions of *Ætna*, than to be occaſion'd by them.

That the Sea's retiring from the Shore, and ſinking down, is the leſs to be wonder'd at, ſince there are many Examples in Hiſtories of whole Towns and Iſlands ſwallow'd up by it. This Misfortune befel, according to *Pliny*, *Tyndarida*, formerly a famous City in *Sicily*.

That in all probability *Sicily* itſelf, which, by the Teſtimonies of *Diodorus Siculus*, *Strabo*, *Pomponius Mela*, *Plinius*, *Solinus*, *Juſtinus*, *Seneca*, and ſeveral others, both antient and modern Authors, appears to have been formerly annex'd to the Continent of *Italy*, by a ſmall Neck of Land between the Cape of *Meſſina* and the lower Parts of *Calabria*, was ſeparated from it by ſome violent Succuſſion of the Earth.

That the ignivomous *Ætna*, far from being the Cauſe, as ſome imagine, of the Earthquakes, which ſo often deſolate the Kingdom of *Sicily*, ſeems rather to have been created by the allwiſe Providence, in order to ſecure this Iſland by its fiery Eruptions from more frequent Misfortunes.

That contrary to what *Ariſtotle* and ſome others aſſert, ſolid and rocky Places receive infinitely more Damage by Earthquakes, than looſe, ſandy Grounds. This appears by the Ruins of *Catania*, *Leontini*, *Agoſta*, *Syracuſa*, and *Noto*, which were all built upon a ſolid ſtony Ground, whereas *Meſſina*, though every where undermin'd by ſubterranean Cavities, was abundantly leſs ſhaken.

The remaining Part of the Book is employ'd about examining the Opinions of all the antient and modern Philoſophers about the Cauſes of Earthquakes, and eſtabliſhing the Author's own, which is, that the Earth is ſhaken by the Violence of ſubterraneous Fires, occaſion'd by the Fermentation of the combuſtible Minerals hid in its Entrails, and that the Effects of the Earthquakes may in all Regards be compar'd to the Effects of Mines. By the way, he obſerveth, that the Cauſes of Thunder, Lightning and Winds may be deriv'd from the ſame Principle.

FINIS.

Philoſoph: Tranſact: No. 385.

Fig: I.

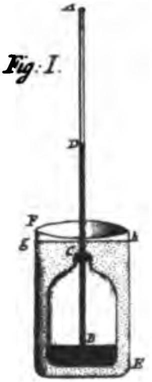


Fig: II.

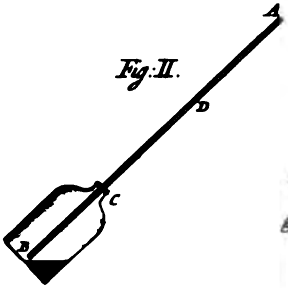
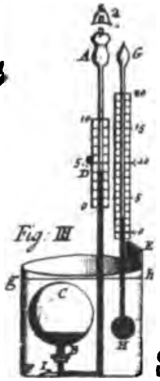


Fig: III



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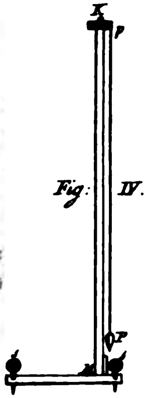


Fig. V

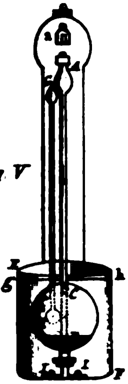


Fig: VI.

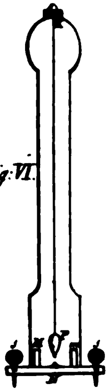


Fig: VII

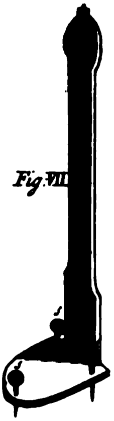
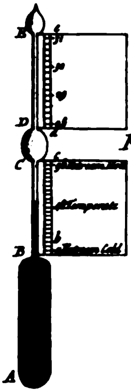


Fig: VIII



Numb. 385.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *October*, *November*, and *December*, 1724.

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I. *An Account of the* Scarabæus Galeatus Pulſator, *or the* Death Watch, *by Mr.* Hugh Stackhouſe. *Communicated by* John Harwood, *L. L. D. R. S. S.*

AS I ſtood thoughtful in my Study, on the 16th of *May*, 1724, I happen'd to hear, what is commonly call'd the *Death Watch*, very near, as I thought, to the Place where I then ſtood: This brought into my Mind, what I had formerly read in the *Philoſophical Tranſactions*, for the Month of *October*, 1698, of the *Scarabæus Galeatus Pulſator*, found, and deſcrib'd, by Mr. *Benjamin Allen*. While I was thinking upon the Account there given, I fancied I heard the Beatings ſomewhat ſtronger than before, which encourag'd me to ſearch after it; I thereupon removed my Station to another Part of the Room, and ſoon diſcover'd, that I had paſs'd the Place where the Beatings were; upon which, I concluded, that it lay ſomewhere betwixt my two Stations; I therefore remov'd, and placed myſelf about the middle Diſtance; where, though the Beatings did not repeat ſo frequently as before, yet when they did, I found I was ſtill nearer 'em. While I ſtood in this Place, I fancied 'twas over my Head, toward the Ceiling of the Room; upon this, I got upon a Chair, that had a Sedge Bottom, and ſtood to the Wall juſt before me; but ſtanding there for ſome time, I could hear no Beatings at all. I then ſtept down from the Chair, and after I had ſtood ſtill for ſome time, the

Beatings began again; this made me conjecture, that they might be about the Chair, and that my ſtanding upon it might prevent them. Upon inclining my Head down toward the Chair, I found I was ſtill nearer and nearer to the Sound, and, at laſt, placing my Ear cloſe to the Sedge Bottom of the Chair, I diſcover'd that it was ſtill further towards the Ground: Upon that, I turn'd the Chair's Bottom upwards, but heard no Beatings for a conſiderable time after; at length, they began again, and, as I caſt a diligent Eye over the Bottom of the Chair, I happen'd, at laſt, to hit luckily upon the Place where the Inſect was beating; ſo that it diſcover'd itſelf to me by its own Pulſations. I was very much pleas'd with the Diſcovery, and not only ſtood viewing it beat for ſome time, but alſo afterwards call'd up others to ſee it beat, which they did, and that, not without Admiration. The Manner of its Beating was thus.

It lifted up itſelf upon its hinder Legs, and ſomewhat extending, or rather inclining its Neck, beat down its Face upon the Sedge, with great Force and Agility; the Sedge, upon which I found it, was bared of its outward Coat, for about the Length of half an Inch; the Inſect ſtood upon the inward bulbous Part, and beat upon the outward Coat, as if it had been working it off as it went; the Impreſſions of its Strokes were very viſible, the Coat of the Sedge being depreſs'd, where it had beaten, for about the Compaſs of a ſilver Penny; whether it beat for Exerciſe, or Food ſake, I cannot certainly ſay; but very probably it might be for the latter; and I am rather inclin'd to think ſo, becauſe there were more than one ſuch Places upon the Sedge, where it had been at work, and where, 'tis likely, it might have been a Sojourner for ſome Days.

As to what the ingenious Mr. *Derham* of *Upminſter* has obſerv'd, that the Beatings are a ſort of Prelude for Copulation, I could not diſcover that this Beetle had any other of the ſame Species near it; and therefore I am inclin'd to think, that it beat for the Preparation of its Food, at this time, at leaſt, whatſoever it might do at other times, for Pleaſure. The Deſcription Mr. *Allen* gives of the Inſect, as referr'd to above, foraſmuch as I can find by this (which I took from the Chair where I found it, into a Box) is very true: 'Tis about a Quarter of an Inch in length, of a dark, dirty Colour, having a broad *Galea*, or Helmet, over its Head; under which, when quiet, it draws up its Head; ſo that this *Galea* is, when this Inſect reſts, a very notable and providential Defence againſt ſuch Falls, as are frequent in rotten and decay'd Places, in which, it ſhould ſeem, this Inſect is very much converſant. The ſecond Day after I took it, I open'd the Box wherein it was, and ſet it in the Sun: The Inſect was ſoon very brisk, and crept backwards and forwards along the Pieces of Sedge, and rotten Wood, that I had put with it into the Box, till at length getting to the End of one of the Pieces, it immediately ſtruck out its Wings, and was juſt going to take its Farewell; but having the Lid of the Box ready in my Hand, I ſhaded it over, and it ſoon drew in its Wings, and was very quiet. I could not before perceive, though I had the Uſe of a tolerable good Glaſs, any the leaſt Sign of a Fiſſure upon its Back; and, for that Reaſon, did greatly queſtion, whether it had any Wings or not, till I ſet it, as above, in the Sun. The Head of the Inſect appears to be of a very fine Contexture, as it is ſeen, when it creeps about, and ſtretcheth it forward; but when 'tis drawn up under its *Galea*, it ſeems to be cover'd with a Membrane thick ſet with fine Hairs.

It lived with me about a Fortnight, but I could never perceive that it beat, after it was confin'd in the Box.

II. *Obſervations of the Eclipſes of the firſt* Satellite *of* Jupiter, *communicated by his Excellency* William Burnet, *Eſq; Governor of* New York, *F. R. S.*

Theſe Obſervations were made in the Fort of *New York*, for determining the Longitude of that Place by us,

William Burnet, *Cadwallader Colden*,
James Alexander, and calculated by
Cadwallader Colden.

The Latitude of the Fort, was formerly determin'd to be 40° 40′.

Auguſt the 9th, 1723.

	H.	′	″
TIME of Emerſion at *London*, according to Mr. *Pound*'s Tables, reduced to apparent Time	16	09	25
Time as it was ſeen at *New York*	11	10	43
Difference of Meridians	4	58	42

I neglected to write down the Altitudes which were taken of the Sun, for correcting the Clock.

Auguſt the 25th.

	Altitude of the Sun's Upper Limb.			Time by the Clock.			Time by Calculat.		
	°	'	"	*H.*	'	"	*H.*	'	"
Sun's Declin.	49	30	00	10	17	52	10	17	28
6° 55'	51	13	30	10	33	10	10	32	8
Aug. 26. Sun's Declin.	46	24	00	9	57	40	9	56	25
6° 33'	47	50	00	10	8	22	10	6	57

	H.	'	"
Time of Emerſion by Mr. *Pound*'s Tables	14	31	25
Equation of Time to be added	00	01	22
	14	32	47
Time obſerv'd by the Clock	09	35	14
The ſame corrected	09	34	14
The Difference of Meridians	04	58	33

This I look upon as the moſt diſtinct and beſt Obſervation.

September the 10th,

	Altitude of the Sun's Upper Limb,		Time by the Clock.			Time by Calculat.		
	°	'	*H.*	'	"	*H.*	'	"
Sun's Declin.	33	21	09	01	00	09	00	16
49'	34	06	09	06	01	09	04	49
Sept. 17th Sun's Declin.	17	17	04	21	40	04	21	44
1° 54'	15	15	04	33	05	04	32	47

Time

	H.	′	″
Time of Emerſion by the Clock *September* 10th	08	00	10
Time of Emerſion by Mr. *Pound*'s Tables	12	50	36
Equation of Time to be added	00	06	54
	12	57	30
Corrected Time at *New York*	07	59	08
Difference of Meridians	04	58	22

June 26th, 1724.

	Altitude of the Sun's Upper Limb. °	′	Time by the Clock. H.	′	″	Time by Calculat. H.	′	″
June 20th, Sun's Declin. 23 . 7	56	44	09	48	03	09	43	37
	60	27	10	09	40	10	05	05
June 27th. Sun's Declin. 22 . 26.	63	31	10	27	43	10	27	05
	65	21	10	40	00	10	39	27

	H.	′	″
June the 26th, Time of Immerſion by the Clock - - - -	11	41	12
Time of Immerſion by Mr. *Pound*'s Tables	16	43	02
Equation of Time to be ſubtracted	00	04	26
	16	38	36
Time at *New York* corrected	11	40	15
Difference of Meridians	04	58	21

The Mean of all theſe Obſervations is 4h 58′ 30″ which agrees to 3″ with that Obſervation which I thought the moſt exact, and therefore the Longitude of *New York*, is nearly 74° 57′ 30″ West from *London*.

The Variation of the Magnetick Needle was obſerv'd, this Year, to be 7° 20′ Weſt. *Philip Wells*, Surveyor General of this Province, in the Year 1686, obſerv'd it to be 8° 45′; by which, it appears to decreaſe about 1° 25′ in 38 Years, or a little more than two Minutes in a Year.

III. *A New Contrivance for taking Levels, by the Reverend* John Theophilus Deſaguliers, *L. L. D. R. S. S.*

THAT the Air Thermometer is alſo a Barometer, has been obſerv'd long ago; and, becauſe the Liquor in it will riſe and fall, as well by the Change of the Weight of the Air, as by the Air's Rarefaction by Heat and Cold, this Inſtrument has no longer been made uſe of as a Thermometer, and, in its ſtead, Spirit of Wine Thermometers, hermetically ſeal'd, have been us'd ever ſince.

But, becauſe the Errors of the Air Thermometer (or its Difference from the Spirit Thermometer) depend only upon the Change of the Weight of the Atmoſphere from what it was, when the two Thermometers were ſet at the ſame Degree of their reſpective Scales; the late Dr. *Hook* contriv'd an Inſtrument, that he call'd a Marine Barometer, made of a Combination of the two abovemention'd Thermometers; in ſuch Manner, that a third Scale being made uſe of, to obſerve the Difference of the two Thermometers, thereby the Change of the Air's Gravity, and conſequently Storms, Rains, and fair Weather, might be foretold at Sea, where the Quickſilver Barometer becomes uſeleſs by the ſhaking of the Ship.

Dr. *Halley*, ſome Years ago, publiſh'd two Tables to ſhew, how much the *Mercury* in the Barometer wou'd ſubſide, when the Inſtrument is carried up to determinate Heights, above the Level of the Place where the firſt Obſervation was made; but as he makes only one Tenth of an Inch of Fall of *Mercury*, to correſpond with an Height of 90 Feet (which Height is rather of the leaſt) it is evident, that only very high Hills and Mountains can have their Heights determin'd by this Method. The ſame learned Profeſſor has lately, in the *Philoſophical Tranſactions*, propoſed Mr. *Patrick's* pendent Barometer for taking the Level of diſtant Places, becauſe the *Mercury*, in the Tube of the ſaid Barometer, does ſometimes riſe and fall a Foot, or a Foot and an Half; if therefore the Motion of the *Mercury* in this Barometer, be five times more ſenſible than in the common one, a Tenth of an Inch of Fall of the *Mercury*, will anſwer to an Height of 18 Feet; and therefore ſuch an Inſtrument might be of Uſe in taking the Levels of diſtant Places. But I know by many Experiments, that this won't anſwer in Practice; becauſe as the Tube of ſuch a Barometer is of a very ſmall Bore, the Attraction of Coheſion, whereby the *Mercury* is apt to adhere to the Tube, will diſturb the Motion of the *Mercury* caus'd by the different Preſſure of the Atmosphere; ſo that ſetting up this Barometer ſeveral Times ſucceſſively in the ſame Place, it will often differ a Tenth of an Inch, or more; and if it be ſhaken, as is commonly done to ſet it right, the *Mercury* will ſometimes part, and a Drop of it fall from the reſt; ſo that it is leſs to be depended upon for this Uſe, than the common Barometer.

Mr. *Stephen Gray* has often made a very ſenſible Barometer in the following Manner. Into a Bottle C B, (Fig. I.) he fixes a Tube A B, of a very ſmall Bore, open at both Ends, and cemented tight to the Neck of the Bottle at C; then having warm'd the Bottle with the Hand to drive ſome of the Air out of it, he immerges the End A into Water, ting'd with *Cochineal*; ſo that as the Air cools in the Bottle C B, ſome of the red Water is forc'd into the Bottle; then ſetting the Bottle upright again, as in the Figure, the Liquor in the Bottle will ſtand at B, (above the End of the Tube) and that in the Tube at D; but if it ſhou'd ſtand higher or lower than D, it may be brought to that Place by ſucking or blowing at A. The Inſtrument, thus prepar'd, if it be firſt ſet on the Ground, and a ſpringing Ring of fine Wire ſlipp'd on the Tube down to D, by Way of Index, and then ſet upon any Table, or other Place, ſcarce a Yard higher, one may obſerve that the Liquor is riſen ſenſibly. I have ſeen it riſe a Quarter of an Inch, when the Bottle was ſet but a Yard higher than where it ſtood before; ſo that the Column of Atmoſphere, that preſs'd down the Tube, whilſt the Machine was on the Ground, being ſhorten'd only three Feet, was ſo overbalanc'd by the Expanſion of the Air in the Bottle at B, that the Liquor roſe a Tenth of an Inch above D. There is, indeed, a great Uncertainty in this Inſtrument; for ſince it is a Thermometer, as well as a Barometer, the Warmth of the Hand that touches it, or even comes near it, will make it riſe, if the Air in the Bottle was cold before. Mr. *Gray* therefore contriv'd to put the Bottle C B, into the Veſſel F E, which he fill'd with Sand; that in raiſing the Inſtrument, and moving it up and down, the Air in C B might continue in the ſame State, and the Machine be only a Barometer during the Experiment.

This ſeems to bid fair for an Inſtrument, whereby the different Levels of Places may be taken; but upon a nice Examination, it will be liable to Error. For, in the firſt Place, tho' Sand is not alter'd in its Heat or Cold ſuddenly; yet in two or three Hours, as it is carried into a warmer or a colder Place, it will become hotter or colder, and the leaſt Degree of Heat or Cold, communicated to the Air C B, will alter the Height of the Liquor at D, when the Inſtrument is made ſo ſenſible as I have mention'd. Then if, in carrying the Inſtrument, it ſhould be accidentally inclin'd (as in Fig. II.) ſo that the Liquor in the Bottle ſhould not cover the Bottom of the Tube at B, ſome Liquor may fall out of the Tube at B, or ſome Air may get into it: each of which Accidents, will quite ſpoil the Experiment. But if this Machine be made portable, without any Inconveniency, and be ſecur'd againſt the Action of Heat and Cold (or, which is the ſame, if the Alterations by Heat and Cold be exactly allow'd for) it will be of very great Uſe and Certainty, in taking the Levels of diſtant Places, provided they be not ſo far diſtant from each other, that it requires above ſix Hours Time to carry the Inſtrument from one Place to another; nay, very diſtant Places, even at two or three Days Journey from one another, may be taken tolerably well with two Inſtruments, nicely adjuſted to each other, if they be taken notice of by two Obſervers at the ſame Hour, in fair Weather, and when there is no Wind.

Now ſuch an Inſtrument, I hope, I have contriv'd, whereby the Difference of Level of two Places, which could not be taken in leſs than four or five Days with the beſt Teleſcope Levels, may be taken in as few Hours.

To the Ball C (Fig. III.) is join'd a recurve Tube B A of a very fine Bore, with a ſmall Bubble at Top at A, whoſe upper Part is open. It is evident from the Make of this Inſtrument, that if it be inclin'd in carrying, no Prejudice will be done to the Liquor, which will always be right, both in the Ball and the Tube, when the Inſtrument is ſet upright. If by Heat, the Air at C be ſo expanded, as to drive the Liquor to the Top of the Tube, the Cavity A will receive the Liquor, which will come down again and ſettle at D, or near it, according to the Level of the Place where the Inſtrument is, as ſoon as the Air at C returns to the ſame Tenor in reſpect to Heat and Cold. To preſerve the ſame Degree of Heat, when the different Obſervations are made, the Machine is fix'd in a Tin Veſſel F E, fill'd with Water up to *g h*, above the Ball ; and a very ſenſible Thermometer has alſo its Ball under Water, that one may obſerve the Liquor at D in each Experiment, when the Thermometer ſtands at the ſame Height as before. The Water is pour'd out, when the Inſtrument is carried, which one may do conveniently by means of the wooden Frame of Fig. IV. which is ſet upright, by means of three Screws, ſuch as *s*, and a Line and Plummet *p* P. The back Part of the wooden Frame is repreſented by Fig. VI. where, from the Piece at Top K, hangs the Plummet P, over a Braſs Point at N: M *m* are Brackets to make the upright Board K N continue at Right Angles with the horizontal One at N. The VIIth Figure does likewiſe repreſent the wooden Frame and Screws. The Vth Figure repreſents the Machine ſeen in Front, ſuppoſing the Forepart of the Tin Veſſel tranſparent. And here the Braſs Socket of the recurve Tube, into which the Ball is ſcrew'd, has two Wings at II. fix'd to the Bottom, that the Ball may not break the Tube by its Endeavour to emerge, when the Water is pour'd in as high as *g h*.

After I had contriv'd the Machine, as above mentioned, I conſider'd, that as the Tube is of a very ſmall Bore, if the Liquor ſhou'd riſe into the Ball A, in carrying the Inſtrument from one Place to another, ſome of it wou'd ſtick to the Sides of the Ball A, and that upon its Deſcent in making the Experiment, ſo much might be left behind, that the Liquor wou'd not be high enough at D, to ſhew the Difference of Level; therefore, to prevent that Inconveniency, I have contriv'd a blank Screw to ſhut up the Hole at A (Fig. IV. and V.) as ſoon as one Experiment is made, that in carrying the Engine, the Air in A may balance that in C, ſo that the Liquor ſhall not run up and down the Tube, whatever Heat and Cold may act upon the Inſtrument, in going from one Place to another.

Now, becauſe one Experiment being made in the Morning, the Water may be ſo cold, that when a ſecond Experiment is made at Noon, the Water cannot be brought to the ſame Degree of Cold that it had in the Morning; therefore in making the firſt Experiment, warm Water muſt be mix'd with the Cold; and when the Water has ſtood ſome time, before it comes to be as cold, as it is likely to be at the warmeſt Part of that Day; obſerve, and ſet down the Degree of the Thermometer, at which the Spirit ſtands; and likewiſe the Degree of the Water in the Barometer at D; then ſcrew on the Cap at A, pour out the Water, and carry the Inſtrument to the Place whoſe Level you wou'd know; there pour in your Water, and when the Thermometer is come to the ſame Degree as before, open the Screw at Top, and obſerve the Liquor in the Barometer.

My Scale, for the Barometer, is ten Inches long, and divided into Tenths, ſo that ſuch an Inſtrument will ſerve for any Heights not exceeding ten Feet, each

Tenth of an Inch anſwering to a Foot of Height. *NB.* I have not made any Allowance for the Decreaſe of Denſity in the Air, becauſe I don't propoſe this Machine for meaſuring Mountains (tho' with proper Allowance for the decreaſing Denſity of the Air, it will do very well) but for Heights to be known in Gardens, Plantations, and the Conduct of Water, where an Experiment, that anſwers to two or three Foot in a Diſtance of twenty Miles, will render this a very uſeful Inſtrument.

IV. *Inteſtinum Parturiens, ſeu Caſus oppido rarus de cranio & coſtis Fœtus per anum ejectis, deſcriptus a* Johanne Lindeſtolpe, *M. D. & Reg. Colleg. Medic. Stockholmenſis Aſſeſſore. Ex Actis Literariis Sueciæ, An.* 1723. *Trimeſtr.* 1.

J*Ohannes Heſſelius*, M. D. & Weſtro-Gothiæ Medicus provincialis, Vir ſane doctiſſimus, & Hiſtoriæ Naturalis, ut Pliniana utar phraſi, peculiariter curioſus, ſcripſit ad me Scaris d. 20 Sept. præterlapſi anni, tranſmiſitque literas *D. Bernhardi Shiever*, Chirurgi munere fungentis in Legione Roſenſtierniana, ad Magiſtrum Equitum Nobiliſſimum *D. Cederberg* perſcriptas, in quibus refert caſum, in hiſce ſane terris rariſſimum, quod mulier quædam, poſtquam per ſeptem menſes gravida fuiſſet, evaneſcente tumore omni abdominis, per anum peperit cranii humani particulam, duaſque coſtas. Sed cum multa deſiderari viderentur, quæ lucem fœnerari poſſent tam miro phænomeno, dignum putavi, amiciſſimum *Heſſelium* per literas denuo compellare, vellet ulterius in eam rem inquirere, mihique communicare, quo poſſem tam mirandam rem ceteris meis inſerere obſervationibus, quas prelo paratas dudum habeo. Quod & factum eſt. Chirurgi vero relatio, ex Germanico in Latinum tranſlata, ſic ſe habet:

“ Uxor *Laurentii Skallroth*, Paſtoris in *OEſtad*, Territorii *Able* dicti, ditionis Elfsburgenſis, ætatis 41. quatuor vero annos nupta, A. 1720, menſe Julio, ingravidatur, fructumque fert per ſeptem menſes, viſis tamen interdum paucis menſtruis, ſed ſperans, juſto demum tempore & poſt decimi menſis faſtidia, pulcra prole parentem ſe futuram. Præterlapſo hoc tempore, evaneſcere tumorem ſentit, remanente ſolummodo preſſione quadam in latere dextro. Poſt menſem rurſus fit gravida, peperitque filiam, ſed mortuam juſtæ magnitudinis, in Decembri A. 1721. Ex quo tempore lecto adfigitur in menſem uſque Junii hujus anni. Menſe vero Maii, cum alvum levaret aliquando, anus ipſi tam dolere, ut ipſum inteſtinum rectum omnino excidere crederet: duobus ergo digitis prætentans & ſe juvare volens, extrahit cranii partem, magnitudine nummi Coronæ Svethicæ (vulgo *dubbel Carolin*); repertis deinde in ſella duabus coſtis, & poſt 14 dies abeuntibus eadem via reliquis oſſibus, colore excrementitio, quæ & ipſe vidi, & manu mea contrectavi. Maritus, uxor ipſa, & ejus ſoror, alia non via exiiſſe hæc oſſa mihi ſancte adfirmarunt, incredulo licet, & contra ordinem naturæ hæc fieri non poſſe, ſtrenue defendenti. Bene nunc valet, domeſticis apta curis, triumque mater liberorum, qui adhuc in vivis ſunt: menſtruis quoque naturaliter ſe habentibus.” *Ablingsâhs* die 28 Octobris A. 1722.

Exercuit jam dudum, & exercebit, ni fallor, hic caſus Medicorum ingenia, in diverſas forte abeuntium ſententias. Si enim fœtus in ipſo conceptus fuiſſet utero, ibique adcreviſſet, non niſi per ruptum uterum, ruptumque inteſtinum egredi potuiſſet. Sed quomodo tantum vulnus intra temporis tantillum fuiſſet ſanabile? Oſſeas aliquando fœtus particulas per apoſtema prorupiſſe, res ſatis clara eſt; interdum per umbilicum, quod vidit Albucaſis, *Chirurg. Lib.* 2. *cap.* 76. & Marcellus Donatus *Hiſt. Med. Mirab. Lib.* 4. *cap.* 22. *p. m.* 438, 439; interdum per Hypogaſtrium, ut ſcribit ad Segerum Wepferus; interdum quoque, uti nunc refertur, per inteſtinum rectum, ſicut annotavere Marſilius Cognatus, *Var. Obſ. Lib.* 4. *cap.* 9, & Joh. Langius, *Epiſt. Med. Lib.* 2. *Ep.* 39. *p. m.* 739. vir ſane doctiſſimus & candidus. *

Rumpi quoque parietes uteri, infantemque in abdomen extrudi, vel totum, vel quoad partes, teſtes fuere oculati, Hildanus *Obſ.* 64. *Cent. I.* Veſlingius *Syntagm. Anat. cap.* 7. & Joh. Caſp. Fauſius in Epitaphio Eliſabethæ Treipſenning, Petri Gilfeldii uxoris, quæ fœtum maſculum non in mundum, ſed in ſemet ipſam, rupto utero, edidit; Heidelbergæ 1657. Conf. Bohnius *Circul. Anat. Progymnaſm, I. p. m.* 20. & 21

Marquardus *Empir. Pract. p.* 304. 305. vidit molam in vaſis ſpermaticis Catharinæ Zarthin juxta uteri fundum genitam. Et Riolanus *Lib.* 2. *Antropograph. cap.* 35. quatuor producit exempla fœtuum ex Tubis Fallopianis exſectorum. In quam ſententiam multis argumentis ivit Celeberrimus Thomas Bartholinus in *Epiſtola* ad Horſtium, & Antonius Deuſingius, Profeſſor olim Academiæ Groningenſis. Hærvæus præterea de *Gener. Animal. Exerc.* 65. animantia vivipara, non in utero, ſed in cornubus concipere, ac uterum ipſis propylæum eſſe, tubas vero muliebres, cornubus aliorum animalium proportione, ſitu, connexione, amplitudine, ſimilitudine, perforatione & officio, reſpondere teſtatur. Ita ut nullum ſit dubium, fœtum extra uterum in tuba generari poſſe, ibique grandeſcere, hinc in abdomen delabi, & qua data porta excerni; quamvis hoc negent Guido Patinus, Conringius, aliique.

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* Conferri meretur *Hiſtoire d' un Fœtus humain tirè du ventre de ſa mere par le fondement* par *Monſ. LITTRE*, inſerta Commentariis Reg. Academiæ Scientiarum Anni 1702. p. 234. ſq. edit. Pariſ.

Taceo nunc Lithopædion Senonenſe, a Columba Chatry, Sartoris uxore, per 28 annos utero circumlatum, * quo nunc Regium Muſæum Hafnienſe gaudet: nominaſſe ſaltem contentus Fœtum Muſſipontanum *Pont. a Mouſſon.*) extra uterum inventum, ſemilapideum, ab Illuſtriſſimo Angliæ Cancellario Kenelmo Digbæo apud Pharmacopæum Barbilottum, aliiſque olim viſum, & publica Diſſertatione in Academia Dolana, ſub Præſidio Stephani Willet, ventilatum; quæ omnia prolixius apud Strauſſium de *Fœtu Muſſipontano* legi poſſunt. Namque quod Pentzius, Fehrius & Academia tum putabat Altdorfina, non fœtum, ſed ſororem fuiſſe, a phantaſia matris olim gravidæ progenitam, nihil moror.

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* Non ita pridem *GEORG. FRIDER. ORTH* Ratiſp. *hiſtoriam* exhibuit *Fœtus in utero materno per XLVI annos detenti*, in Diſſert. Inaugur. ſub præſidio *Rudolfi Jacobi Camerarii* habita Tubingæ, 1720.

V. *Obſervatio circa Lumen Boreale d.* 20. *Sept. An.* 1717. *prope* Upſal. *Per* E. J. Burman. *Ex Actis Liter. Sueciæ An.* 1724. *Trimeſtr.* 3.

CHaſmata, uti vocantur, cœli, quæ alias horizontale lumen & aurora ſeptentrionalis audiunt, Suethice *Nord-skjen*, *Nord ljus*, *Nordblyſs*, *Nordblâſs*, *Lâtterskjen*, *Lyſſnor*, *&c.* illæ, vulgi judicio, acies, ſeu exercituum præliorumque ideæ, Meteoron in regionibus noſtris, aliiſque Polo vicinioribus, illuſtre ſane & frequens (frequentius hodie quam olim nobis ætate provectiores perſuadere volunt) juſta cum diligentia ſæpius obſervavimus. Obſervationes omnes recenſere longum; in publicum edere, citra chalcographi opem, vel impoſſibile vel fruſtra eſſet. Sed fertur alicubi Virum Mathematica doctrina atque experientia clariſſimum phænomeni hujus hiſtoriam parare: quod ſi rem ipſi gratam nos præſtituros conſtaret, ſingulas rite delineatas deſcriptaſque ipſi lubentes communicaremus. Communicabimus tamen interim unicum, quod A. 1717. d. 20. Septembris in nocturno itinere accuratius conſiderare licuit, & cujus rarior planeque ſingularis facies conjecturam de natura phænomeni paraſtatica eruditorum examini accuratiori heic ſubjiciendi anſam dedit.

Erant ſolito plures phaſmatum horuncce tractus, arcus nimirum albicantes & reliquo cœlo (ſereno utique ac tranquillo) lucidiores, quatuor ad minimum aut tres, mediocribus tenebrarum intervallis diſtincti, & unus ſupra alterum poſiti. Quod autem rariſſimum adſpectuque jucundum, dictorum arcuum diſtantiæ ſub ipſa Cynoſura maximæ (infra quam ſupremus quidem ultra viginti & ſex gradus non conſiſtebat, quippe per quem ſtella Urſæ majoris Dubbe leviter ſubinde tranſparebat) verſus horizontem utrinque ſenſim decreſcebant, donec illi tandem in ipſis orientis occidentiſque cardinibus mutuo ſeſe interſecarent, haud ſecus quam in artificiali globo Meridiani ad Polos Æquatoris convergere cernuntur. Candidi iſti arcus ſeu ſemicirculi maximam partem ex ſtriis diſtinctis & ad horizontem normalibus conſtabant, præſertim in ſummitate, vel ſub ipſo ſeptentrione; ad latera lux debilior conſpiciebatur ac ſubobſcura.

Striarum geminus erat motus, horizontalis unus, alter perpendicularis: hic minor longe & tardior, ut vix circularem arcuum formam turbaret; ille varius ſimul atque celerrimus, ab oriente in occidentem, & retro. Quoties autem ſtriæ plures (quod ſæpius accidebat) a contrariis venientes plagis ſibi invicem occurrebant, ſive id in medio arcu, ſive alibi fieret; toties, quaſi ex illa radiorum mixtura ſeu multiplici interſectione proveniens colorum pulcherrima apparebat varietas, & quidem ordine prorſus eodem atque in priſmate vitreo, explicatis ſolique obverſis avium minorum pennis, aliiſve corporibus ſimilibus colores produci ſolent.

Hinc vero dari occaſio poterit adhuc de Opticis phænomeni rationibus, cum *Carteſio in de Meteoris Cap. VII. §.* 18. cogitandi; ſed neque tamen ideo ſubtiliori materiæ ſulphureæ in regione aëris inferiori accenſæ omnem denegari poſſe locum exiſtimamus. Ipſi enim alias ſæpius, præſertim in chaſmate A. 1716. d. 17. Martii heic longe illuſtriori quam in Anglia, Gallia, Germania, alibi, per totam noctem viſo, colores multo plures, necnon ſuſurros & ſibilos, quales excitari a focali flamma ſolent, obſervavimus. Forſan autem duo diverſa ſtatuere luminis borealis genera oportet: unum meteoron igneum ab effluviis & exhalationibus; alterum mere paraſtaticum, ex diverſimoda ſolarium radiorum refractione & reflexione, ſive in glacialibus quibuſdam lamellis, ſtelluliſve atmoſphæræ regionem excelſiorem occupantibus facta, ſive etiam in maribus quibuſdam ad ſeptentrionem, indeque nobis ex nubibus communicata, ortum. Certe poſterius ſingulari experimento illuſtrari poſſe videtur, quod occaſione jam deſcripti phaſmatis (cui tamen ſimile vidimus A. 1716. in Februario hora veſpertina 9. ex duobus ejuſmodi arcubus ſemicircularibus, ſed minus ſtriatis & ſupra borealem horizontis plagam elevatioribus, conſtans) inventum factumque, huc redit.

Si lamina ſumatur ſtannea longitudinis latitudiniſque arbitrariæ, eademque acuto & fortiori cultro, uno ductu ſecundum longitudinem univerſam, donec tota ſtriata facta fuerit, raſa, manu ita teneatur, ut ejus planum cum lucente candela & obſcuro pariete tabulave æquales faciat angulos; deinde autem variis modis incurvetur & torqueatur, ipſam nunc concavam nunc convexam parieti vel tabulæ obvertendo, tardius ad lubitum aut celerius: phaſmata ſupra recenſitis admodum ſimilia ſpectaculo non injucundo repræſentabuntur.

Quid ſi utriuſque generis lumen forte aliquando una exiſtere, atque unum cum altero coincidere dicamus, ut neutrum alterius cauſſa ſit aut effectus, ſed ambo ad noctem illuminandam terroremque ſpectatoribus incutiendum concurrant? Quemadmodum enim ſæpiſſime quidem lumen horizontale, Zonas videlicet candentes, nunc nudas (imo per ipſum fere Zenith tranſeuntes, quas cum Galaxia, utut non parum latiori, ob ſimilitudinem vulgus confundere ſolet) nunc columnis, pyramidibus inverſis aliiſque figuris variis ſtipatas, ſed citra omne aëris, ut ita dicam, incendium; ita nec raro hoc ſine illis vel antecedentibus vel concomitantibus, apparentiis diſtincte notatis, vidimus: quamvis etiam e regione quadam cœli, nude primum candente, tandem ferventiſſimas faces, ſive per ſolis radios in glacialibus Oceani partibus, ceu in ſpeculis quibuſdam cauſticis, reflexos, ſive alio quocunque modo accenſas, ad Zenith & ſuper totum nonnunquam hemiſphærium evolaſſe fatendum ſit.

Sed quia genuinas veraſque phænomeni hujus admirandi cauſſas vix cuiquam certo invenire prius licet, quam plurimarum in diverſis terræ locis unâ habitarum obſervationum rite inſtitui queat comparatio; unde ante omnia conſtet, num lumen iſtud in remotioribus etiam locis ſub eodem altitudinis angulo conſpiciatur; num quod heic horizonti parallelum, alibi verticale ſit, & id genus alia; verbo, utrum unus idemque ſit arcus qui in diverſis locis conſpicitur, an quemadmodum in Iride, ita quoque heic, quot in terra ſpectatores, tot arcus in cœlo: Enixe proinde omnes in univerſum atque ſingulos rogamus, quibus rerum naturalium in aliquo pretio eſt ſcientia, velint ubicunque terrarum, maxime vero in regionibus borealioribus, boreali huic lumini quoad omnes circumſtantias obſervando quam diligentiſſime invigilare, ſuaque obſervata quantocyus cum publico vel ſaltem nobiſcum communicare, gratiam ab erudito orbe ſane maximam merituri. Nos alia occaſione quaſdam regulas ſeu harum obſervationum normam & exemplar dabimus, parati interim & ipſi aliorum monita grato excipere animo, & quænam judicaverit quiſque potiora hujus negotii momenta, fieri certiores.

VI. *Ba-*

VI. *Barometri novi deſcriptio, a* D. G. Fahrenheit, *R. S.* S.

IN recenſione experimentorum nonnullorum circa ebullitionem quorundam liquorum a me factorum mentionem feci, gradum caloris aquæ ebullientis termino tunc memorato, 212 nempe graduum coerceri; poſtea variis obſervationibus atque experimentis edoctus ſum terminum hunc, manente eadem atmoſphæræ gravitate, ſatis fixum eſſe, ſed variante gravitate atmoſphæræ hunce terminum quoque diverſimode variari poſſe. Experimenta in hunc ſcopum jam facta nunc quidem traderem, ſed quoniam adhuc de certis circumſtantiis edoctus eſſe velim, hinc recenſionem illorum uſque in aliud tempus reponam, & interea ſolummodo thermometri alicujus mentionem faciam, quod forſitan ſi non magis, tamen æque idoneum erit ad explorandam atmoſphæræ gravitatem, quam barometrum. Hujus delineatio in Figura VIII. eſt appoſita.

Cylindro A B annectitur tubus B C, cui additur globulus oblongus C D, & huic tubulus gracillimo foramine præditus D E. Cylindrus liquore quodam, qui calorem aquæ ebullientis perferre poteſt, replebitur. In tubulo B C, gradus caloris in aere obvii menſurabuntur ope ſcalæ affixæ *b c*. Si autem thermometrum hocce aquæ bullienti imponatur, liquor thermometri non ſolum globulum CD implebit, ſed etiam uſque ad terminos varios tubuli D E aſſurget, ſecundum gradum caloris, quem aqua tempore experimenti a gravitate atmoſphæræ acquiſitura eſt. Ita, ſi, exempli gratia, tempore experimenti altitudo mercurii in barometro ſit 28 pollicum Londinenſium, liquor in hocce thermometro attinget infimum locum in tubulo D E ; Si vero gravitas atmoſphæræ æquipolleat altitudini mercurii triginta & unius pollicum, liquor a calore aquæ ebullientis uſque ad locum ſupremum tubuli D E attolletur, termini varii autem caloris aquæ ebullientis non gradibus, ſed illorum loco numeris digitorum, quibus altitudo mercurii in barometris vulgo menſuratur, ope nempe ſcalæ additæ *de* denotabuntur.

VII. *Obſervatio Lunaris Eclipſis habita* Ulyſſipone *in Palatio Regio Die* 1. Novembris 1724. *A PP.* Joanne Baptiſta Carbone, & Dominico Capaſſo, *Soc. Jeſu. Communicante Excellentiſſimo Domino, D*^{no} *de* Galvaon, *Commendatore* Villæ Meam & Francæ, *Equ. Ord. Chriſt. Legato Sereniſſ. Reg.* Portugall. *ad Sereniſſ. Regem* Magn. Britan. *S. R. S.*

Obſervavimus hanc Eclipſim Teleſcopiis, altero quidem Pedum Pariſinorum 8. ſed clariſſimo, altero 10. ſed minus claro: utroque tamen lunares maculæ perfectiſſimè diſcernebantur. Ad temporis dimenſionem uſi ſumus Horologio oſcillatorio, vulgo *Pendula*, ſatis exacto, pluribus ante diebus in ipſo obſervationis loco firmato, & quotidiano examine per meridianam lineam, ibidem a nobis jamdiu inventam, & pluries examinatam, ad medium Solis motum quam proxime reducto. Nocte vero ipſius Eclipſis ter illud ad trutinam revocavimus, ut ejus a vero tempore diſcordiam deprehenderemus. Primo in tranſitu *Fomahantis* per Meridianum, Hor. 8. M. 17. Sec. 18. Secundo in tranſitu *Rigel* ſeu *Pedis Lucidi Orionis*, Hor. 2. M. 35. Sec. 21. Tertio in tranſitu *Sirii*, Hor. 4. M. 7. Sec. 40. (Aſcenſiones rectæ deductæ ſunt ex Tabulis Hirianis.) Invenimus autem Horologium tardius incedere ſecundis tantum 7. quæ jam addita ſunt momento Obſervationis mox apponendæ.

A Solis Occaſu uſque ad Mediam Noctem Nubes ac Pluviæ Cœlum nobis identidem adimebant. Ventus tamen ſub horam 1. illud nobis ſatis clarum reſtituit, eoque uſi ſumus ad horam prope tertiam.

Temp. Ver.			
Hor.	′	″	
1	38	0	Penumbra incipit eſſe ſenſibilis
1	41	0	fit ſpiſſior
1	43	29	fit ſpiſſiſſima
1	47	45	Umbra incipit
1	49	25	Diſcus Lunæ apparet deficiens
2	0	16	Umbra pertingit ad *Ariſtarchum*
2	0	39	Pertingit ad *Platonem*
2	1	10	*Ariſtarchus* totus in Umbra
2	6	22	*Architas*
2	8	7	*Ariſtoteles*
2	10	29	*Pitheas*
2	11	28	*Galilæus*
2	13	22	Umbra ad littus Orientale *Maris Serenitatis*
2	15	34	*Endymion* immergitur totus
2	18	2	*Copernicus* incipit immergi
2	20	7	totus in Umbra
2	21	5	*Poſſidonius* incipit
2	22	8	totus latet
2	27	49	*Ricciolus* incipit
2	31	56	Umbra pervenit ad *Grimaldum*
2	34	37	Ad Litus Boreale *Maris Criſium*
2	37	17	*Proclus* immergitur

Hor.

Hor.	′	″	
2	40	0	*Nubes ſupervenit, quæ Lunam omnino tegit, diuque videtur duratura.*
3	25	0	*Nubes diſcedit. Jam autem ex Umbra emerſerunt* *Grimaldus,* *Ricciolus,* *Keplerus,* *Galilæus.*
3	29	2	*Ariſtarchus* emergit
3	30	30	*Copernicus* incipit emergere
3	31	34	totus extra Umbram
3	39	18	*Pitheas* emergit
3	47	46	*Timocharis*
3	54	57	*Archimedes*
3	57	18	*Plato* incipit emergere
3	58	59	emergit totaliter
4	2	0	*Nubecula iterum Lunæ aſpectum nobis adimit.*
4	6	0	*Jam Luna reſtituitur*
4	8	15	*Ariſtotelis* totalis emerſio
			Nubeculis identidem Luuam occupantibus, reliquarum macularum emerſiones exacte obſervari nequeunt.
4	20	36	Finis Eclipſis, *tardius fortaſſe viſus ob tenuem vaporem interpoſitum*
4	26	0	Deſinit Penumbra ſpiſſior
4	28	0	Deſinit Penumbra ſenſibilis.

Peculiari profecto cura, ac ea qua fieri potuit diligentia, Eclipſim hanc obſervare conati ſumus; non modo ut noſtri muneris partes pro modulo noſtro impleremus, quippe Matheſi, ac præcipue Aſtronomicis obſervationibus hic addicti, verum etiam, ut Sereniſſimi Regis ingenio plenius, ut par erat, indulgeremus. Valde enim Ipſe in hujuſmodi obſervationibus delectatur, ad eaſque perfecte inſtituendas, copioſam nobis Inſtrumentorum ſupellectilem, munificentia vere Regia, ſuppeditavit. Neque vero aut iiſdem obſervationibus intereſſe, aut Cbſervatorem ſe nobis identidem adjungere dedignatur; illud unice ſemper ſolicitus, ne, vel minimum, a rectitudine aberretur. Hoc igitur acti ſtimulo nec diligentiæ pepercimus, nec labori, quo tanti Principis ingenio hæc noſtra obſervatio reſponderet. Acceſſit & alia qua dudum tenebamur cura, novis ac clarioribus confirmandi argumentis hujuſce Urbis Longitudinem, vel certe ejuſdem cum præcipuis Europæ Meridianis differentiam; itemque explorandi, an aliorum Tabulæ Aſtronomicæ, ad hunc Meridianum reductæ per inventas hactenus differentias, perfecte Cœlo congruerent? Ex quo enim Ulyſſiponem pervenimus, ubi jam alterum egimus annum, nulla contigerat Lunæ Eclipſis ſupra noſtrum Horizontem conſpicua. Duæ vero ſolares, altera Die 8. Decembris anni 1722. altera 22. Maii labentis anni, oppoſitis undique nubibus, obſervari nullatenus potuere. Jam autem repetitis hoc biennio obſervationibus tum Immerſionum, tum Emerſionum Intimi Jovis Satellitis, proxime deprehenderamus, ſaltem quoad minuta prima, differentiam inter hujuſce Urbis, ſeu verius hujuſce Regii Palatii, ac Regii Pariſienſis Obſervatorii Meridianos: minuta nempe horaria 48 (mox ubi receperimus obſervationes habitas in eodem Obſervatorio, deprehendere poterimus & ſecunda, quæ hic diligenter notavimus.) Hac igitur uſi differentia præſentem Eclipſim ſupputavimus ex Tabulis Hirianis. Tum autem mihi, tum P. *Dominico Capaſſo* eadem quantitas, idemque pene temporis momentum pro Initio, Medio, ac Fine Eclipſeos prodiit: Mihi nempe Init. Hor. 1. m. 48. ſec. 4. Medium, Hor. 3. m. 3. ſec. 59. Finis, Hor. 4. m. 19. ſec. 54. Illi vero, Init. Hor. 1. m. 48. ſec. 0. Med. Hor. 3. m. 3. ſec. 58. Fin. Hor. 4. m. 19. ſec. 56. Quantitas utrique digit. 6. min. 53. Sufficiens proinde argumentum erat, quod recte Tabulis uterque uſus eſſet; atque illud unice expectandum, ut Cœlum pariter ſupputationibus reſponderet. Quamquam ex nonnullis Ephemeridibus aliter futurum deducebatur. At revera quam proxime reſpondit (neglectis ſcilicet tantum ſecundis) ut ex ipſa patet obſervatione.

Fateor tamen nec me multum fidere hujuſmodi obſervationibus Lunarium Eclipſium, quod attinet ad ſecunda definienda. Adeo enim male terminata apparet terreſtris umbra, ut ejus ac ſpiſſioris penumbræ confinia illico diſcernere difficile admodum videatur. Hinc non abs re fore putaverim nonnullas hic ſubtexere obſervationes Immerſionum atque Emerſionum Intimi Jovis Satellitis, ex iis, quæ præſertim hoc anno habuimus; nam multas præteriti anni in promptu non habeo. Duas tantum memini, & alibi etiam adnotatas invenio, quibus ſcilicet uſus eſt Cl. V. & Illuſtriſſ. Dominus *Franc. Blanchinus*, ad determinandam differentiam inter Meridianos Ulyſſiponenſem, ac Urbinenſem. Has igitur tantum ex præterito anno ſubnectam.

Obſer-

Obſervationes Immerſionum, ac Emerſionum Intimi Jovis Satellitis, habitæ Ulyſſipone in Palatio Regio, & in Collegio Divi Antonii Soc. Jeſu.

Regium Palatium, ac Collegium ſunt in eodem omnino Meridiano. Omnes obſervationes habitæ ſunt Teleſcopio palmor. rom. 30. *Joſephi Campani.*

	H.	′	″
1723.			
Emerſiones.			
Die 23. Julii	7	47	00
Die 7 Septem.	8	21	48
1724.			
Immerſiones.			
Die 8 Jun. *mane*	2	03	28
Die 15	3	56	27
Die 30	2	08	51
Emerſiones.			
Die 2 Sept.	9	36	57
Die 9	11	34	26
Die 25	9	59	21
Die 4 Octob.	6	26	44
Die 18	10	21	20
Die 3 Novemb.	8	42	30

VIII. *Meridianorum* Ulyſſiponenſis, Pariſienſis *&* Londinenſis *differentia, ex literis Clariſſimi Doctiſſimique Viri, Reverend. Patr.* Johannis Baptiſtæ Carbone *Soc. Jeſ. ad* Iſaacum Sequeyra Samuda *M. D. Coll. Med. Lond. Lic. S. R. S.*

Illud advertere juvat, differentiam inter hunc meridianum, ac Pariſienſem. minorem jam inventam, ac nos antea putabamus; non quidem novis obſervationibus hic habitis, ſed iiſdem cum Pariſienſibus, quas modo accepimus, comparatis. Nullas antehac acceperamus, unde dictam differentiam deprehendere certo poſſemus; ſed noſtras obſervationes unice contuleramus cum ſupputationibus domini *Lieutaud*, meridiano Pariſienſi accommodatis, in ſuo libello, quem *Connoiſſance des Temps* inſcribit, & quotannis, Academiæ regiæ juſſu, in lucem edit. At nimium a vero aberrare illas dignovimus, præcipue, quæ ad immerſiones, atque emerſiones intimi Jovis ſatellitis ſpectant. Ipſæ enim obſervationes habitæ in Obſervatorio regio, modo duobus, modo tribus, modo etiam 4 minutis, ab illis diſſentiunt. En tibi comparationem noſtræ obſervationis lunaris Eclipſis, cum obſervatione Pariſienſi, ab ipſo *Maraldo* obſervatore, & regio aſtronomo facta; cui etiam apponam comparationes immerſionum atque emerſionum, quæ utrobique fuerunt obſervatæ: omnia genuino idiomate, quo fuerunt ad me miſſa, Pariſiis.

Com-

Comparaiſon des Obſervations de l' Eclipſe de Lune du 1 Novembre 1724, faites a Lisbone, & a Paris.

Quoyque dans cette eclipſe, l'ombre de la Lune n' ait point paru terminèe a Lisbone, ny a Paris, ce qui a rendu la determination de ces phaſes plus difficiles, cependant la plus part des obſervations s' accordent ſi bien enſemble, que nous avous cru devoir faire la comparaiſon des phaſes principales, qui paroiſſent avoir etè obſervées avec le plus d' exactitude, pour en determiner la difference des meridiens entre Lisbone, & Paris.

Hor.	′	″	
1	47	45	Commencement a Lisbone
2	33	30	a Paris
	45	45	Difference des meridiens entre Lisbone & Paris
2	0	16	L'ombre a Ariſtarque a Lisbone
2	46	15	a Paris
	45	59	Difference
2	11	28	a Lisbone l'ombre a Galilée
2	56	20	a Paris
	44	52	Difference
2	34	37	a Lisbone l'ombre au bord ſeptentrional de la mer Caſpiene
3	20	30	a Paris
	45	53	Difference
2	37	17	a Lisbone l'ombre a Proclus
3	23	30	a Paris
	46	13	Difference

Hor.	′	″	
3	29	2	a Lisbone Ariſtarque ſort de l'ombre
4	14	30	a Paris
	45	28	Difference
3	31	34	Tout Copernic eſt hors de l'ombre a Lisbone
4	17	50	a Paris
	46	16	Difference
3	47	46	Timocharis eſt ſorti de l'ombre a Lisbone
4	33	34	a Paris
	45	48	Difference
3	58	59	Platon eſt entierement hors de l'ombre a Lisbone
4	44	23	a Paris
	45	24	Difference
4	20	36	Fin de l' eclipſe a Lisbone
1	6	30	a Paris
	45	54	Difference

Suivant ces obſervations la durée de l' eclipſe a Liſbone a eté de 2^h 32′ 51″ plus petite ſeulement de 9 ſecondes qu'elle n'a eté obſervée a Paris, & la difference des meridiens, qui reſulte des obſervations du commencement & de la fin eſt de 45′ 50″ ce qui s'approche beaucoup de ce qui reſulte de la comparaiſon des autres taches obſervées a Lisbone, & a Paris.

Com-

Comparaiſon de quelques Obſervations des Satellites de Jupiter faites a Liſbone, & a Paris.

Nous avons fait a l'obſervatoire royal de Paris pluſieurs obſervations correſpondantes a celles qui nous ont eté envoyées de Liſbone, en voici la comparaiſon.

		H.	′	″	
Le 30 Juin. 1724	a	2	08	51	Immerſion a Liſbone
		2	54	41	a Paris
			45	50	Difference
Le 2 Sept. 1724	a	9	36	57	Emerſion a Liſbone
		10	22	46	a Paris
			45	49	Difference
Le 25 Sept. 1724	a	9	59	21	Emerſion a Liſbone
		10	45	05	a Paris
			45	44	Difference
Le 4 Octobre	a	6	26	44	Emerſion a Liſbone
		7	11	58	a Paris
			45	14	Difference

La plus part de ces obſervations, s' accordent a donner la difference des meridiens, entre Liſbone & Paris de 45′ 48″ d'heure, ce qui s'accorde avec toute l'exactitude que l'on peut eſperer, a celle que l'on a determinée par l'obſervation derniere de l'eclipſe de Lune, &c. Hactenus dominus *Maraldus*, cujus obſervationem lunaris eclipſeos, ſeorſim tranſcribere non vacat; pluribus enim curis ſum diſtentus.

Si vera eſt prædicta differentia, nempe 45′ 48″, erit differentia inter hunc meridianum Ulyſſiponenſem, & Londini, 36′ 7″, quam mox collatis obſervationibus in utroque meridiano faciendis, melius examinabimus, certiuſque deprehendemus.

IX. *An Excretory Duct from the* Glandula Renalis. *Extracted from the* Giornale di Literati *of* Venice, *for the Year* 1719.

THE celebrated Anatomiſt, Signor *Anton Maria Valſalva*, already known by his noble Treatiſe *De Aure humana*, has lately made a conſiderable Diſcovery. He has found the excretory Ducts of the *Glandulæ Renales*, or, *Renes Succenturiati*, which diſcharge themſelves into the Parts of Generation; that is to ſay, into the *Epididymides* in Men, and into the *Ovaria* in Women. He has read a learned Diſſertation relating to this Diſcovery, before the Academy of Sciences in *Bologna*, in which he undertakes to prove, that thoſe *Renes ſuccenturiati*, are to be reckon'd among the principal Organs of Generation. He is ſoon to publiſh it in Print, but the Publication of the Cuts belonging to it, will be retarded, becauſe the engraving of them, cannot be finiſh'd in ſo ſhort a time.

We have ſince learn'd the diſagreeable News of the Death of this very learned and inquiſitive Anatomiſt; but, as his Reputation, and known Accuracy in diſſecting, leaves no room to doubt of the reality of the abovemention'd Diſcovery, we hope it will be proſecuted by other curious Perſons.

X. *Of the Currents at the Streights Mouth. By Capt. - - - - Communicated by Dr.* Hudſon.

CApe *Spartel*, and Cape *Trafalgar*, from the weſtern Ocean, are known to make the *Streights* Mouth, from whence a Current, in the middle of the Channel (which is about five Leagues broad) betwixt the *Barbary* and *Spaniſh* Land, runs, at leaſt, two Miles each Hour, as far as *Ceuta* Point; and there the two Coaſts opening about eighteen Leagues diſtant from each other, the Current does not run above one Mile an Hour, and ſo continues as far as Cape *de Gat*, which is ſeventy Leagues up the *Mediterranean*. Our Mariners obſerve a Current to ſet to the weſtern Sea, or the great Ocean from *Ceuta*, along the *Barbary* Shore; and, from *Gibraltar*, along the *Spaniſh* Shore; but that on the *Barbary* Shore is generally their common Rout, not only as being the freeſt from Rocks and leſs dangerous, but by reaſon that the Tide is much ſtronger, than it is on the other Side, which the ſooner helps the Ships out of the *Streights*, which are the narroweſt betwixt the Points of *Gibraltar* and *Ceuta*; at which laſt Place, a Neck of Land extends itſelf a conſiderable Way into the Sea; and it's my Opinion, and that of others, that whereas the Current runs, as aboveſaid, two Miles an Hour againſt this Neck of Land, the Water there meets with ſo violent an Oppoſition in its Courſe, as occaſions it to rebound with ſo much Force, that Part of it returns back along the ſame Coaſt, and ſo out of the *Streights* Mouth; which, with the ſmall Tide that ſets out on the *Spaniſh* Shore, it's believ'd, may exhauſt a conſiderable Part of that Current, which continually ſets in, to the Eaſtward, at the Rate I have already mention'd. What I look upon to be very remarkable, is that in the Year 1712, Monſieur *du L'aigle*, that fortunate and generous Commander of the Privateer call'd the *Phœnix* of *Marſeille*, giving Chaſe, near *Ceuta* Point, to a *Dutch* Ship bound for *Holland*, he came up with her in the middle of the Gut, or *Streights*, betwixt *Tariffa* and *Tangier*, and there gave her one Broad-ſide, which directly ſunk her, all her Men being ſav'd by the Means of Monſ. *du L'aigle*; and a few Days after, the ſunk Ship, with her Cargo of Brandy and Oil, aroſe on the Shore near *Tangier*, which is, at leaſt, four Leagues to the Weſtward of the Place were ſhe ſunk, and directly againſt the Strength of the Current; which has perſuaded many Men, that there is a Recurrency in the deep Water in the middle of the Gut, that ſets outwards to the grand Ocean, which, I think, this Accident very much demonſtrates; and poſſibly, a great Part of the Water, which runs into the *Streights*, does return that Way, and along the two Coaſts which I have already mention'd; otherwiſe, this Ship of Courſe, muſt have been drove towards *Ceuta*, and ſo upwards. I was at *Gibraltar* when this happen'd, where I ſaw above 100 of the Butts of that Cargo of Brandy, which were ſent thither from *Tangier*; I likewiſe ſpoke with the Captain of the *Dutch* Ship, who told the Governor, myſelf, and many others, where his Veſſel ſunk; and her riſing afterwards at *Tangier*, appear'd very unaccountable to us, as it does to me to this Day; for there's no Doubt but the Ship ſunk where the *Dutchman* told us, ſince the *Spaniards* from the Land, who ſaw it, confirm'd it to us. The Water in the Gut muſt be very deep, ſeveral of the Commanders of our Ships of War having attempted to ſound it with the longeſt Lines they could contrive, but could never find any Bottom.

XI. Ambergris *found in* Whales. *Communicated by Dr.* Boylſton *of* Boſton *in* New-England.

THE moſt learned Part of Mankind are ſtill at a Loſs about many Things, even in medical Uſe; and, particularly, were ſo in what is called ***Ambergris***, until our ***Whale*** Fiſhermen of ***Nantucket***, in ***New-England***, ſome three or four Years paſt, made the Diſcovery. Their Account to me is this.

Cutting up a ***Spermaceti*** Bull ***Whale***, they found accidentally in him, about twenty Pound Weight, more or leſs, of that Drug. After which, they, and other ſuch Fiſhermen, became very curious in ſearching all ſuch Whales they kill'd; and it has been ſince found in leſſer Quantities, in ſeveral Male Whales of that Kind, and in no other, and that ſcarcely in one of an Hundred of them. They add further, that it is contained in a Cyſt, or Bag, without any inlet or outlet to it, and that they have ſometimes found the Bag empty, and yet entire.

The Bag is no where to be found, but near the Genital Parts of the Fiſh. The ***Ambergris*** is, when firſt taken out, moiſt, and of an exceeding ſtrong and offenſive Smell.

Whether or not (from the Account above) the Ambergris ***be naturally, or accidentally produced in that Fiſh, I leave to the Learned to determine.***

XII. *Obſervations on ſome of the Plants in* New-England, *with remarkable Inſtances of the Nature and Power of Vegetation. In a Letter to the Publiſher from the Honourable* Paul Dudley, *Eſq;* F. R. S.

THE Society, without Doubt, have long ſince been acquainted, that the Plants of *England*, as well thoſe of the Fields and Orchards, as of the Garden, that have been brought over hither, ſuit mighty well with our Soil, and grow here to great Perfection; however, that the curious may better make a Compariſon, I ſhall ſingle out ſome Particulars, of my own Obſervation.

To begin with the Fruit Trees.

Our Apples are, without Doubt, as good as thoſe of *England*, and much fairer to look to, and ſo are the Pears; but we have not got of all the Sorts.

Our Peaches do rather excel thoſe of *England*, and then we have not the Trouble or Expence of Walls for them; for our Peach Trees are all Standards, and I have had, in my own Garden, ſeven or eight Hundred fine Peaches of the Rare-ripes, growing at a Time on one Tree.

Our People, of late Years, have run ſo much upon Orchards, that in a Village near *Boſton*, conſiſting of about forty Families, they made near three Thouſand Barrels of Cyder. This was in the Year 1721. And, in another Town of two Hundred Families, in the ſame Year, I am credibly inform'd, they made near ten Thouſand Barrels. Some of our Apple Trees, will make ſix, ſome have made ſeven Barrels of Cyder, but this is not common; and the Apples will yield from ſeven, to nine Buſhels for a Barrel of Cyder. A good Apple Tree, with us, will meaſure from ſix to ten Foot in Girt. I have ſeen a fine Pearmain, at a Foot from the Ground, meaſure ten Feet, and four Inches round. This Tree, in one Year, has bore thirty eight Buſhels, (by Meaſure) of as fine Pearmains, as everI ſaw in *England.* A *Kentiſh* Pippin at three Foot from the Ground, ſeven Foot in Girt; a Golden Ruſſetin ſix Foot round. The largeſt Apple Tree, that I could find, was ten Foot and ſix Inches round, but this was no Graft.

Pear Trees.

An *Orange* Pear Tree grows the largeſt, and yields the faireſt Fruit. I know one of them near forty Foot high, that meaſures ſix Foot and ſix Inches in Girt, a Yard from the Ground, and has bore thirty Buſhels at Time; and this Year I meaſur'd an *Orange* Pear, that grew in my own Orchard, of eleven Inches round the Bulge. I have a Warden Pear Tree, that meaſures five Foot ſix Inches round. One of my Neighbours has a *Bergamot* Pear Tree, that was brought from *England* in a Box, about the Year 1643, that now meaſures ſix Foot about, and has bore twenty two Buſhels of fine Pears in one Year. About twenty Years ſince, the Owner took a Cyon, and grafted it upon a common Hedge Pear, but the Fruit does not prove altogether ſo good, and the Rind, or Skin, is thicker than that of the Original.

Peach Trees.

Our Peach Trees are large and fruitful, and bear commonly in three Years from the Stone. I have one in my Garden of twelve Years Growth, that meaſures two Foot and an Inch in Girt, a Yard from the Ground, which, two Years ago, bore me near a Buſhel of fine Peaches. Our common Cherries are not ſo good as the *Kentiſh* Cherries of *England*, and we have no Dukes, or Heart Cherries, unleſs in two or three Gardens.

Trees of the Wood.

Some Years ſince, I meaſur'd a *Platanus Occidentalis*, or *Button Wood* Tree (as they are called here) of nine Yards in Girt, and it held its Bigneſs a great Way up. This Tree, when it was cut down, I am inform'd, made twenty two Cord of Wood. A Gentleman tells me, that in the Foreſt, he met with a ſtreight Aſh, that grew like a Pillar, of a great Height, and free from Limbs, that meaſured fourteen Feet eight Inches round, near a Yard from the Ground; and, the other Day, I met with a *Saſſafras* Tree, that meaſur'd five Foot three Inches in Girt. I meddle not here with our noble Pines and Cedars, becauſe I deſign to treat of them in a Chapter of the Evergreens of this Country. Among our Trees of quick and eaſy Growth, the *Button Wood* before-mentioned, and the *Locuſt* Tree, are the moſt remarkable: As to the latter, by the Deſcription Mr. *Moore*, while in *New-England*, gave me of the *Manna* Tree, our *Locuſt* Tree may be called the *American* Manna. I have known a Seed of it blown off from the Tree into my Garden, that took Root of itſelf, and, in leſs than two Years, was got above ſix Foot high, and as big about, as a common walking Cane. The *Platanus* I have frequently propagated, by cutting off Sticks of five or ſix Foot long, and ſetting them a Foot deep into the Ground in the Spring of the Year, when the Seaſon is wet; they thrive beſt in a moiſt Soil.

Garden.

An Onion, ſet out for Seed, will riſe to four Foot nine Inches in Height. A Parſnip will reach to eight Foot, red *Orrice* will mount nine Foot, white *Orrice* eight. In the Paſtures, I meaſured Seed *Mullen* nine Foot two Inches in Height, and one of the common Thiſtles above eight Foot.

Vegetation.

Among the remarkable Inſtances of the Power of Vegetation, I ſhall begin with an Account of a Pumpkin Seed, which I have well atteſted, from a worthy Divine. * The Relation is as follows: That in the Year 1699, a ſingle Pumpkin Seed was accidentally dropp'd in a ſmall Paſture where Cattle had been fodder'd for ſome Time. This ſingle Seed took Root of itſelf, and without any Manner of Care or Cultivation; the Vine run along over ſeveral Fences, and ſpread over a large Piece of Ground far and wide, and continued its Progreſs till the Froſt came and kill'd it. This Seed had no more than one Stalk, but a very large one; for it meaſured eight Inches round; from this ſingle Vine, they gathered two hundred and ſixty Pumpkins; and, one with another, as big as an half Peck; enough in the Whole, to fill a large Tumbrel, beſides a conſiderable Number of ſmall and unripe Pumpkins, that they made no Account of. The *Philoſophical Tranſactions* give an Account of a ſingle Plant of Barley, that by ſteeping and watering with *Salt-Petre* diſſolv'd in Water, produced two hundred and forty nine Stalks, and eighteen thouſand Grains; but then there was

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* *The Reverend Mr.* Edwards *of* Windſor.

Art, and even Force in that Caſe; whereas in ours, there was nothing but pure Nature and Accident.

Our *Indian* Corn is the moſt Prolifick Grain that we have, and commonly produces twelve hundred and often two thouſand Grains from one; but the faireſt Computation is thus; ſix Quarts of this Grain will plant an Acre of Ground, and it is not unuſual for an Acre of good Ground to produce fifty Buſhels of Corn. The Mention of *Indian* Corn obliges me to take notice of an extraordinary *Phænomenon* in the Vegetation of that Grain, *viz.* the interchanging, or mixing, of Colours after the Corn is planted. For your better underſtanding this Matter, I muſt obſerve, that our *Indian* Corn is of ſeveral Colours, as blue, white, red, and yellow; and if they are planted ſeparately, or by themſelves, ſo that no other Sort be near them, they will keep to their own Colour, *i. e.* the blue, will produce blue, the white, white, *&c.* But if in the ſame Field, you plant the blue Corn in one Row of Hills (as we term them) and the white, or yellow, in the next Row, they will mix and interchange their Colours; that is, ſome of the Ears of Corn, in the blue Corn Rows, ſhall be white, or yellow; and ſome again, in the white or yellow Rows, ſhall be of a blue Colour. Our Hills of *Indian* Corn are generally about four Foot aſunder, and ſo continued in a ſtreight Line, as far as the Field will allow; and then a ſecond Line, or Row of Hills, and ſo on; and yet this mixing and interchanging of Colours has been obſerved, when the Diſtance between the Rows of Hills, has been ſeveral Yards; and a worthy Clergyman, of an Iſland in this Province, * aſſures me, that the blue Corn has thus communicated, or exchanged, even at the Diſtance of four or five Rods;

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* *The Reverend Mr.* Mayhew, *of* Martha's *Vineyard.*

and, particularly in one Place, where there was a broad Ditch of Water betwixt them. Some of our People, but eſpecially the *Ab-Origines*, have been of Opinion, that this Commixtion, and Interchange, was owing to the Roots, and ſmall Fibres reaching to and communicating with one another; but this muſt certainly be a Miſtake, conſidering the great Diſtance of the Communication, eſpecially at ſome Times, and croſs a Canal of Water; for the ſmalleſt Fibres of the Roots of our *Indian* Corn, cannot extend above four or five Foot. I am therefore humbly of Opinion, that the *Stamina*, or Principles of this wonderful Copulation, or mixing of Colours, are carried thro' the Air by the Wind; and that the Time, or Seaſon of it, is, when the Corn is in the Earing, and while the Milk is in the Grain, for at that Time, the Corn is in a Sort of Eſtuation, and emits a ſtrong Scent. One Thing, which confirms the Air's being the Medium of this Communication of Colours in the Corn, is an Obſervation of one of my Neighbours, that a cloſe, high board Fence, between two Fields of Corn that were of a different Colour, entirely prevented any Mixture or Alteration of Colour, from that they were planted with.

It has been obſerv'd by Naturaliſts, that even Nature, which gives Laws to every Thing, does not always ſtrictly obſerve her own Rules; and I think, I have, in my own Town, met with a very notable Inſtance of this in the Vegetable World.

An Apple Tree there bears a conſiderable Quantity of Apples, eſpecially every other Year, which never had a Bloſſom; I had formerly heard the Owner ſpeak of it: But for the three laſt Years, I made it my Buſineſs, in the proper Seaſon, to go and obſerve it my ſelf; and when all the reſt of the Orchard was in the Bloom, this Tree had not one Bloſſom. Not being contented with once going, I went again, and again, till I found the young

Apples perfectly formed The laſt Year, I went early, not knowing but that it might blow ſooner than the other Trees, but I found no Bloſſoms; and the Owner, with many of his Neighbours, aſſured me, they have known the Tree theſe forty Years, and that it never had a Bloſſom. I opened ſeveral of the Apples, and obſerv'd but very few Seeds in them; and ſome of them lodged ſingle in the Side of the Apple. This Tree was no Graft, and the Fruit but ordinary for Taſte. I could not perceive, by my Obſervation, but that, in all other Reſpects, it fructify'd like other Apple Trees. But that I may not leave Nature in any Diſorder, I would juſt mention, what is frequently obſerv'd in our Gardens, as to the winding or running Vines, more eſpecially the Hop, and the *French*, or Kidney Beans; how contrary they are to one another in their climbing, and yet how ſteadily they obſerve their own Laws: The Hop Vine winding about the Pole with the Sun, and the Bean againſt the Sun; and this Courſe they keep with ſuch Obſtinacy, that though an Attempt has been made over Night, to force the Hop Vine to wind againſt the Sun; yet, in the Morning, it has got back again to its natural Courſe, and the Bean again has done the ſame in her Way. In like Manner, the *Indian* Corn, abovementioned, has always an equal Number of Rows of Grain on the Ear, as eight, twelve, *&c.*

FINIS.

ERRATA.

Philoſ. Tranſact. N°. 383.

PAG. 94. N° 77. l. Ki—tſai—tsè. P. 95. N° 95 pro Germanicæ l. Gerardi. Ibid. N° 98. l Vicia Sylveſtris, &c.

PHILOSOPHICAL TRANSACTIONS:

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OF THE

Preſent Undertakings, Studies, and Labours

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IN MANY

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PHILOSOPHICAL TRANSACTIONS.

For the Months of *January* and *February*, 1725.

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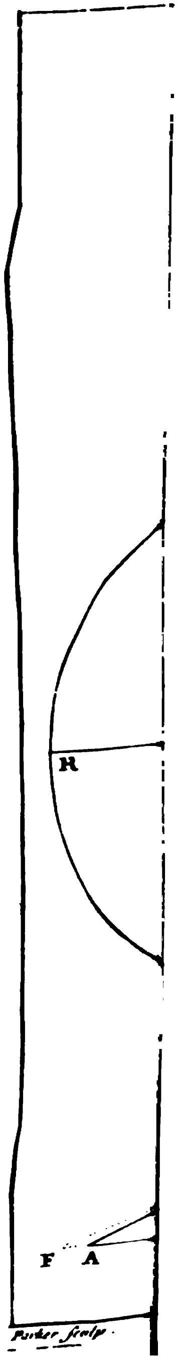

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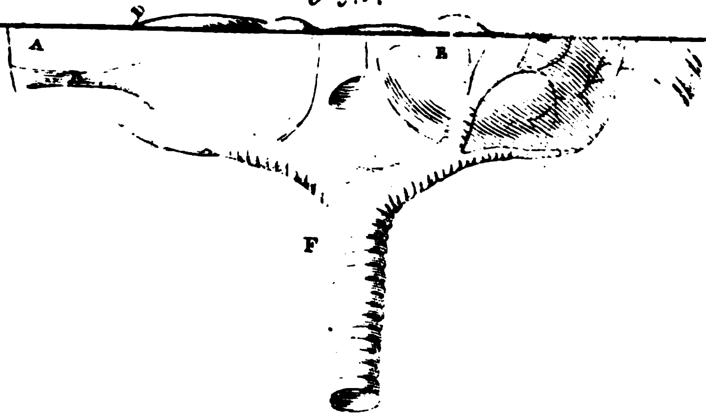
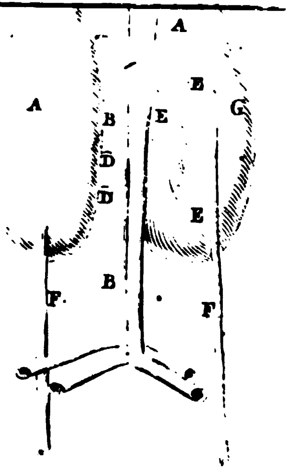
Fig. 3.





A Diſſertation concerning the Figure of the Earth, by the Reverend John Theophilus Deſaguliers, *LL. D. F. R. S.*

THAT the Earth (that is, our Terraqueous Habitation) is of a ſpherical Figure, or nearly ſuch, has been prov'd ſo often, and by ſo many unanſwerable Arguments, that to repeat them here muſt needs be tedious to this learned Society. But, as a little Variation from a true Sphere (beſides the Irregularity of high Hills and deep Vallies) does not hinder us from calling the Earth a Globe; ſo, to determine what that Variation may be, ſince modern Philoſophers are divided about it, may be a Subject not ungrateful at this Time.

Monſieur *Caſſini* ſays: " That the Earth is an oblong
" Spheroid, higher at the Poles than the Æquator,
" making the Axis longer than a Diameter of the Æquator
" about thirteen *French* Leagues, which he deduces
" from comparing his Father's Meaſures of the
" Meridian, from *Paris* to the *Pyrenæean* Mountains,
" with thoſe of Monſieur *Picard*; of which an
" Account may be ſeen in the Memoirs of the Royal
" Academy for 1713. But having afterwards continued
" the Meridian, which is drawn through *France*,
" from *Paris* to *Dunkerque*, he ſtill draws Conſequences
" to prove the Earth an oblong Spheroid;
" but then makes the Axis exceed the Æquatorial
" Diameter 34 Leagues.

“ Sir *Iſaac Newton* makes the Earth higher at the “ ~~Æquator, and, conſequently, flatted towards the~~ “ Poles, reckoning its Æquatorial Diameter 34 *Engliſh* “ Miles longer than the Axis; which he proves from “ the Principles of Gravity, and the Centrifugal Force “ that ariſes from the Diurnal Rotation of the Earth; “ and, to confirm this, mentions ſeveral Experiments “ on Pendulums, which have been made ſhorter, to “ ſwing Seconds, near the Æquator, than in greater “ Latitudes.”

Theſe are the two Opinions which have divided Philoſophers, and which we propoſe to examine here.

Monſieur *Caſſini*, taking the Meaſures above-mention’d to be exact enough, not only to determine the Magnitude of a Degree of the Earth, correſponding with a Degree of the great Circle of the Heavens, but alſo to ſhew the Difference in the Degrees of the Earth; (reckoning thoſe, that were meaſured in the South of *France*, to exceed thoſe towards the North, by a certain Number of Toiſes and Feet) demonſtrates, that if the Degrees of the Earth are longer towards the Æquator than the Poles, the Plane of the Meridian muſt be an Ellipſe, whoſe long Axis is that of the Earth. Here follows his firſt Demonſtration. [*See the* French *Memoirs for the Year* 1713]

“ Let B D C R * be an Ellipſe that repreſents a “ Meridian of the Earth, whoſe Poles B and C are at “ Ends of the great Axis B C, and whoſe Foci E and “ F are taken at Pleaſure. *Now, to divide this* “ *Ellipſe into Degrees, that is, to find ſeveral* “ *Points* H, I, V, *ſuch, that the Diſtance, from the* “ *Pole to the Zenith, of every one of them, ſhall* “ *be of any given Number of Degrees.*

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* Fig. 1.

" From E, one of the Foci of the Ellipſe, draw the " Line E T, ſo that it may, with the Axis B C, " make the Angle B E T equal to the Diſtance given " from the Pole to the Zenith. From the other Focus " F, with the Diſtance B C equal to the Axis, draw " an Arc, to cut the Line E T at T. I ſay, that the " Line F T, drawn from the Point T to the Focus " F, will cut the Ellipſe at the Point H; which Point " is ſuch, that the Diſtance of the Pole, from its " Zenith, contains the given Number of Degrees.

"DEMONSTRATION.

" From the Point H, raiſe H Z, perpendicular to " the Ellipſe which will paſs through the Zenith Z; " and, being produc'd inwards, will meet the Axis of " the Earth at O, and (by the Property of the Ellipſe) " divide the Angle E H F into two equal Parts. From " the Point H, draw likewiſe H P, parallel to the Axis " B C, and directed to the Pole P, ſuppos'd at an infi- " nite Diſtance. The Angle P H Z, or P O Z, mea- " ſures the Diſtance, from the Pole to the Zenith, of " an Inhabitant dwelling upon the Earth at the Point " H. F T is equal to the Axis B C, by Conſtruction; " but, by the Property of the Ellipſe, B C is equal to " E H *plus* H F; taking away from both F H, which " is common, E H will remain equal to H T. The " Angles E T H, T E H, will therefore be equal, " and, conſequently, each of them will be half of the " external Angle E H F; but the Angle E H O is " likewiſe equal to half of the Angle E H F; there- " fore the Angles T E H, E H O, will be equal to " one another; and, conſequently, the Lines E T and " H O will be parallel to one another; and the Angle " P O Z, which meaſures the Diſtance from the Pole

“ to the Zenith of the Point H, will be equal to the
“ Angle B E T, which was, by Conſtruction, taken
“ equal to the given Diſtance of the Pole from the
“ Zenith ; *which was to be demonſtrated.*

“ Now, if the Proportion of the longeſt Diameter
“ of the Ellipſe B C to E F, the Diſtance of the Foci,
“ be taken at Pleaſure, one may by Calculation
“ find all the Points of the Ellipſe as H, to deter-
“ mine the Degrees by this Analogy.

“ *As F T, or B C:*

“ *Is to E F : :*

“ *So is the Sine of the Angle P E T (the given Di-*
“ *ſtance from the Pole to the Zenith) :*

“ *To the Sine of the Angle E T F, or T E H*:
“ whoſe Quantity will conſequently be known. This
“ Angle T E H being added to the Angle P E T, the
“ given Diſtance from the Pole to the Zenith of the
“ Point H, will give the Quantity of the Angle B E H,
“ which a Line drawn from the Focus to H, the Point
“ requir’d, makes with the Axis of the Ellipſe.

“ Then in the Triangle E H F, whoſe Side E F is
“ known, as well as the Angle E H F, which is the
“ Double of the Angle T E H, and the Angle F E H
“ Supplement of the Angle B E H ; one ſhall have the
“ Length of the Side E H, known in Parts of the Axis
“ B C.

“ After the ſame Manner, may be found the Angles
“ B E I, B E V, &c. and the Length of the Lines E I,
“ E V, to determine the Diſtance, from the Pole to the
“ Zenith, of all the Degrees of the Circumference of
“ the Earth ; and in the rectilinear Triangles H E I,
“ I E V, whoſe Sides H E, E I, E V, are known as
“ well as the Angles comprehended between the Sides
“ H E, E I, I E, E V, which are the Differences of
“ the Angles B E H, B E I, B E V, determin’d above ;

" one ſhall find the Length of the Chords H I, I V, " comprehended between each Degree.

Monſ. *Caſſini*, in the Memoirs for the Year 1718, repeats the ſame Demonſtration; except that, before it, he ſhews, that if ſeveral Points be taken upon a Terreſtrial Meridian, on the Surface of an Elliptick Earth, as G, H, I, K,* in ſuch Manner, that their reſpective Zeniths Z, L, M, N, are diſtant from one another, an equal Number of Degrees meaſur'd in a Celeſtial Meridian. The Lines Z G, L H, M I, N K (which are perpendicular to the Ellipſe) being produced, will meet in the Points O, R, and S, making equal Angles; but as thoſe angular Points are not equally diſtant from the Curve of the Ellipſe, that Elliptic Arc muſt be the longeſt whoſe angular Point is fartheſt off. Now, by the former Demonſtration, it appears, that thoſe Arcs, which are taken neareſt to the leſſer Axis, will have their angular Points farther remov'd, &c.

If Monſ. *Caſſini*'s Meaſures of Terreſtrial Degrees, decreaſing from the Æquator towards the Pole, were grounded on Obſervations liable to no Error, he wou'd have fully prov'd his Figure of the Earth. But ſince thoſe Meaſures (however accurately taken) are not built upon a mathematical Certainty, his Premiſes may be call'd in Queſtion, and his Concluſion, tho' mathematically drawn from theſe Premiſes, is only probable.

Now therefore, if I can ſhew from undoubted *Phænomena*, that his Concluſion will lead to an Abſurdity, his Meaſures muſt be falſe; becauſe his Reaſoning from them is juſt. This I ſhall endeavour to do firſt, which will diſprove his Figure of the Earth; and afterwards endeavour to point out ſome of the Errors which

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* Fig. II.

I ſuppoſe to have occaſion'd the Miſtake in the Meaſures.

Monſ. *Caſſini*, as well as the *Engliſh* Aſtronomers, believes that the Earth makes one Revolution about its Axis, once in 23 Hours 56′, becauſe in that Time, the Plane of any Meridian returns to the ſame fix'd Star from which it had departed.

Let * H be taken in any Parallel of Latitude, as for Example, in the Latitude of 51° 46′, a Plumb Line, L H, will be perpendicular to the Curve B H, at H, and produced paſs thro' the Zenith of the Point H, if the Earth had no Diurnal Rotation; but ſince the Earth moves round its Axis, all Bodies upon its Surface, endeavour to fly from the Axis of their Motion with a Force proportionable to their Diſtance from it in a Direction along the Plane of that Parallel, in which they are. Let that Force (explain'd by Monſ. *Huygens*, and called a Centrifugal Force) be repreſented by the Line H *l*, or its Equal and Parallel L *h*; now a Plummet placed at L, if the Earth ſtood ſtill, would deſcend in the Line L H, but as it is at the ſame Time acted upon by the Force H *l* in the Direction L *h*, it will move in the Direction L *l* Diagonal of the Parallelogram H *l*, according to the known Laws of Mechanicks; and the Plumb Line L H, inſtead of being perpendicular to the Curve at H, will in the Latitude 51° 46′ make an Angle of 5′ with H L. This Angle will be leſs towards the Poles, till at the very Pole it quite vaniſhes, as it alſo does at the Æquator. *The Demonſtration of the Quantity of this Angle, I ſhall give before I end this Diſſertation.* Now ſince there is no ſuch Angle obſerv'd, but in all Water Levels we find the Plumb Line always perpendicular to the Line of Level, the

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* . II.

Surface of the Earth muſt be depreſs'd towards G, and riſe farther from the Axis towards I, in order to become perpendicular (that is, to have its Tangent perpendicular) to the Line L *l*, in which we have ſhewn that the Plumb Line muſt deſcend.

If there is any Body ſo fond of Monſieur *Caſſini*'s Hypotheſis, as to deny the Diurnal Motion of the Earth for the ſake of it, I hope they will be convinc'd, when I ſhew the Meaſures, upon which it is founded, to be inſufficient for determining the different Lengths of the Degrees of a Terreſtrial Meridian.

But here I would not be thought to endeavour to leſſen the Praiſe due to the Gentlemen of the *Royal Academy*, for carrying on a Meridian the whole Length of *France*, from *Dunkerque* thro' the Royal Obſervatory at *Paris*, quite to the *Pyrenæan* Mountains on the Borders of *Spain*. Aſtronomy and Geography are doubtleſs much indebted to the Encouragement given by the *French* Government, and to the Care of their Mathematicians, who have omitted no proper Method for drawing their Meridian, and correcting it as they went on. So many Obſervations of the riſing and ſetting Sun, ſo many equal Altitudes of the ſame Stars accurately taken, ſo many Digreſſions of Stars, ſo many other Obſervations made with the Teleſcope and good Pendulum-Clocks — all compar'd together, for the true ſettling of the direct Way of this famous Meridian, leave no Doubt but that it is as perfect as the Nature of the Thing is capable of. And, certainly, by the Help of this Line, and the ſeveral Triangles made uſe of for carrying it on, a better Map of *France* is made, than has ever been of any Country before: Nay, beſides, I believe we may, at a Medium, very well receive their Number of 57060, or 57061 Toiſes, for the Meaſure of a Degree of a Meridian of the Earth, one with another. But to ſay, that thoſe Gentlemen cou'd obſerve the Latitude ſo nicely, as to find a Difference in the Length of the Terreſtrial Degrees, and that only of eleven or twelve Toiſes, (when they made it the leaſt) or of thirty-one Toiſes, (when they made it the moſt) is attributing to them an Exactneſs, ſo far beyond the Nature of the Inſtruments which they made uſe of, that it wou'd be rather a Diſpraiſe than a Commendation to inſiſt upon it.

For in the firſt Place, the Inſtrument, with which they took Obſervations for the Latitude at the two Ends of their Meridian, was a ten Foot Sector (which was worſe than that which Monſ. *Picard* had made Uſe of before, becauſe the Teleſcope of his Sector was of ten Foot, whereas Monſ. *Caſſini*'s was but of three Foot, tho' applied to the ten Foot Sector) where the two hundreth Part of an Inch anſwers to eight Seconds of a Degree: Now the two hundredth Part of an Inch, being one of the leaſt viſible Parts that we can ſee in a divided Line; they could not take an Angle nearer than that; nay, their Inſtrument, according to their own Deſcription of it, was divided but to every twenty Seconds. Now they allow, that ſixteen Toiſes, upon the Surface of the Earth, anſwer to one Second in the Heavens; and they don't pretend to have taken an Obſervation nearer than to about three Seconds, which therefore cannot determine a Difference leſs than forty eight Toiſes; whereas the Degrees are only ſuppoſed to decreaſe at moſt, thirty one Toiſes each, from *Collioure* to *Dunkerque*. But an Error of eight Seconds wou'd make a Difference of one hundred and twenty eight Toiſes, on the Surface of the Earth; above ten Times greater than the Difference of Degrees in the firſt Suppoſition, and four Times greater than that Difference in the laſt. Beſides, the Latitude was not obſerv'd in the intermediate Places be-

between *Paris*, and *Collioure*, with the abovemention'd Inſtrument of ten Foot Radius; but they made Uſe of a Quadrant, whoſe Radius was only thirty nine Inches, and ſometimes an Octant of three Foot Radius. Nay, they ſay themſelves, in their Account, that it is not the Obſervations made at the Ends of the Meridian, that we are to deduce the Difference of the Length of a Degree from, but the Altitudes taken at ſeveral Places between the Extreams; and, if we grant, that they can take an Angle very well, to four or five Seconds, with the great Inſtrument, they cannot come nearer than twelve or fifteen Seconds, with the Quadrant or Octant, which we muſt depend upon for the Difference of the Meaſure of Degrees: So that upon the whole, we are to determine a Length of thirty one Toiſes, by an Inſtrument which is liable to err above two hundred.

If any Conſequences of this Kind cou'd be drawn from actual meaſuring, a Degree of Latitude ſhou'd be meaſur'd at the Æquator, and a Degree of Longitude likewiſe meaſur'd there; and a Degree very northerly, as for Example, a whole Degree might be actually meaſur'd upon the *Baltick* Sea, when frozen, in the Latitude of ſixty Degrees.—— There, according to Monſ. *Caſſini*'s laſt Suppoſition, a Degree wou'd be of 56653 Toiſes, whereas, at the Æquator, it wou'd be of 58019 Toiſes, the Difference being 1364 Toiſes, about the two and fortieth Part of a Degree, which muſt be ſenſible; and likewiſe the Degree of Longitude wou'd, according to him, be of 56817 Toiſes, leſs by 1202, or the forty eighth Part, than a Degree of Latitude at the ſame Place.

But here it may be objected, that tho' the Latitude was not taken with the ten Foot Sector, in the intermediate Places between *Paris* and *Collioure*, yet the

Latitude was taken with that Inſtrument at *Dunkerque*, *Paris*, and *Collioure*, and therefore the ſouthern Part of the Meridian, containing 6° 18′ 56″ may be compar'd with the northern Part of it, which contains 2° 12′ 16″; and that the former appears to contain more Toiſes, in Proportion to the Difference of Latitude at its Extremities, than the latter. To this may be anſwer'd, that, even in this Caſe, the Obſervations made cannot be nice enough to determine the Difference of the Length of Degrees; but there is another Error, which might conſiderably miſlead the *French* Gentlemen, and make the Degrees appear longer in the South of *France*; that is, the Error in taking the true Height of ſeveral Mountains in *Auvergne*, *Languedoc*, and among the *Pyræneans*, For if they have allow'd too much for the Air's Refraction (which,by the Obſervations of Travellers, is greater towards the northern Regions, and diminiſhes as we go Southward) the Heights of thoſe Mountains will be taken too little, and their Baſes conſequently longer, which will make the Degrees appear bigger than they are. Let A B C D, * for Example, be a Mountain, as the Mountain of *Rodez*, in the Latitude of 44° 21′, whoſe Height B D is 300 Toiſes, and whoſe Sides A B and B C (ſuppos'd to make an Angle of 26° 33′, with the Horizon) are found by Trigonometry, to be of 670 8 Toiſes each; if by a Miſtake, in taking the Height, it be ſuppos'd only equal to E D, or 257 Toiſes, then the Lines A B and B C will become E F and E G; ſo that the Baſe A C, which before was of 1200 Toiſes, will become equal to F G, which will appear to be 1279,6 Toiſes, by *Eucl.* 47. 1. Now one ſuch Miſtake, in one Degree, will give a Difference above twice as great as the ſuppos'd Difference of Degrees in that Latitude, which they make of 31 Toiſes. And that there was a Miſtake of this Kind in taking the Height of that Mountain, I ſhall ſhew.

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* Fig. III.

The Vapours, that generally float in the Air about the Tops of high Hills, make it ſo difficult to take their Height exactly, that Experiments, made with the Barometer, will, by obſerving the Fall of the *Mercury*, ſhew the Height nearer than any Thing elſe we know of. There were, indeed, ſeveral Experiments made with the Barometer,* where the Differences of the Height of the *Mercury*, from the Heights at which it ſtood at the *Royal Obſervatory*, are ſaid to anſwer to ſo many Toiſes; but of nine Obſervations mention'd by Monſ. *Caſſini*, there are not two where the Number of Toiſes, ſaid to correſpond to the Heights of the Barometer, do agree together.

The firſt Experiment of the Barometer there mention'd, made at *Collicure*, was this, " At the Height
" of 11⅔ Toiſes above the Sea, the Barometer was ſet
" up, and the *Mercury* ſtood 3⅓ Lines higher than at
" the *Royal Obſervatory* (in the Tower of the eaſtern
" Hall) at the ſame Time; and therefore, ſince that
" Tower is 44 Toiſes higher than the Sea, 3⅓ Lines
" of *Mercury* muſt anſwer to 32⅓ Toiſes.

Now, reducing theſe Toiſes to Feet, and dividing by 3⅓ it will appear that an Height of 58,5 Feet will anſwer to the Fall of one Line of *Mercury* in the Barometer. Let this be taken as the Standard, and the other Obſervations be compar'd with it. This may be done by the following Table, where the firſt Column ſhews the Place where the Obſervation was made; the ſecond, the Fall or Riſe of *Mercury* at each Place expreſs'd in Lines, or 12th Parts of a *French* Inch; the

third,

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* *Mem. of the* Royal Academy, *for* 1718, ch. 10.

third, the Heights or Depths anſwering to thoſe Lines of *Mercury*, which, in the Memoirs, are given in Toiſes, but here reduced to Feet; the fourth, the Number of Feet anſwering to one Line of *Mercury* in each Obſervation, which is the Quotient of the Feet in the third Column, divided by the Number of Lines in the Second.

Obſervations of the Barometer made at	*Lines of* Mercury.	*Said to correſpond with Feet.*	*The Fall of one Line of* Mercury *anſwers to Feet.*
I. Collioure - -	$03\frac{1}{3}$	195	58,5
II. *The Tower of* Maſſane.	31	2382	76,8
III. Bugarac -	42	3636	86,5
IV. Rupeyroux -	30	2181	72,7
V. Rodez -	24	1647	68,6
VI. Rodez -	20	1425	71,25
VII. Courlande -	54	4812	89,1
VIII. Coſte - .	54	4890	92,4
IX. Clermont -	03	200	66,6

A Sight of this Table will convince any one, that theſe Obſervations are not to be depended upon for determining the Height of the Mountains in the South of *France*; for the Differences are not ſmall, ſuch as might happen in making the Experiments; but ſuch as render the Obſervations uſeleſs for the Purpoſes above-mention'd. For Example, the firſt and the ſeventh differ almoſt $\frac{1}{3}$: And if 58,5 Feet were allow'd for the Fall of one Line of *Mercury* in the ſeventh Obſervation, inſtead of 944 Feet, then the Mountain of *Coſte* would be but 3085 Feet, inſtead of 4890. Nay, upon examining the Memoirs, I find that in ſeveral Obſervations the Number of Toiſes, ſaid to correſpond to a certain Height of *Mercury*, are only anſwerable to the Height of the Mountain above the Level of the Sea found by Trigonometry, from which the Height of the *Royal Obſervatory*, above the Sea, is ſubſtracted; though, by the Manner of the Expreſſion, a curſory Reader would imagine, that the Number of Toiſes nam'd, was always proportionable to the Fall of the *Mercury*, and think all the Experiments and Obſervations very accurately made, when they ſeem to agree ſo well in every Reſpect.

Now after all, I do not queſtion but that the Height of the Barometer, might be as it is ſet down in the Memoirs, and well enough obſerv'd; but it was wrong to compare the Height of the *Mercury* in the South of *France*, with the Height that the *Mercury* was at in the Barometer of the *Royal Obſervatory* at the ſame Time; for, at that great Diſtance and Difference of Latitude, the Weather (and conſequently the Preſſure of the Air and Height of the Barometer at the ſame Level) might very much vary.

Even when there is fair Weather all over *France*, it does not follow that the Barometer ſhall ſtand at the ſame Height. Let us ſuppoſe, for Example, that a North Wind blows: Where-ever the Air is check'd by a Chain of Mountains that run Eaſt and Weſt, it will be accumulated over thoſe Mountains, and conſequently preſs more as its Columns are higher; which will make the *Mercury* riſe higher than it wou'd do with the ſame Wind, if there were no Mountains, or if they ran North and South.

The Way, to have made the Experiments with the Barometer exactly, wou'd have been to have obſerv'd the Height of the *Mercury* at the Bottom and at the Top of the Mountain, and that with a Tube of a pretty large Bore (with a proportionably large Ciſtern for the ſtagnant *Mercury*) becauſe, in a ſmall Tube, the *Mercury* will often ſtick to the Sides, and riſe irregularly, as it will alſo in inclin'd Barometers. Simple Barometers are the beſt, and a magnifying Glaſs may be made uſe of to obſerve ſmall Riſes or Falls, having two fine and well made Indices to the Tube.

Dr. *Edmund Halley*, our Royal Aſtronomer, has, ſome Years ago, given us, in the *Philoſophical Tranſactions*, the Falls of *Mercury* in the Barometer, correſponding with the Heights to which the Barometer muſt be carried to produce thoſe Falls. The firſt tenth Part of an Inch in the Fall of the *Mercury*, he makes to anſwer to an Height of 90 Feet; the next tenth, to an Height ſomething greater, and ſo in Proportion, as the Air diminiſhes in Denſity, according as we riſe in Height. The Proportion of the firſt Tenth of the *Mercury*'s Fall, he has built upon the Compariſon of the different Specifick Gravities of Air and *Mercury*; and taking *Mercury* to be 13½ Times heavier than Water, and Water (in cold Weather) to be 800 Times heavier than Air; it follows, that 13, 5 × 800, will give 10800; which Number, if it be taken in Feet, and divided by 120 (the Number of the 10th of an Inch in a Foot) we ſhall have 90 Feet anſwerable to the 10th of an Inch, and 75 Feet to a Line or the 12th Part of an Inch. As the Doctor's Tables may be of uſe to the Curious, I have inſerted them here.

I. *A TABLE ſhewing the Altitude, to given Heights of the* Mercury.

Inches.		Feet.
30		0
29		915
28		1862
27		2844
26		3863
25		4922
20		10947
15		18715
10		29662
5		48378
1		91831
0 . 5		110547
0 . 25		129262
0 . 1	29 Miles or	154000
0 . 01	41 Miles or	216169
0 . 001	53 Miles or	278338

II. *A TABLE ſhewing the Heights of the* Mercury, *at given Altitudes.*

Feet. 0	30	00	Inches and Tenths.
1000	28	91	
2000	27	86	
3000	26	85	
4000	25	87	
5000	24	93	
1 Mile	24	67	
2	20	29	
3	16	68	
4	13	72	
5	11	28	
10	4	24	
15	1	60	
20	0	95	
25	0	23	
30	0	08	
40	0	012	

Now, as very few Mountains in the World are 3 Miles high, and, generally ſpeaking, thoſe that we look upon as high Hills, (except the *Andes*, and ſome others in *America*) are not much above a Mile high; we may, for finding the Height of Mountains, take a fix'd Number of Feet in Altitude to anſwer to every 10th or 12th of an Inch in the Fall of the *Mercury*; becauſe 90 Feet are by Dr. *Halley* only taken for the firſt Tenth, and greater Heights for other Tenths, encreaſing with the Fall of the *Mercury*. Therefore I wou'd propoſe another for a round Number, namely 96 Feet for every Tenth, and 80 Feet for every 12th of an Inch, very near the Number that I have found by my Calculation, which is as follows.

Fine *Mercury* (ſuch as is made uſe of in Barometers) is, generally ſpeaking, $13\frac{2}{3}$ times heavier than Water; and, I found ſome brought from the *Eaſt-Indies*, to be 14 times heavier. I have found Air in Summer, to be near 900 times lighter than Water; and 800 times in Winter; therefore I take 850 at a Medium. Now 850 × $13\frac{2}{3}$ = 11606,6, which, divided by 120, gives 96,7 Feet, for $\frac{1}{10}$ of an Inch of *Mercury*, or 80,5 Feet for $\frac{1}{12}$ of an Inch. This Number, taken invariable, will, in taking the Height of ſeveral Hills, agree pretty well with the Numbers that come out, when Dr. *Halley*'s Table is made uſe of; and with the Experiment made by the late Profeſſor, Mr. *J. Caſwell*, who, having taken the Height of *Snowdon* Hill in *Caernarvanſhire* very accurately, and finding it to be 3720 Feet above the Level of the Sea, tried how much lower the *Mercury* wou'd ſtand in the Barometer upon that Hill, than at the Level of the Sea, and obſerv'd it to ſubſide 3 9 Inches. I am ſenſible that it will be alledged, that the Air will be denſer than I may imagine on the Top of high Hills, becauſe of the great Cold, ſince they are generally cover'd with Snow; but then we are to conſider, that when we are got above a Mile higher than the Level of the Sea, the incumbent Atmoſphere has loſt almoſt a 5th Part of its Weight *; and therefore the Air at the Top of the Hill, being ſo much leſs preſs'd, will, notwithſtanding the intenſe Cold, be more rarified than at the Bottom of the Hill.

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* *If the whole Air was reduced to the ſame Denſity, as it is near the Level of the Sea, the Atmoſphere wou'd be but between five and ſix Miles high, whereas, in its preſent State of gradual Rarefaction, it is above 50 Miles high, as we find by ſeveral* Phænomena *of Meteors obſerv'd to float in the Air, ſo high at leaſt.*

Now if we go back to the Obſervations of the Barometer, made by the Gentlemen that drew the Meridian in *France*, we ſhall find, that on the Mountain of *Rhodez*, in the Latitude of 44°, 21′ the Barometer fell 24 Lines below the Level of that in the Obſervatory, and they allow'd only 274½ Toiſes to correſpond to that Fall; whereas, according to Dr. *Halley*'s Proportion of a Tenth of an Inch for 90 Feet, they ſhou'd have taken 300 Toiſes; and tho' the *Hypotenuſes* A B, and B C, * were taken longer than the bare Declivity of the Mountain (which wou'd make the Error leſs than the 79 Toiſes I mention'd above) yet if my Proportion be made uſe of, *viz.* of 80 Feet for each Line of *Mercury*, that will make the Mountain 320 Toiſes, which, being higher, will therefore ſhew the Baſe to be yet ſhorter, and conſequently the Error, at that Rate, will be greater.

This Error (and ſuch like, if any more were made) will encreaſe the Meaſure of the 44th Degree of Latitude on the Earth; and, by obſerving what was done in the next Degree, we ſhall find that that Degree was taken too ſhort. In the Latitude of 45°, 38′, the Mountain of *Coſte* is made 815 Toiſes high; whereas the 54 Lines of the falling *Mercury* in the Barometer, ſaid to anſwer to that Height, will give but 705,6 Toiſes (which we will call 705,5) even according to my Computation of 80 Feet to a Line, which is the greateſt Allowance. If we ſuppoſe this Mountain to riſe in an Angle of 26°, 33′, as we did that of *Rhodez*, the Sides of the Mountain, or *Hypotenuſes* A B, and B C, † will be each equal to 1577,54 Toiſes, and the whole Baſe A C, to 2822 Toiſes. Now, when the Height of this Mountain is call'd 815 Toiſes, the Baſe A D, or D C

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* Fig. III. † Fig. IV.

(*by Eucl.* 47. 1.) becomes only equal to F D or D G = 1350,7 Toiſes, and its Double, or FG the whole Baſe, will be but 2701,4 Toiſes, leſs than the former by 120,6 Toiſes. This Error is ſo great (ſo much more than the Difference of 31 Toiſes for a Degree) that tho' I ſuppos'd the Lines found by Trigonometry, which terminate at the Top of the Mountain, to be much longer than the Hypotenuſe A B, yet there will be Error enough to make the 45th Degree of Latitude appear much ſhorter than it is. Suppoſing (becauſe of the Length of the Lines A B, or the great Diſtance from which the Mountain might be obſerv'd) that theſe Errors were four times leſs than I made them; yet, at that Rate, one muſt add near 20 Toiſes to the 44th Degree of Latitude, and take away above 30 from the 45th Degree, which will make the 44th of 57080 Toiſes, and the 45th, of only 57030; and this will give a Difference of 50 Toiſes; ſo that if an Angle can be taken to two or three Seconds, to which 32, or 48 Toiſes, are ſaid to anſwer upon the Surface of the Earth, ſuch a Difference might be viſible.—— And much more ſo, if other Errors of the ſame Kind ſhou'd happen to have been made the ſame Way; or if thoſe Errors were nearer my firſt Suppoſition than this laſt. Nay, tho' the 45th Degree of Latitude may be 13 Toiſes bigger than the 44th, it might by this Means appear to be conſiderably leſs.

Such a Miſtake might be the Occaſion of making the Hypotheſis of the Earth being an oblong Spheroid, eſpecially becauſe in this Hypotheſis, the Degrees differ moſt in Length from one another about the 45th Degree; and, when once an Hypotheſis is ſet on Foot, we are too apt to draw in Circumſtances to confirm it; tho', perhaps, when examin'd impartially, they may rather weaken, than ſtrengthen our Hypotheſis; otherwiſe, the Author of the Hiſtory of the *Royal Academy*, for the

Year

1713, wou'd not have alledg'd, that *the late Monſ.* Caſſini *obſerv'd* Jupiter *to be oval*, as a Proof of young Monſ. *Caſſini*'s Hypotheſis; becauſe *Jupiter* is oval the other Way, that is, an oblate Spheroid flatted at the Poles, as the ſaid late Monſ. *Caſſini* gave the Proportion of the Axis, to the Æquatorial Diameter, to be as 15 to 16.——— And our late ingenious Aſtronomer, the Reverend Mr. *Pound*, with a Teleſcope of 123 Foot *Focus*, and an excellent Micrometer, has given thoſe Proportions as 11 to 12. If a Proof is to be drawn from Analogy, or what is obſerv'd in other Planets, this muſt deſtroy Monſ. *Caſſini*'s Hypotheſis, and confirm Sir *Iſaac Newton*'s.

The Opinion of Dr. *Burnet* (the Theoriſt) quoted in the Memoirs, for the Year 1713, is but a very weak Argument in favour of Monſ. *Caſſini*'s Hypotheſis, on Account of the Reaſon Dr. *Burnet* give, to prove the Earth higher at the Poles, than the Æquator; for he ſays, "That the Velocity of the Parts of the Earth, "in its Diurnal Rotation, being greater at the Æqua-"tor than towards the Poles, all the Water muſt be "driven towards the Æquatorial Regions; from "whence, being repelled by the Reſiſtance of the "Air, it muſt run off again towards the Poles; and "ſo the Figure of the Water was lengthen'd out into an "oblong Spheroid, and conſequently the Cruſt of the "Earth over it did put on the ſame Figure, &c.

But why the Air ſhou'd reſiſt more towards the Æquator than the Poles, the Doctor did not give any Reaſon to ſhew; and, if it had been ſo, the ſame Force, that drove the Water towards the Æquator, muſt have kept it there. The Doctor, in the latter Part of his Aſſertion, forgot what he had ſaid in the former; for the Water cou'd not run off towards the Poles, whilſt the Earth continued its Rotation with the ſame Velocity.

For if he had conſider'd, he wou'd have found his Argument in other Words to be this. *Becauſe Bodies, that move in a Circle, always endeavour to recede from the Axis of their Motion; therefore the Water, by that Endeavour, comes nearer to the Axis of its Motion*; which is abſurd. But Dr. *Burnet*, afterwards, alter'd his Opinion, as I am credibly inform'd.

Having thus given my Reaſons for diſapproving of Monſ. *Caſſini*'s Opinion, concerning the Figure of the Earth; I come now to conſider Sir *Iſaac Newton*'s, who makes it higher at the Æquator, than at the Poles; but before I enter upon it, I beg Leave to quote a Paragraph out of the Hiſtory of the *Royal Academy* for 1713. Theſe are the Words of the Author. "Reaſonings drawn from the different Lengths of a Pendulum in different Climates, or from the Inequality of the Centrifugal Force ariſing from the Diurnal Motion of the Earth, are, perhaps, too nice to produce a certain Conviction; nay, perhaps, we are not well enough aſſured of the Principles, and the Conſequences may ſometimes be different. And therefore it is evident, that the beſt Way in this Enquiry, is only (as Monſ. *Caſſini* does) to make uſe of unqueſtion'd Obſervations, which ſerve directly to decide the Queſtion.

That Monſ. *Caſſini* has not made uſe of *unqueſtion'd Obſervations*, and the Meaſures, he mentions, are not able to decide the Queſtion, appears from what I have already ſaid. We muſt therefore ſhew, whether the Principles, from which Sir *Iſaac Newton* has deduced his Figure of the Earth, are fully prov'd or not: Whether the Concluſion drawn from them is plain and evident; and whether the Experiments on Pendulums, that confirm the Theory, are eaſy to be made, and may be depended upon.

Tho' Sir *Iſaac Newton*, in his *Principia*, has not endeavour'd to give the Cauſe of Gravity, or to determine whether it be owing to an Impulſe or not; yet he has ſhewn what its Effects and Laws are, from plain Experiments made by others and himſelf. From the Laws of Gravity, and from the Obſervation of a Comet, * he has deduced the Annual Motion of the Earth; and it muſt have a Diurnal Motion, if it has an Annual one, otherwiſe, it will not agree with the *Phænomena.* The Laws of the Centrifugal Force, or that Force by which a Body, whirl'd round in any Circle, endeavours to recede from the Center of its Motion, have been demonſtrated by Monſ. *Huygens.*

Theſe are the Principles from which Sir *Iſaac Newton* draws his Concluſion; and tho' ſome Perſons, that will not be at the Pains to examine them, may deny them by the Lump, yet no Body has yet been able to ſhew any Flaw in the Demonſtrations that relate to them.

NB. *This Diſſertation will be continued in our next.*

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* *Princip. Lib.* 3. *Prop.* 12, 13, & 42.

II. *Some*

II. *Some Obſervations made in an* Oſtrich, *diſſected by Order of Sir* Hans Sloane, *Bart.* *By Mr.* John Ranby, *Surgeon.* *F. R. S.*

HAving ſeparated the Muſcles of the *Abdomen*, which in this Subject were only two oblique Pair, we obſerv'd, between their *Tendons*, which were very ſtrong, and the *Peritonæum*, which was exceeding thin, a thick Layer of ſevous Fat, whoſe Office, conſidering the Smallneſs of the *Epiploon*, and the few adipoſe Veſicles of the *Meſentery*, with the Thinneſs of the *Peritonæum*, might probably be to ſupply the Part both of *Epiploon* and *Meſentery* in other Animals, as to lubricating the Inteſtines.

There were, in our Subject, two diſtinct Ventricles, contrary to the Obſervation of the *Royal Academy* at *Paris*. The firſt, and in its natural Situation, the lower, which the Members of the ſaid Academy, call the Craw, and ſuppoſe to be only a Dilatation of the *Oeſophagus*, was conſiderably larger than the ſecond, and uppermoſt Muſcular One; beſides, that it had ſtrong Muſcular Fibres, both circular and longitudinal: The *Duodenum* comes immediately out of the ſecond Ventricle.

Both Ventricles were diſtended beyond their uſual Form, and fill'd up with ſo large a Quantity of Food of different Kinds, as Stones, Bones, Sticks, Grain, and other Food, that it was almoſt impoſſible for them to perform their Office of Digeſtion, which very likely was one of the chief Cauſes of the Animal's Sickneſs and Death; and, really, the Contents of both ſeem'd to have undergone but very little or no Alteration.

The *Epiploon* partly cover'd the firſt Ventricle, but was no Ways proportionable to the Size of the Animal.

The Spleen was faſten'd, by a Membrane, to the right Side of the ſecond Ventricle, and was very ſmall, conſidering the Size of the Animal.

The Glands of the *Meſentery* were hardly viſible, but the Veins and Arteries very conſpicuous.

The *Cæcums*, in our Subject, were near three Foot in Length, the Diameter one Inch eight Lines; they were faſten'd to the *Ileum*, and not to the *Colon*, as the Gentlemen of the *Royal Academy* aſſert.

To their Deſcription of the Kidneys, I have nothing to add, except that the two *Ureters* lay upon their Surface, as they do in other Birds, and that their different Branches, coming from all the Parts of the Kidney, of which the ſuperior was very conſpicuous, enter'd the Kidney about its Middle, and form'd there a very large *Pelvis*.

The Liver was in one Cavity with the Heart, of which it cover'd near one half; it had no Gall Bladder, and but one *Ductus Bilarius* inſerted into the *Duodenum*, about two Inches below the *Pylorus*, which ſeem'd to have an immediate Communication with the *Vena Portæ*, becauſe, by blowing into it, this latter was alſo diſtended. The Heart and Liver were ſeparated from the Inteſtines, by a membranous *Diaphragm*.

Both Heart and Liver were ſuſpended by one common *Mediaſtinum*, by the Help of its ſeveral Membranes, and eight ſtrong Muſcles on each Side, ariſing from the upper Part of the Ribs, going from thence over the Lungs, and ending in a very ſtrong Tendinous Membrane, which is inſerted into the *Spina Dorſi*.

The Liquor, contain'd in the *Pericardium*, was ſmall in Quantity, and perfectly tranſparent.

The Lungs lay under the *Diaphragm* and its Muſcles, in a deep Cavity, form'd by the five true Ribs. They were pretty thick about the middle, and exceeding thin and ſharp towards the Extremities.

In viewing the Eye external, it did ſomewhat reſemble the Human Eye, except that it was leſs Convex, with a free and moveable upper Eye-lid, with Eye-laſhes, as moſt Terreſtrial Animals have, beſides a *Tunica Nictitans*, as in other Birds. Beſides the ſeven Muſcles of the Eye, as they are in Brutes, it had two more, one ariſing from the fore-part of the *Sclerotica*, which ſoon form'd a ſmall *Tendon*, obliquely ſurrounding the Optic Nerve, and then join'd to another Muſcle, which ariſes oppoſite to the former, from which the *Tendon* continues its Way, and is inſerted in the *Tunica Nictitans*. The *Aqueous* Humour we found in greater Quantity, than is common. The *Cryſtalline* was of an uniform Subſtance, but leſs Convex on the inſide, then without. The *Vitreous* was ſmall in Quantity, conſidering the Largeneſs of the Eye; the *Choroides* was entirely black, without that Variety of Colours at its Bottom, which is common to moſt Brutes. The fore-part of the *Sclerotica*, where it is annexed to the *Cornea*, was bony, conſiſting of 15 bony Scales join'd one to another, ſo as to make one circular Bone round the *Cornea*.

As for a more particular Deſcription, I refer to the Anatomical Account given by the *Royal Academy* at *Paris*, in their *Natural Hiſtory of Animals*, and to *Valliſneri*, Profeſſor at *Padua*, his *Notomia del Struthio*.

References to the Figures.

Fig. V.

Shews the upper Part of the *Thorax*, the *Sternum* being remov'd, with the Heart and Liver and neighb'ring Parts, in their natural Situation.

A A. The Membranous *Diaphragm*, in which are obſerv'd ſeveral diſtinct Cavities.

a a a. The Ligament that ſuſpends the *Diaphragm*.

b b. The Ribs.

B. The Heart. C C. The two Lobes of the Liver immediately above the Heart.

c c. The Brachial Artery. *d*. The Vein.

e e. *Vena Cava*.

f. A *Gland*, on the Brachial Artery.

g g. Part of the *Aſpera Arteria*.

h h. Part of the *Oeſophagus*.

i i. Two Muſcles ariſing from the *Sternum*, and inſerted into the *Aſpera Arteria*.

Fig. VI.

The inferior Part of the *Thorax*, the Heart and Liver being remov'd.

A A A. The lower Part of the *Diaphragm*, immediately covering the Lungs.

B B, &c. Eight ſtrong fleſhy Muſcles ariſing from the Ribs, and inſerted into the *Diaphragm*, forming a Cavity for the Heart and Liver.

c c c. The Ribs. D. The deſcending Trunk of the *Aorta*.

E E. The left Lobe of the Lungs freed from the *iaphragm*.

F. Part of the *Aſpera Arteria.*

Fig. VII.

Part of the Globe of the Eye.

a. The *Cornea*. *b b*. The Ciliary Ligament.
c c c. The fore-part of the *Sclerotica*, compos'd of 15 bony Scales.

Fig. VIII.

The back-part of the Globe.

a a a. The back-part of the *Sclerotica*.
b b b, *&c*. The ſeven Muſcles.
c c c. The eighth and ninth; the *Tendon* of which (*d d*) goes round the Optic Nerve, *f*, and is inſerted into the *Tunica Nictitans*.
e e. *Membrana Nictitans*.

Fig. IX.

The Kidneys with their Veſſels.
A A The Kidneys.
B B. *Aorta deſcendens*.
C C. *Vena Cava*.
D D. The Emulgent Arteries.
E E. The Emulgent Vein with its Ramifications.
F F. The Ureters.
G. The Union of the ſuperior and inferior Ureter.

III. *An Account of the Appearance of* Mercury, *paſſing over the Sun's Disk, on the 29th of* October, 1723 *determining the mean Motion, and fixing the Nodes of that Planet's Orb.* *By* Edmund Halley, *LL. D. Aſtron. Reg. and R. S. S.*

THE Tranſit of the Planet *Mercury*, over the Diſk of the *Sun*, being one of the moſt curious and uncommon Appearances that the Heavens afford, our Aſtronomers, both at home and abroad, made due Preparation to obſerve, with the utmoſt Exactneſs, that which happened on the 29th of *October*, 1723, which I had predicted in the Year 1691 (*Phil. Tranſ.* N° 193.) would be, in Part, viſible in *England*. And the Sky proving, more than ordinary, favourable at that Time, we were enabled to obſerve the Ingreſs on the *Sun*'s Limb, with the greateſt Accuracy.

Accordingly, the ſame Day, *Octob.* 29. *ſtyl. vet.* at *Greenwich* in the *Royal Obſervatory*, I firſt perceiv'd, with my 24 Foot Tube, the *Planet* making a ſmall Notch in the *Sun*'s Limb at $2^h\ 41'\ 23''$ *T. app.* And at $2^h\ 42'\ 26''$ he was wholly enter'd, making an interiour Contact, the Light of the *Sun*'s Limb juſt beginning to appear behind his dark Body; which, notwithſtanding the Slowneſs of the Motion, was, in a Manner, inſtantaneous. Then, applying the *Micrometer* to the ſaid 24 Foot Tube, I open'd it ſo as to take in $16'\ 15''$ equal to the *Sun*'s Semidiameter at that time; and cauſing the northern Edge of the *Sun*, to move exactly along one of the Pointers, I waited till the Center of

Mercury came to move along the other, as I found it to do at 3^{h} 1′ 16″ *T. app.* But *Refraction* contracting this Difference of Declination about 5 Seconds (the *Sun* being then but about 11° high) I concluded that the Centers of the *Sun* and *Mercury*, were truly in the ſame Parallel of Declination at 3^{h} 3′ *T. app. proxime.*

At *Wanſted* in *Eſſex* my worthy Collegue, the Rev. Mr. *James Bradly*, *Savilian* Profeſſor of *Aſtronomy*, obſerv'd with the *Hugenian Teleſcope*, of above 120 Foot long, the total Immerſion, or interiour Contact of the Limbs, at 2^{h} 26′ 45″ *T. æq.* that is 2^{h} 42′ 38″ *T. app.* twelve Seconds later than I found it at *Greenwich*; moſt of this Difference being due to the Difference of our Meridians. And applying the *Micrometer* to that vaſt *Radius*, he meaſured the Diameter of the *Planet* 10″ 45‴. At 2^{h} 48′ 57″ he found the Difference of Declination between the ſouthern Limbs of the *Sun* and *Planet* by the *Micrometer*, in a fifteen Foot Tube, to be 15′ 19″. Wherefore, allowing the obſerv'd Semidiameter of the *Planet*, and the *Refraction*, the ſaid Difference was neareſt 15′ 30″, and conſequently, *Mercury* more ſoutherly than the *Sun*'s Center in reſpect of Declination 0′ 45″.

Mr *George Graham*, in *Fleet-ſtreet*, *London*, obſerv'd the firſt Impreſſion on the *Sun*'s Limb at 2^{h} 41′ 9″ *T. app.* and at 2^{h} 42′ 19″ *Mercury* was intirely within the Disk. At 3^{h} 6′ 41″ he meaſur'd with a *Micrometer*, in a twelve Foot Tube, the Diſtance of his Center from the neareſt Limb of the *Sun* 2′ 13″. And again, at 3^{h} 25′ 24″ their Diſtance was found 3′ 57″. At 3^{h} 34′ 43″ he meaſur'd the Difference of Declination, from the northern Limb of the *Sun* 14′ 57″, which, corrected by *Refraction*, becomes 15′ 4″, that is, 1′ 11″ more northerly than the *Sun*'s Center.

In the *Obſervatory* at *Paris* Signor *Maraldi* obſerved the firſt Appearance of *Mercury* on the *Sun's* Limb at $2^{h}\ 50'\ 13''$ *T. app.* and the interiour Contact at $2^{h}\ 51'\ 48''$. And Mr. *de Liſle*, obſerving a-part, concluded the ſame at $2^{h}\ 51'\ 37''$, but ſuſpects it might have been ſome few Seconds later. This Gentleman has communicated his Obſervation at large, from whence we ſhall only borrow the following obſerved Latitudes.

	h	′	″		′	″
At	2	56	20	*Latitudo Borea Mercurii*	3	36
	3	00	40		3	42
	3	10	20		3	46
	3	16	12		3	55

At *Bononia*, in *Italy*, Signor *Manfredi* obſerved *Mercury* indenting the *Sun*'s Limb at $3^{h}\ 26'\ 22''$; and that he was gotten entirely within, at $3^{h}\ 27'\ 45''$. And theſe are the Obſervations moſt to be depended on, that we have receiv'd from abroad.

In order to deduce from this *Phænomenon*, ſo accurately obſerv'd, what may contribute to the Perfectioning of the Theory of *Mercury*'s Motion, which (as appears by the near Agreement of our Numbers with this and many other Obſervations of him) ſeems to need but very little Correction; I carefully computed, from our Tables, the Motion of the Planet in five Hours, and found his apparent Motion on the *Sun*, to be in Longitude 29′ 21″ Retrograde, and that his Latitude encreas'd northerly 4′ 17½″ in the ſame time; whence the Horary Motion in Longitude 5′ 52″, and in Latitude 0′ 51½″, and thence the Angle of the viſible Way with the Ecliptick 8° 19′, and the Horary Motion in that Way 5′ 56″. Again, the Angle of the Ecliptick with the Meridian, being in this Place 73° 24′, the viſible Way of *Mercury*, made an Angle of 65° 5′ with the Meridian paſſing through the Center of the *Sun*, whence the Horary Change of Declination becomes exactly 2′ 30″.

Theſe *Data* I chooſe rather to take from the Theory, than from immediate Obſervation; becauſe there is always an unavoidable, tho' ſmall Uncertainty, in what we obſerve, yet greater than there can be in the Computation for ſo ſmall a Space of Time, eſpecially now the Theory is, as I ſaid before, ſo very near the Truth.

This premiſed, let us now enquire the true Time of the central Ingreſs, and the Latitude of the Planet at that Time. And firſt, by my own Account, *Mercury* was gotten into the Parallel of the *Sun*'s Center, 21½ Minutes after the central Ingreſs, in which Time he aſcended to the *Northward* 0′ 54″, and ſo much, therefore, was he more *Southerly* than the *Sun*'s Center at his Ingreſs. Mr. *Bradly*, 7½ Minutes after the ſaid Ingreſs, in which the Planet aſcended 0′ 19″, found his Declination 0′ 45″ South, and therefore at the Ingreſs, his Declination was 1′ 4″ South. And by by Mr. *Graham*'s Obſervation, *Mercury* was more northerly than the *Sun*'s Center 1′ 11″, 53′ 20″ after the central Ingreſs; but in that Time, *Mercury* aſcended 2′ 13″, wherefore, according to him, at the Ingreſs the Planet had 1′ 2″ South Declination. We ſhall not therefore err above a Semidiameter of *Mercury*, if we aſſume his Declination, at that Time, to have been preciſely one Minute.

Now the *Sun*'s Semidiameter being then 16′ 15″, one Minute is the *Sine* of 3° 32′ in the Arch of the *Sun*'s Limb; and conſequently, the Point of this Ingreſs was 13° 4′ more northerly than the Ecliptick; whence the Latitude of *Mercury* was then 3′ 40″ North, and

Difference of Longitude 15′ 50″, by how much he, at that Time, follow'd the *Sun*'s Center.

If therefore, to the Arch of 13° 4′, we add the Double of 8° 19′, or of the Angle which the viſible Way made with the Eclip tick, we ſhall have 29° 42′ for the Point on the *Sun*'s weſtern Limb, at which the Planet made his *Exit*, likewiſe to the North of the *Ecliptick*. Hence the Chord, deſcrib'd in the whole Tranſit, was of 137° 14′, and the Chord itſelf 30′ 16″; and the neareſt Diſtance to the *Sun*'s Center 5′ 56″. Now the Horary Motion in this Chord, being 5′ 56″, the whole Duration of this *Mercurial Eclipſe* becomes 5h 6′ in reſpect of the Center of the *Planet*; and therefore the neareſt Approach of their Centers was at 5h 14′ 30″ at *Greenwich*, and the *Exit* at 7h 47′: both viſible in our *American Plantations*, had there been any curious Perſon there qualified to obſerve them.

It follows likewiſe, by the obſerv'd Diameter of *Mercury*, 10″ 45‴ that he was very little leſs than two Minutes of Time in paſſing the Limb; and, by the given neareſt Diſtance to the *Sun*'s Center, it is concluded that he was in Conjunction, in Point of Longitude, at 5h 23′ 15″ having then preciſely 6′ 00″ North Latitude. Nor can it be doubted, but that all this would have been found exceeding near to Truth, had not the too early ſetting of the *Sun* deprived all *Europe* of the deſirable Sight.

There being a very remarkable *Period* of the Motion of *Mercury* in 46 Years, in which Time, he makes 191 Revolutions about the *Sun*; this Tranſit of ours is found to have been preceded by two others at that Interval: The firſt, in the Year 1631, when *Gaſſendus* at *Paris*, on the 28th Day of *October*, *ſtyl. vet.* was the firſt that ever obſerv'd this Appearance of *Mercury* within the *Sun*'s Disk, and found him to paſs off at $10^{h}\ 28'$ *mane*. The ſecond was, *Octob.* 28° 1677, when myſelf had the good Fortune to obſerve both the Ingreſs and Egreſs of the Planet in the Iſland of *St. Helena*; the middle Time, when he was neareſt to the *Sun*'s Center, being there but $3'\ 50''$ paſt Noon, and the viſible Duration of the Tranſit of the Center of the Planet $5^{h}\ 14'\ 20''$; which was ſome ſmall matter contracted by Parallax, and moſt likely might have been $5^{h}\ 15'\ 00''$ without it. Now in $5^{h}\ 15'$, *Mercury* deſcrib'd the Chord of $146^{o}\ 52'$ in the *Sun*'s Limb, being $31'\ 9''$, and conſequently the neareſt Diſtance to the Center was $4'\ 38''$, or the Sine of $16^{o}\ 34'$ the *Sun*'s Semidiameter being Radius; that is, $1'\ 18''$ leſs than we found it in 1723. Hence alſo it follows, that the true Conjunction in Longitude was 7 *min.* of Time later than the neareſt Approach of the Centers, *viz.* at $0^{h}\ 10'\ 50''$ at St. *Helena*, or at $0^{h}\ 35'$ paſt Noon at *Greenwich*: and, that the North Latitude of the Planet, at that Time, was $4'\ 41''$.

Suppoſing, therefore, the neareſt Diſtance of the Centers in the Tranſit of 1631, to have been $3'\ 20''$, that is, $1'\ 18''$ leſs than in 1677, we ſhall find that *Mercury* then deſcrib'd a Chord of $156^{o}\ 20'$, traverſing the Diſk of the *Sun* in $5^{h}\ 21'\ 30''$; ſo that ſuppoſing his *Exit* at $10^{h}\ 28'$ at *Paris*, that is $10^{h}\ 18'\ 40''$ at *Greenwich*, he enter'd on the *Sun* at $4^{h}\ 57'\ 10''$ in the Morning; and was neareſt his Center at $7^{h}\ 38'$ *T. app.* but in the ſame Longitude with him at $7^{h}\ 43'$, or *Octob.* $27^{o}\ 19^{h}\ 43'$ *T. app.* having then $3'\ 22''$ North Latitude.

And here, I think I may, without Vanity, advertiſe the Reader, that above thirty Years ſince, *viz.* in *Philoſoph. Tranſ.* N° 193, for the Month of *March*, *&c.* $169\frac{0}{1}$, I predicted, by Help of the two former, this laſt Tranſit, with a ſurpriſing Exactneſs, even beyond my Hopes, making the Time of the middle, or neareſt Approach of the Centers of the *Sun* and *Mercury*, *Anno* 1723, *Octob.* 29^d 5^h $19'$ *T. app.* which we found by Obſervation at 5^h $14'\frac{1}{2}$, only $4'\frac{1}{2}$ Minutes ſooner ; and, in Latitude, *Mercury* was but ſix Seconds more ſoutherly than I then had computed it ; the Error, in Longitude, being little more than two Diameters of this exceeding ſmall Planet ; and, in Latitude, but a ſingle Semidiameter thereof. So, that for the Future, Aſtronomers may truſt my Table of theſe Tranſits, in *Tranſact.* N° 193, to a few Minutes of Time, and not wait with the Uncertainty of Hours, nay Days, as has lately been done.

But, in order to obtain a yet further Degree of Exactneſs by Help of this Obſervation, it may be moſt expedient to compare with it the Ingreſs I obſerv'd at St. *Helena* ; becauſe, in that, as well as in this, the Latitudes of the Planet being very ſmall, a little Error in them will not ſo much affect the Longitudes. Suppoſing therefore, that *Anno* 1677, *Octob.* 27° 21^h $26'$ $15''$ at St. *Helena*, or 21^h $50'$ $15''$ *T. app.* at *Greenwich*, the Center of *Mercury* entered on the *Sun*, and that, at that Time, he was $8\frac{1}{2}$ Degrees on the *Sun*'s Limb, to the North of the Ecliptick (according to what is above concluded) it follows, that he had then $2'$ $20''$ North Latitude, and $16'$ $5''$ greater Longitude than the *Sun*'s Center ; as in this preſent Tranſit, *Octob.* 29° 2^h $41'$ $30''$ *T. app.* at *Greenwich*, he had $3'$ $40''$ North Latitude, and $15'$ $50''$ more Longitude.

Now the apparent Geocentrick Differences of Longitude, are to the real Heliocentrick Differences, as the Planet's true Diſtance from the *Sun*, to his Diſtance from the *Earth* ; that is, in both Caſes, as 313 to 676 ; wherefore, in 1677, *Mercury* wanted $34'$ $45''$ of the Conjunction with the *Sun* ; and, in 1723, but $34''$ $13'$, at the Times of his apparent Ingreſs on the Disk. And, equating the Times, I find, that the *Sun, Anno* 1677, *Octob.* $27^{d}\ 21^{h}\ 34'\ 20''$ *T. æq.* was, in ♏ $15^{\circ}\ 36'\ 55''$ and, conſequently *Mercury*'s Heliocentrick Place ♉ $15^{\circ}\ 2'\ 10''$: and, *Anno* 1723, *Octob.* $29^{d}\ 2^{h}\ 25'\ 30''$ *T. æq.* the *Sun* was in ♏ $16^{\circ}\ 39'\ 43''$, and therefore *Mercury*, at that Time, in ♉ $16^{\circ}\ 5'\ 30''$.

Mercury therefore, in 46 Years with 11 Intercalations, and beſides $1^{d}\ 4^{h}\ 51'\ 10''$, has made 191 Revolutions to the Equinoctial Points, and over and above $1^{\circ}\ 3'\ 20''$. But, by the *Scholion* to *Prop.* XIV. *Lib.* III. *Natur. Philoſoph. Principia Math.* the Motion of the *Aphelion* of *Mercury*, from the Equinox in that Time, is $40'\ 18''$; ſo that there remains $23'\ 2''$ of *True Anomaly* to be reduced to the *Mean*: Now the Mean Anomaly of *Mercury*, in both Caſes, being 5 *ſig.* 12°, $23'\ 2''$ of True Anomaly gives $15'\ 24''$ Mean Anomaly; which added to $40'\ 18''$ becomes $55'\ 42''$, for the Mean Motion above ſo many Revolutions: and this is to be encreaſed by $8''$ to reduce it to the Plane of *Mercury*'s Orb, in all $55'\ 50''$.

Hence, doubling the Interval, in 92 *Julian* Years $1^{d}\ 9^{h}\ 42'\ 20''$, the Mean Motion of *Mercury* from the Equinox is $0^{s}\ 1^{\circ}\ 51'\ 40''$, from which, taking $5^{\circ}\ 44'\ 50''$ the Motion in $1^{d}\ 9^{h}\ 42'\ 20''$, we have his Motion in 92 *Julian* Years $11^{s}\ 26^{\circ}\ 6'\ 50''$, and in 100 Years, $2^{s}\ 14^{\circ}\ 2'\ 13''$, which is but $20''$ more than I had ſome Years ſince printed it, in my Aſtronomical Tables ſhortly to be publiſhed, and differs but one Hour's Motion therefrom in 3000 Years.

The forementioned Proportion of the Diſtances, *viz.* 313 to 676, is alſo between the Latitudes ſeen from the *Earth* and the Inclinations, or Heliocentrick Latitudes of the Planet: ſo that $2'\ 20''$, at the Ingreſs of 1677, gives $5'\ 2''$; and $3'\ 40''$ in 1723, becomes $7'\ 55''$

for the Latitudes at the *Sun.* And the Inclination of the Orb of *Mercury* to the Plane of the Ecliptick (determined by accurate Obſervations near his northern Limit) being 6° 59′ 20″, we compute the Diſtance of the Planet from his Node, in the former 0° 41′ 7″, and, in the latter, 1° 4′ 37″; which, being deducted from his Heliocentrick Places reſpectively, leave the Place of the aſcending Node, in 1677, ♉ 14° 21′ 3″; and, in 1723, ♉ 15° 0′ 53″: So, that in 46 Years the Node is found 39′ 50″ forwarder in the Ecliptick; which is but 1′ 30″ more than the Præceſſion of the Equinox in the ſame Time. We may therefore ſafely aſſume the Plane of the Orb of *Mercury* to be immoveable in the Sphere of fix'd Stars, and its aſcending Node to be ♉ 15° 41′ from the *firſt Star of* Aries. Nor can ſo very ſlow a Motion (ſuppoſing ſuch to be) be fully defined, but by the utmoſt Care and Diligence of future Aſtronomers, after the Obſervation of many Ages.

As to the reſt of the Theory of this Planet's Motion, I make his mean Diſtance from the *Sun*, 38710 ſuch Parts as the mean Diſtance of the *Sun* and *Earth* is 100000; and his greateſt Equation 23° 42′ 37″. The *Epocha* of his middle Motion, *ineunte Anno* 1723, *ſtyl. vet.* from the Equinoctial Point, I make ♐ 19° 9′ 31″; and that of his Aphelion to the ſame Time ♐ 13° 3′ 34″: the Aphelion moving *ſecundum Seriem Signorum*, ſeven Minutes in eight Years. And theſe Numbers I preſume, may repreſent the Motion of *Mercury*, with an Exactneſs equal to that of any of the other Planets; perhaps as near as the *Sun*'s Place by any Tables, or thoſe of the fixed Stars by any Catalogue yet extant.

It were to be wiſhed, that ſome good Obſervation, like this, had been made of the like Tranſit of *Mercury* at his other Node in *April*, where he was ſeen in-

deed *April* 23° 1661, but ſo imperfectly, that neither Ingreſs nor Egreſs was any where obſerv'd; and, though it be certain, that he traverſed the *Sun* on *April* 26°, 1674; and again *April* 24, 1707, yet we were ſo unfortunate, that the Conjunction in both happened ſo near Midnight, that he eſcaped unſeen by all the Aſtronomers of *Europe*, excepting ſingly Mr. *Roemer* at *Copenhagen*, whoſe Obſervation I have lately received by the Favour of Mr. *De l' Iſle* the Aſtronomer, communicated in the Words of the Manuſcript Journal of Obſervations of the ſaid Mr. *Roemer*.
" *Hodie ſexto Maii* (*Anno* 1707) *hora matutina*
" 4^h 19', *ſpectabatur Mercurius in extremo margine*
" *Solis jamjam exiturus; altus ſupra imum ſolis*
" *marginem* ¼ *diametri ſolaris, & ad ſiniſtram in*
" *Tubo* (*ſc. invertente*) *Accuratius hæc determi-*
" *nare non licuit ob moram nimis brevem.*" It was great Pity, that he did not, at leaſt, eſtimate, how many Diameters of his Body he was diſtant from the Limb of the *Sun*, or what Part of a Diameter, if ſo near: But having examined this Obſervation, I find that the *Sun*, at that Time, was but juſt riſen, or rather riſing, and ſoon after entered into a Cloud, ſo that the Limb of the *Sun* could not be diſtinctly ſeen, it always undulating and ſparkling much, when ſo near the Horizon; in which Circumſtance, a juſt Obſervation could hardly be made.

Let us now ſee how our Numbers, corrected as above, will repreſent this Obſervation. *Anno* 1707, *April* 24° 16^h 19' at *Copenhagen* is 15^h 28' at *Greenwich*, but 15^h 24 20'' *T. æq.* To this Time, I find the *Sun*'s true Place ♉ 14° 50' 1'', and his Diſtance from the *Earth* 101005. The correct *Epocha* of *Mercury*'s mean Motion, for the Year 1707, is 3^s 13° 18' 45'', to which adding, for the reſt of the Time, 3^s 19° 9' 28'', we ſhall have his middle Motion at the Time of the Obſervation ♏ 2° 28′ 13″; and, taking his *Aphelion* in ♐ 12° 49′ 49″ therefrom, we have his mean Anomaly 10^s 19° 38′ 24″, and thereby the Equation to be added 12° 39′ 41″, and thence the Place of *Mercury* in his Orb ♏ 15° 7′ 54″. But the correct Place of the deſcending Node is ♏ 14° 46′ 25″, and therefore *Mercury*, being 21′ 29″ paſt the Node, had 2′ 36″ South Latitude at the *Sun*; and his Place, reduced to the Ecliptick, was ♏ 15° 7′ 45″, that is, 17′ 44″ paſt the Conjunction of the *Sun*, which diminiſhed in the Proportion of 5567 to 4533, or of the Diſtance of the Planet from the *Earth* to his Diſtance from the *Sun*, becomes 14′ 27″; and by ſo much was he paſt the Conjunction as viewed from the *Earth*. Again, by the ſame Proportion, his Geocentrick Latitude, at that Time, was 2′ 7″ South; and therefore, his apparent Diſtance from the *Sun*'s Center, was 14′ 37″; that is, but 1′ 18″ from his weſtern Limb; ſo that he might well be ſaid to be, *jamjam exiturus*.

But, that *Mercury* ſhould at that Time be ſo far northerly, as Mr. *Roemer*'s Words import, was abſolutely impoſſible; and, I am apt to believe, that ſo acute an Aſtronomer as Mr. *Roemer* was, could not himſelf be the Obſerver, but ſome Perſon leſs acquainted with theſe Matters; which the Words *ſpectabatur Mercurius*, inſtead of *Mercurium vidi*, ſeem to import. If he had then had North Latitude, he muſt needs have been ſeen in the *Sun* in *April* 1720, which we are aſſured he was not.

Laſtly, it may not be amiſs to advertiſe, that on the laſt Day of *October* 1736, *Mercury* will again traverſe the northern Part of the *Sun's* Disk, both Ingreſs and Egreſs being viſible to all *Europe*.

FINIS.

Numb. 387.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *March* and *April*, 1725.

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Philos. Transact. No 287

Fig. 1.

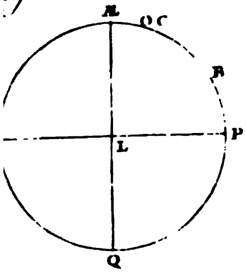


Fig. 3.

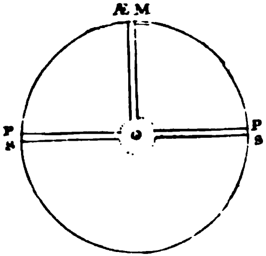


Fig. 4.

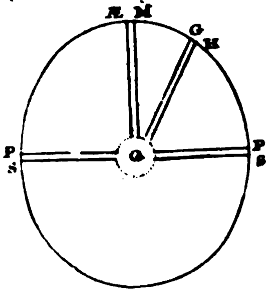
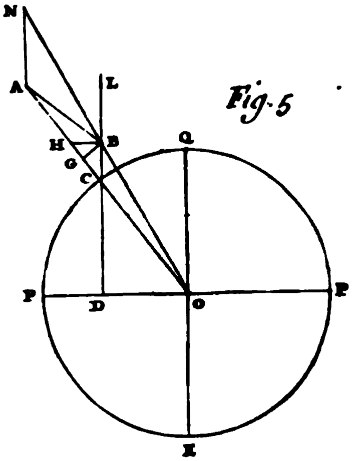


Fig. 5



S. Parker Fecit

I. *The* * *Diſſertation concerning the Figure of the Earth continued; by the Reverend* J. T. Deſaguliers, *LL. D. F. R. S.*

HOW the Figure of the Earth is deduc'd from the Laws of Gravity and Centrifugal Force, is very well ſhewn by the late *Savilian* Profeſſor of Aſtronomy, Dr. *John Keill*, in a Book that he wrote in the Year 1698, againſt Dr. *Burnet's* Theory of the Earth; and therefore I ſhall tranſcribe from him what he has ſaid upon that Subject; becauſe, otherwiſe, I ſhou'd only ſay the ſame Thing in other Words.

I own indeed that he has made a Miſtake in that Book concerning the Meaſure of the Degrees of an Ellipſe; but I find that all that relates to the oblate Spheroidical Figure of the Earth is right; and the little Difference of taking 15 *Paris* Feet for the Space that a Body falls thro' in a Second, inſtead of 15 Feet 1 Inch and 2 Lines, and a Number of Feet, a little leſs than true, for the Diameter of the Earth (which was not ſo well known at that Time) will no way invalidate his Demonſtration and Proof. Here follow his Words.

" To prove the Earth to be higher at the Æquator " than at the Poles, I will ſuppoſe firſt, that, at the Be- " ginning of the World, the Earth was fluid and ſpheri- " cal; but afterwards God Almighty having given it a " Motion round its own Axis, all Bodies upon the Earth " wou'd deſcribe either the Æquator or Circles, paral- " lel to the Æquator, and, by conſequence, all wou'd " endeavour to recede from the Center of their Motion.

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* *V.* Philoſ. Tranſact. N. 386.

“ It is to be here obſerv'd, that if a Body doth freely revolve in a Circle about a Center, as the Planets do about the Sun, that its Centripetal Force (or that Force by which it is drawn towards the Center) is always equal to its Force, by which it doth endeavour to recede from the Center; for the Force, which detains a Body in its Orbit, muſt be equal to the Force by which it endeavours to recede from its Orbit, and fly off in the Tangent. This may be clear by the Example of a Body turn'd round a Center by the Help of a Thread, which detains the Body in its Orbit; the Thread, being ſtretch'd by the Motion of the Body, will endeavour to contract itſelf equally towards both Ends, by which it will pull the Center as much towards the Body, as it doth the Body towards the Center.

“ Now this Centrifugal Force is always proportional to the Periphery, which each Body deſcribes in its diurnal Motion by the firſt *Theorem* of *Hugenius de Vi Centrifuga:* So that under the Æquator, which is the biggeſt Circle, the Centrifugal Force wou'd be greateſt, and ſtill grow leſs as we approach the Pole where it quite vaniſheth, there being there no diurnal Rotation. And without doubt, all Bodies having this Centrifugal Force, by which they endeavour to recede from the Center of their Motion, wou'd fly off from the Earth, if they were not kept in their Orbit by their Gravity, or that Force by which they are preſs'd towards the Center of the Earth, which is much ſtronger upon our Earth than the Centrifugal Force; and becauſe the Gravity upon the Surface of the Earth is always the ſame; but the Centrifugal Force alters and grows leſs, the nearer we come to the Poles, it is plain that the Gravity under the Æquator, having a greater Force to oppoſe it, than that

“ which is near the Poles, will not act ſo ſtrongly in “ the one Place as in the other, and conſequently Bodies will not be ſo heavy under the Æquator as at “ the Poles. ——— If the Circle * Æ P Q P repreſent “ the Earth, Æ Q the Æquator, and P P the Poles, if “ C be a Body in the Æquator, it is evident that it will “ be pull'd by two contrary Forces; namely, that of “ its Gravity, which pulls it towards the Center, and “ that of its Centrifugal Force, which pulls it from it. “ Now, if both theſe Forces were equal, it is evident “ it wou'd go neither of theſe Ways; but if one were “ ſtronger than the other, it wou'd move where the “ ſtrongeſt Force pulls it, but only with a Velocity “ which is proportional to the Differences of theſe two “ Forces, and therefore it wou'd not deſcend ſo faſt as “ if there were no Centrifugal Force, pulling againſt it; “ that is, a Body in the Æquator, does preſs leſs towards “ the Center, than at the Pole, where there is no Centrifugal Force to leſſen its Gravity. Bodies therefore, “ of the ſame Denſity, are not ſo heavy in one Place as “ in the other.

“ Now in a ſpherical Fluid, all whoſe Parts gravitate “ towards the Center, I think it is evident from the “ Principles of Hydroſtaticks and Fluidity, that all thoſe “ Bodies, which are equally diſtant from the Center, “ muſt be equally preſs'd with the Weight of the incumbent Fluid, and if one Part come to be more preſs'd “ than another, that which is moſt preſs'd will thruſt “ that out of its Place which is leaſt, till all the Parts “ come to an *Æquilibrium* one with another; and this “ is known by a common and eaſy Experiment, if you “ take a recurv'd Tube, † and fill it with Water or any “ other Fluid, it will riſe equally in both Legs of the

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* Fig. I. † Fig II.

“ Tube, ſo that the Surfaces CE and FI are equally
“ preſs'd by the incumbent Columns B C E D, and
“ G F I H, but if one of the Legs of this Tube ſhou'd
“ be fill'd with Oil, or ſome other lighter Fluid, and
“ the other with Water, the lighter Fluid will riſe
“ higher than the other, for otherwiſe, theſe Surfaces,
“ which are equally diſtant from the Center, wou'd not
“ be equally preſs'd.

“ Juſt ſo if P ÆMPS, * repreſents a fluid Sphere,
“ which we may imagine compoſed of a great many
“ communicating Canals or Tubes, the Fluid in every
“ one of which preſſes upon the Center; now if the
“ Fluid, in every one of theſe Tubes, was of equal
“ Weight or Gravity, it is plain, that, by that means,
“ they wou'd be alſo of an equal Height from the Cen-
“ ter; for by that means only, wou'd the Center be e-
“ qually preſs'd by the Weight of all the Tubes; but
“ if the Fluid, in the Canal ÆOM, were lighter than
“ the Fluid in the Canal POS, it is plain, that in this
“ Caſe, the Fluid POS, preſſing more on the Center,
“ than the Fluid in the Canal ÆOM, the Surface of
“ the Fluid ÆOM, will riſe to a greater Height or
“ Diſtance from the Center; ſo that by its greater
“ Height, which recompenſes its leſſer Gravitation, it
“ will preſs equally upon the Center with the Fluid in
“ the Canal POS. After the ſame manner, † if the
“ Fluid in the Canal GOH, were heavier than the
“ Fluid in the Canal ÆOM, but lighter than that
“ which is in POS, then wou'd the Canal GOH be
“ ſhorter than ÆOM, but longer than POS, and the
“ Figure compoſed of all theſe Tubes, wou'd be in the
“ Form of a Spheroid which is generated by the Cir-
“ cumrotation of a Semi-ellipſis round its Axis; but as I

Fig. III. Fig. IV.

« have already ſhew'd, that if ÆCM repreſent the Semidiameter
« of the Æquator, that all Bodies in it are
« lighter than in POS, the Axis of the Æquator
« (we take the Diameter and Axis here, not as pure
« Mathematical Lines, but as ſmall Canals or Tubes.)
« and juſt ſo thoſe Bodies which are in the Tube GOH,
« I have prov'd to be lighter than thoſe in POS, but
« but heavier than the Bodies which are in ÆOM,
« the Centrifugal Force in GH being leſs than that
« which is in ÆM, and there is no Centrifugal Force
« in the Poles PS. It is plain, therefore, that the Tube
« ÆOM will be longer than GOH, and GOH will
« be longer than POS, that is, the Diameter of the
« Æquator, will be longer than the Axis of the Earth,
« and conſequently the Figure of the Earth will be after
« the Faſhion of a broad Spheroid, which is generated
« by the Rotation of a Semi-ellipſis round its leſſer
« Axis. This, I hope, will be ſufficient to convince
« *Theoriſt* of the Falſeneſs of his own Aſſertion, ſince
« it is plain Demonſtration, than an Earth, form'd from
« a Chaos, muſt have a very different Figure from what
« he ſuppoſes it had.

“ But I will now proceed farther, and inquire how
“ much the Gravity is diminiſh'd at the Æquator, or
“ any other Parallel by the Centrifugal Force, which
“ all Bodies acquire by being turn'd round the Earth's
“ Axis, that from thence we may endeavour to determine,
“ what Proportion the Diameter of the Earth's
“ Æquator has to its Axis; to calculate which, I will
“ firſt ſuppoſe, that the mean Semidiameter of the
“ Earth is 19615800 *Paris* Feet, according to the late
“ Obſervations of the *French* Mathematicians, and ſince
“ the Earth turns round its Axis in the Space of 23
“ Hours, 56′, for in that Time, the ſame Meridian returns
“ to the ſame immoveable Point of the Heaven

“ again (but the Sun, in the mean time, ſeeming to be
“ mov'd a Degree, according to the Series of the Signs,
“ is the Cauſe why there are four Minutes more requir'd
“ before the Meridian can overtake him) from thence
“ it follows, that a Body, under the Æquator, moves
“ through 1426,88 Feet, in the Space of one Second of
“ Time. Now, according to the Theorem given us by
“ Sir *Iſaac Newton* in his *Philoſophiæ Naturalis*
“ *Principia Mathematica*, Schol. Prop. 4. Lib 1. The
“ Centrifugal Force of any Body has the ſame Proportion
“ to the Force of Gravity, that the Square of the
“ Arch, which a Body deſcribes in a given Time, divided
“ by its Diameter, has to the Space, through which
“ a heavy Body moves, in falling from a Place in which
“ it was at reſt in the ſame Time; and ſuppoſing a heavy
“ Body falls 15 Foot in a Second of Time, by Calculation,
“ it will from thence follow, that the Force of
“ Gravity has the ſame Proportion to the Centrifugal
“ Force at the Æquator, that 289 has to Unity; and
“ therefore by this Centrifugal Force which ariſes from
“ the Diurnal Rotation of the Earth round its Axis;
“ any Body, placed in the Æquator, loſes $\frac{1}{289}$ Part of its
“ Gravity, which it wou'd have were the Earth at reſt,
“ or which is the ſame Thing, a heavy Body plac'd at
“ either of the Poles (where there is no Diurnal Rotation,
“ and conſequently no Centrifugal Force) which
“ weighs 289 Pounds, if it were brought to the Æquator,
“ wou'd weigh only 288 Pounds.

“ Having thus determin'd the Proportion of the Centrifugal
“ Force, at the Æquator, to the Force of Gravity,
“ it will be eaſy from thence to ſhew their Proportions
“ in any Parallel; for it is compounded of the Proportion
“ of One to 289, and of the Co-ſine of the Latitude
“ to the Radius; for if two Bodies deſcribe different
‘ Peripheries in the ſame Time, their Centrifugal

“ Forces are proportional to their Peripheries, or to the
“ Semi-diameters of theſe Peripheries, as is determin'd
“ by Monſ. *Hugens*, in his *Theoremata de Vi Centri-fuga*
“ *& Motu circulari*; but the Periphery which
“ a Body in the Æquator deſcribes, has its Semi-diameter
“ equal to the Radius or Semi-diameter of the Earth,
“ and in any other Place, the Parallels, in which Bodies
“ move, have the Co-ſines of their Latitude for their
“ Semi-diameters, and therefore it will follow, that the
“ Force of Gravity is to the Centrifugal Force in a Proportion,
“ compounded of the Radius to the Co-ſine of
“ the Latitude, and of 289 to 1. and therefore at the
“ Latitude of 51° 45′ (for Example) it will be as
“ 466 to 1.

“ But we muſt obſerve, that it does not from thence
“ follow, that a Body in that Latitude loſes $\frac{1}{466}$ Part of
“ its abſolute Gravity, which it wou'd have, were the
“ Earth at reſt. For that cou'd not be, unleſs the Centrifugal
“ Force acted directly contrary to the Force of
“ Gravity, which it doth no where but at the Æquator;
“ for let the Circle * Q P E repreſent the Earth, Q E
“ the Diameter of the Æquator, O its Center, and let
“ B repreſent a Body which we ſuppoſe to hang by the
“ Thread A B, and to be placed any where between the
“ Pole P and the Æquator Q, and let B D be drawn perpendicular
“ to the Axis. It is plain, that if the Earth had
“ had no Diurnal Rotation, the Body B wou'd draw the
“ Thread A B into the Poſition of A C, ſince by that
“ means it deſcends as near as it can to the Center, and
“ there it wou'd ſtretch the Thread with all the Force
“ of its Gravity; or if we will ſuppoſe, that the Centri-fugal
“ Force acted according to the ſame Direction A C,
“ it wou'd then directly oppoſe the Force of Gravity,

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* Fig. V.

" and the Thread wou'd remain in the ſame Poſition, but it wou'd be ſtretch'd with a Force proportional to the Differences of theſe two Forces.

" But becauſe the Body B turns round the Center D, it will endeavour to recede from it according to the Line C B, in which Direction the Centrifugal Force acting, it will not directly oppoſe the Force of Gravity, but it will draw the Thread from the Poſition A C into the Poſition A B, let B G be drawn perpendicular to A C; if B C repreſent the Centrifugal Force, acting according to the Direction B C, it is equivalent (as is commonly known) to two Forces, one of which is as G C, and acts according to the Direction C G, which is contrary to that by which it deſcends to O, the other is as G B, and acts according to the Direction G B, which is no way contrary to the Force of Gravity. If therefore B C repreſent the total Centrifugal Force of the Body B, that Part of it, which directly oppoſes the Force of Gravity, will be G C; from whence it follows, that the Decreaſe of Gravity, in going from the Pole to the Æquator, is always as the Square of the Co-ſine of the Latitude; for draw B H parallel to the Axis P P, and becauſe the Triangles H C B, C D O are Equi-angular, therefore H C is to C B as C O is to C D, or as Q O is to C D, but Q O is to C D as the Decreaſe of Gravity at Q is to the Centrifugal Force at C. And therefore H C is to C B, as the Decreaſe of Gravity at Q, is to the Centrifugal Force at C. But if C B repreſent the Centrifugal Force at C, G C will repreſent that Part of it which acts directly againſt the Force of Gravity, and conſequently the Decreaſe of Gravity at the Æquator is to the Decreaſe of Gravity at C, as H C is to G C; now H C is to G C, in duplicate Proportion of H C to C B, or of C O or O Q to C D

" by the 8th of the 6th of *Euclid*, and therefore the " Decreaſe of Gravity at Q is to the Decreaſe of Gravity at C, as the Square of C O is to the Square of " C D, which was to be demonſtrated.

" From whence, it is plain, that if H C repreſent the " Decreaſe of Gravity at the Æquator, and G C its Decreaſe at C, then will G H repreſent the Difference " of theſe two Diminutions, or the Difference between " the Gravity at Q, and the Gravity at C, but H C is " to H G in duplicate Proportion of H C to H B, or " of O C to D O; that is, the Decreaſe of Gravity at " the Æquator is to its encreaſe at C, as the Square of the " Radius is to the Square of the Sine of the Latitude.

" By this alſo it will appear, that the Direction of " heavy Bodies is not to the Center of the Earth, as has " been always ſuppoſed; for if we take a heavy Body " and hang it by a Thread, the Thread produced will " not paſs through the Center any where but at the " Poles and the Æquator, for in the Figure the Thread " is carry'd by the Centrifugal Force of the Body B, " from the Poſition A C into the Poſition A B, where " it will reſt.

" Now to determine the Angle C A B, which the " Line of Direction of the Body makes with the Line " A C, let A N be drawn parallel to B C, and produce O B till it meet with it in N, and let us conſider " the Body B as drawn by three Powers, according to " three different Directions B O, B L, and A B, the " Power which pulls it, according to B O, is its Gravity, " that which draws it, according to the Direction B L, " is its Centrifugal Force, and that which acts according to A B, is the Strength of the Thread, by which " the Body is hinder'd to move according to either of the " two other Directions, and therefore it is an *Æquili-*

“ *brium* with the other two Powers; but by a Theorem which is demonſtrated by ſeveral of the Writers of Mechanics, but particularly by Monſ. *Huygens* in his ſmall Treatiſe *De Potentiis per Fila trahentibus.* If a Body be pull'd by three different Powers which are in *Æquilibrio* with one another, according to three different Directions, A B, B L and B O, theſe three Powers will be as the three Sides of the Triangle A B N, *viz.* as A B, A N and B N reſpectively; or as A B, B C and A C; B N being very near parallel, and conſequently equal to A C, ſince they do not meet but at a great Diſtance. From hence it follows, that the Force of Gravity is to the Centrifugal Force, as A C to B C. But a Method has been already ſhown, how the Proportion of the Force of Gravity to the Centrifugal Force may be determined, and therefore the Proportion of A C to B C may be be alſo determined, which at the Latitude of 51° 46′ is as 446 to 1. Therefore in the Triangle A B C, the Proportion of A C to B C is known, and the Angle A C B being equal to the Angle C O Q, which is ſubtended by the Arch C Q, the Latitude of the Place, from thence by the Tables of Sines and Tangents, the Angle B A C may be known, which in the abovementioned Latitude is about 5 Minutes.

“ From hence alſo it will appear, that it is not the Line A C, which being produced paſſes through the Center, but the Line A B that is perpendicular to the Curve P Q, for all the Particles of the Fluid will ſettle themſelves in ſuch a Poſition, that their Lines of Direction downwards, muſt be perpendicular to the Surface of the Body which they compoſe, for otherwiſe the Parts of the Fluid wou'd not be in *Æquilibrium* one with another, and therefore altho' the Lines of Direction of heavy Bodies do not paſs through the

" Center of the Earth, yet are they ſtill perpendicular
" to their Horizons; and, upon this Account, there
" cou'd ariſe no Error in levelling of Lines, and in find-
" ing the Riſings and Fallings of the Ground.

" Upon this account alſo it will appear, that the
" Surface of the Earth is not ſpherical, for if it were,
" then wou'd all Lines, drawn from the Center, be per-
" pendicular to the Surface of the Earth, ſince it is the
" known Property of a Sphere that they muſt be ſo;
" but I have already ſhew'd, that it is not ſo in the
" Earth, and therefore it is plain, that the Earth is not
" a Sphere. That therefore I may inquire more parti-
" cularly into the Figure of the Earth, I will reſume
" my former Hypotheſis, that the Earth is compoſed of
" an infinite Number of Canals, which communicate
" with one another at the Center, and are all equipon-
" derant, of which we will conſider two, as O Q and
" O C, and let O Q be $= r$, O D $=$ X and D C $= y$, let
" the abſolute Gravity be call'd p, and the Centrifugal
" Force at the Æquator n. O C is equal to $\sqrt{x^2 \times y^2}$
" the Weight of the Canal O Q is equal to the abſolute
" Gravity of the whole Canal *minus* the Centrifugal
" Force of each Particle contain'd in it, and becauſe the
" Centrifugal Force of each Particle is as its Diſtance
" from the Center, and therefore it encreaſes in an A-
" rithmetical Progreſſion, the greateſt of which is n,
" conſequently the Sum of all the Centrifugal Force is
" equal to $\frac{1}{2} n r$, but upon the Hypotheſis, that Gravity
" is the ſame at all Diſtances from the Center, the ab-
" ſolute Gravity of the Canal O Q is $p r$, and therefore
" its real Weight upon the Center O Q is $p r - \frac{1}{2} n r$,
" after the ſame Manner, the abſolute Gravity of the
" Canal O C is $p \times \sqrt{x^2 + y^2}$; but the Sum of all
" the Centrifugal Forces of all the Fluids in the Canal
" O C, is equal to the Centrifugal Force of the Fluid in

" C D

“ C D (as may be eaſily prov'd from the Conſideration
“ of inclin'd Planes) but the Centrifugal Force at C,
“ being to the Centrifugal Force at Q, as C D is to O Q
“ (that is, as y is to r) the Centrifugal Force at C will
“ be equal to $\frac{ny}{r}$, and becauſe the Centrifugal Force of
“ each Particle is as its Diſtance from the Point D,
“ which is the Center of the Circle that the Fluid in
“ the Canal C D deſcribes, and therefore the Centrifugal
“ Forces, in counting from the Point D, muſt encreaſe
“ in an Arithmetical Progreſſion, the greateſt of which
“ is $\frac{ny}{r}$, and therefore the Sum of all the Centrifugal
“ Forces in C D muſt be equal to $\frac{nyy}{2r}$, therefore the
“ Weight of the Canal O C is $= p\sqrt{x^2+y^2} - \frac{1}{2}$
“ $\frac{nyy}{r} = pr - \frac{1}{2}nr$, which Equation expreſſes the
“ Nature of the Curve that is made by the Section of
“ the Earth with a Plane through its Poles, and by this
“ the Proportion of the Axis of the Earth, to the Diameter of the Æquator, may be eaſily determin'd; for
“ when C O coincides with O P, then C D or y becomes equal to nothing, and the Equation is $p\sqrt{x^2}$
“ $= pr - \frac{1}{2}nr$ or $px = pr - \frac{1}{2}nr$, and therefore
“ by the 16th of the 6th, p has the ſame Proportion to
“ $p - \frac{1}{2}n$ that r has to x, or O Q to O D, but p is to
“ $p - \frac{1}{2}n$ as 289 is to 288½, or as 578 is to 577, which
“ therefore is the Proportion of the greateſt Diameter of
“ the Earth to the leaſt; but this is upon Suppoſition,
“ that Gravity is the ſame at all Diſtances from the
“ Center; but if we will ſuppoſe, that the Gravity of
“ Bodies without the Earth is in a Proportion reciprocal
“ to the Squares of their Diſtances from the Center, the

“ Gravity of thoſe Bodies, which are within the Earth, “ will be directly as their Diſtance, both which do beſt “ agree with the obſerv'd Phænomena of Nature; then “ will the Gravity at the Æquator be to the Gravity “ at the Poles as 689 to 692, which Numbers, in this “ Hypotheſis, do alſo expreſs the Proportion of the Dia- “ meter of the Earth, drawn through its Poles, to its “ Diameter drawn in the Plane of the Æquator.

“ It is upon the Account of this Diminution of Gra- “ vity, according as we approach the Æquator, that “ Pendulums of the ſame Lengths in different Latitudes “ take different Times to perform their Vibrations; “ for becauſe the accelerating Force of Gravity is leſs “ at the Æquator than under any Parallel, and under “ any Parallel it is ſtill leſs than under another which “ is nearer the Poles; it does plainly from thence fol- “ low, that a Body plac'd in the Æquator, or in any “ other Parallel, will take a longer Time to deſcend thro' “ an Arch of a given Circle, than it wou'd do at the “ Poles, and the farther a Body is remov'd from the “ Poles, the longer Time it will take to deſcend through “ any given Space.

“ From hence it follows, that the Length of Pendu- “ lums, which perform their Vibrations in equal Times “ in different Latitudes, are directly as the accelerating “ Forces of their Gravities; for the Time a Body takes “ to deſcend through an Arch of a Cycloid, is to the “ Time it will take to fall through the Axis of the Cy- “ cloid always in a given Proportion, *viz.* as the Semi- “ periphery of a Circle is to its Diameter by the 25th “ Prop. of *Huygen*'s *Horologium Oſcillatorium*; and “ therefore when the Times in which a Body deſcends “ through the Axes of two different Cycloids are equal, “ the Times of the Deſcent through the Cycloids will “ be alſo equal; but when the Times of the Deſcent

“ through the Axes are unequal, theſe Axes, and conſe-
“ quently the Lengths of the Pendulum which vibrates
“ in theſe Cycloids, are proportional to the accelerating
“ Forces of their Gravities.

“ By this if we know the Length of a Pendulum
“ which performs its Vibrations in a given Time, in
“ any one Part of the Earth, it is eaſy to determine the
“ Length of a Pendulum, which performs its Vibra-
“ tions in the ſame Time in any other Part of the Earth;
“ as for Example, the Length of a Pendulum, which vi-
“ brates Seconds at *Paris*, is three Foot eight Lines
“ and a Half, let it be requir'd to find the Length of a
“ Pendulum, which vibrates Seconds at the Æquator.
“ Becauſe the Gravity at the Poles is to the Gravity at
“ the Æquator, as 692 is to 689; therefore the De-
“ creaſe of Gravity at the Æquator is $\frac{1}{231}$ Parts of the
“ whole Gravity; but, as I have before demonſtrated, the
“ Decreaſe of Gravity at the Æquator is to its Encreaſe
“ in any other Latitude, as the Square of the Radius is
“ to the Square of the Sine of the Latitude, now the
“ Latitude of *Paris* being 48° 45′, its Sine is 75.183,
“ and therefore the Square of the Radius is to the
“ Square of the Sine of the Latitude as 1000000 to
“ 565248, but as 1000000 is to 565248, ſo is 3.000
“ the Number which repreſents the Decreaſe of Gra-
“ vity at the Æquator to 1. 695, the Number which
“ repreſents its Encreaſe at *Paris*, which added to 689
“ the Gravity at the Æquator, makes 690.695, the Num-
“ ber which will repreſent the Gravity at *Paris*. But
“ I have already ſhew'd, that as the Gravity at *Paris*
“ is to the Gravity at the Æquator, ſo is the Length of
“ a Pendulum which vibrates Seconds at *Paris*, to the
“ Length of a Pendulum that vibrates Seconds at the
“ Æquator, that is as 690, 695 to 689, ſo is 36,708 the
“ Length of a Pendulum at *Paris*, which performs its

“ Vibration in a Second to 36,616, which therefore is
“ the Length of a Pendulum which performs its Vibra-
“ tions in a Second at the Æquator; ſo that the Diffe-
“ rence between theſe two Pendulums is $\frac{92}{1000}$ Parts of an
“ Inch, which comes pretty near the Obſervations of
“ Monſ. *Richer*, who at the Iſland of *Cayenne*, whoſe
“ Latitude is 5° 00′ found that a Pendulum, which vi-
“ brates Seconds there, was a tenth Part of an Inch
“ ſhorter than a Pendulum, which vibrates Seconds at
“ *Paris*.

“ Thus we ſee that the Principles and Hypotheſis,
“ and withal their Conſequences, upon which the broad
“ ſpheroidical Figure of the Earth is founded, do exactly
“ agree with Obſervations, and therefore there is no
“ Doubt to be made, but that the Earth is really of ſuch
“ a Figure, and that the Hypotheſis upon which this
“ Figure is grounded (*viz.* the diurnal Rotation of the
“ Earth, and by conſequence the Centrifugal Force of
“ all Bodies upon it) muſt be admitted for a true one;
“ ſince the different Vibrations of Pendulums of the
“ ſame Length, in different Latitudes, can depend upon
“ no other Cauſe; for the Change of Air is not able to
“ produce any ſuch Effect, for if the Air made really
“ any Alterations in the Vibrations of a Pendulum, it
“ wou'd produce a quite contrary Effect than what is
“ obſerv'd; for Pendulums near the Æquator wou'd
“ move faſter than they wou'd do in Places of greater
“ Latitude, the Air in the one Place, being more rari-
“ fied, is much thinner and finer than it is in the other,
“ and therefore gives leſs Reſiſtance to Bodies that move
“ in it.

“ In this Reaſoning, we have ſuppos'd the Earth to
“ have been at firſt fluid, as the *Theoriſt* has done be-
“ fore us, but if we will put the Caſe, that the Earth
“ was firſt partly fluid and partly dry, as it is at preſent,

" yet becauſe we find that the Land is very near of the
" ſame Figure with the Sea (only rais'd a little higher
" that it might not be overflow'd) compoſing with it
" the ſame Solid, and I have already ſhew'd that the
" Surface of the Ocean is ſpheroidical and not ſpherical,
" there is no doubt to be made, but that the Land was
" form'd into the ſame Figure by its wiſe Creator at the
" Beginning of the World, for if it were otherwiſe, then
" wou'd the Land towards the Æquator have been over-
" flow'd with Water, which, as I have already prov'd,
" muſt have been higher at the Æquator than at the
" Poles; and therefore the Sea wou'd riſe there and
" ſpread itſelf like an Inundation upon all the Land."

To make an End of this long Diſſertation, let us in a few Words compare the Experiments and Obſervations made uſe of to confirm each of the Opinions abovementioned.

To prove Monſ. Caſſini's Figure of the Earth, we muſt take the Altitude of a Star nearer than to 2 Seconds; becauſe 2 Seconds anſwer to 32 Toiſes on the Surface of the Earth, and the Difference of the Length of Degrees is but 31. And what is more, we muſt take this Angle with an Inſtrument of 39 Inches Radius; becauſe the 10 Foot Sector was only us'd at the Ends of the two Parts of the Meridian.

To diſprove Monſ. Caſſini's Hypotheſis, we need only obſerve whether a Plumb-Line makes an Angle of of 5 Minutes with a Perpendicular to the Surface of ſtagnant Waters, or Lines of Level.

To prove Monſ. Caſſini's Opinion, the Height of a great many Mountains muſt be accurately meaſur'd by Trigonometry, which Mathematicians have always found very difficult.

To prove Sir Iſaac Newton's Opinion, we are only to meaſure about one Tenth of an Inch in a Rod of 39,129 Inches; and to know what to allow for the lengthening of the ſame Rod by the Summer Heat, when it is ſhut up in a Caſe, and carried towards the Æquator. For though the Experiments on Pendulums, made by ſeveral Perſons that travell'd Southward, differ among themſelves, yet they all agree in this, that the Obſervers were oblig'd to ſhorten their Pendulums, in order to make them ſwing Seconds, as they went towards the Æquator. And when we come to compare them together, in order to have the exact Proportion of Length in different Latitudes; we muſt rely on the moſt exact Experimenter, which we may very well do on Monſ. *Richer*: becauſe when he found a Difference, he was ſo careful to find out how much it was, that he caus'd a ſimple Pendulum to ſwing, and compar'd it with a good Pendulum Clock, which he did ſeveral Times every Week for 10 Months together; and when he return'd to *France*, he compar'd it with the Length of the Pendulum at *Paris*; which is of 3 Feet 8 $\frac{1}{2}$ Lines (or 39,129 *Engliſh* Inches) and found it to be ſhorter by 1$\frac{1}{4}$ Line.

II. *An Eſſay upon the Natural Hiſtory of Whales, with a particular Account of the Ambergris found in the* Sperma Ceti *Whale. In a Letter to the Publiſher, from the Honourable* Paul Dudley, *Eſq; F. R. S.*

SIR,

I AM very ſenſible that the World has long ſince, and often been entertained with various Relations of this wonderful Creature: But as I ſhall endeavour to avoid any vain Repetition, ſo I flatter myſelf with the Hopes of preſenting the Royal Society with ſome Particulars not yet publiſhed, both of the Whale in general, and more eſpecially of that Species or Kind of Whale, which our People call the *Sperma Ceti*, but, in my Opinion, much rather deſerves the Name of the Ambergris Whale. But here I would have it noted, that the following Account reſpects only ſuch Whales, as are found on the Coaſt of *New England*.

And of theſe there are divers Sorts or Kinds.

* As firſt, The Right, or Whalebone Whale is a large Fiſh, meaſuring ſixty or ſeventy Feet in Length, and very bulky, having no Scales, but a ſoft fine ſmooth Skin, no Fins, but only one on each Side, from five to eight Feet long, which they are not obſerved to uſe, but only in turning themſelves, unleſs while young, and carried by the Dam on the Flukes of their Tails; when with thoſe Fins they claſp about her Small, and ſo hold them-

ſelves on. This Fiſh, when firſt brought forth, is about twenty Feet long, and of little Worth, but then the Dam is very fat. At a Year old, when they are called Short-heads, they are very fat, and yield to fifty Barrels of Oil, but by that Time the Dam is very poor, and term'd a Dry-skin, and won't yield more than thirty Barrels of Oil, tho' of large Bulk. At two Years old, they are called Stunts, being ſtunted after weaning, and will then yield generally from twenty four to twenty eight Barrels. After this, they are term'd Scull-fiſh, their Age not being known, but only gueſs'd at by the Length of the Bone in their Mouths. The Whale-bone, ſo called, grows in the upper Jaw on each Side, and is ſometimes ſix or ſeven Feet in Length. A good large Whale has yielded a thouſand Weight of Bone. 'Tis thought by ſome, that the hairy Part of the Whale-bone, and which is next to the Tongue, ſerves in the Nature of a Strainer of their Food.

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* *The ſeveral Species of Whales.*

The Eye of a Whale is about the Bigneſs of an Ox's Eye, and they are ſituated in the After-part of the Head on each Side, and where the Whale is broadeſt; for his Head tapers away forward from his Eyes, and his Body tapers away backward; his Eyes are more than half way his Depth, or neareſt his Under-part; juſt under his Eyes are his two Fins before-mentioned; he carries his Tail horizontally, and with that he ſculls himſelf along.

The Entrails of this Whale are made and ſituated much like thoſe of an Ox, and their Scalps are ſometimes found covered with Thouſands of Sea-lice. One of theſe Whales has yielded one hundred and thirty Barrels of Oil, and near twenty out of the Tongue. The Whale-bone Whale is the moſt valuable, except the *Sperma Ceti* Whale.

The Scrag Whale is near a-kin to the Fin-back, but, inſtead of a Fin upon his Back, the Ridge of the After-part of his Back is ſcragged with half a Dozen Knobs or Nuckles; he is neareſt the right Whale in Figure and for Quantity of Oil; his Bone is white, but won't ſplit.

The Finback Whale is diſtinguiſhed from the right Whale, by having a great Fin on his Back from two Feet and an Half, to four Feet long, which gives him the Name; he has alſo two ſide Fins, as the Whale-bone Whale, but much longer, meaſuring ſix or ſeven Feet. This Fiſh is ſomewhat longer than the other, but not ſo bulky, much ſwifter, and very furious when ſtruck, and very difficultly held; their Oil is not near ſo much, as that of the right Whale, and the Bone of little Profit, being ſhort and knobby. The Belly of this Whale is white.

The Bunch or humpback Whale, is diſtinguiſhed from the right Whale, by having a Bunch ſtanding in the Place where the Fin does in the Finback. This Bunch is as big as a Man's Head, and a Foot high, ſhaped like a Plug pointing backwards. The Bone of this Whale is not worth much, tho' ſomewhat better than the Finback's. His Fins are ſometimes eighteen Feet long, and very white; his Oil much as that of the Finback. Both the Finbacks and Humpbacks are ſhaped in Reeves longitudinal from Head to Tail on their Bellies and their Sides, as far as their Fins, which are about half way up their Sides.

* The *Sperma Ceti* Whale. This Fiſh is much of the ſame Dimenſion with the other, but of a greyiſh Colour, whereas the others are black; he has a Bunch on his Back like the Humpback, but then he is diſtin-

guiſhed by not having any Whale-bone in the Mouth; but inſtead of that, there are Rows of fine ivory Teeth in each Jaw, about five or ſix Inches long. One of theſe Teeth I have done myſelf the Honour to ſend the Society. The Man, who gave it me, ſays, the Whale was forty nine Foot long, and his Head made twelve Barrels of *Sperma Ceti* Oil. They are a more gentle Fiſh than the other Whales, and ſeldom fight with their Tails; but when ſtruck, uſually turn upon their Backs, and fight with their Mouths. The Oil, which is made of the Body of this Fiſh, is much clearer and ſweeter than that of the other Whales.

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* *The* Sperma Ceti *Whale*,

* The *Sperma Ceti* Oil, ſo called, lies in a great Trunk about four or five Feet deep, and ten or twelve Feet long, near the whole Depth, Breadth, and Length of the Head, in the Place of the Brains, and ſeems to be the ſame, and diſpoſed in ſeveral membranous Cells, and covered not with a Bone, but a thick griſly Subſtance below the Skin, through which they dig a Hole, and lade out the clear Oil. Not but that the Head, and other Glandulous Parts of this Fiſh, will make the *Sperma Ceti* Oil; but the beſt, and that which is prepared by Nature, is in the Trunk aforeſaid: And an ingenious Man, who has himſelf kill'd many of theſe Whales, aſſures me, that only the Trunk will afford from ten to twenty Barrels. Beſides the *Sperma Ceti* Oil, this Fiſh will yield from twenty to fifty Barrels of common Oil.

One of our Country Doctors tells me, that the Tooth of this Fiſh, ſhaved or powdered, and ſo infuſed in Liquor, equals the Harts-horn, and has been uſed in the Small-Pox, and given to Lying-in-Women, in Caſe of Sickneſs, with Succeſs. The Quantity is as much as will lie upon an *Engliſh* Shilling. I meddle not here with the precious Ambergris found in this Whale, becauſe I deſign to cloſe the Whole with that Diſcovery.

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* Sperma Ceti *Oil.*

I go on therefore, in the ſecond Place, to give an Account of the Manner of the Propagation of Whales. They generate much like to our neat Cattle, and therefore they are termed Bull, Cow, and Calf. The *Latins* called the Whale *Bellua Marina*. They bring forth but one at a Time, and but every other Year. When the Cow takes Bull, ſhe throws herſelf upon her Back, ſinking her Tail, and ſo the Bull ſlides up, and, when the Bull is ſlid up, ſhe claſps him with her Fins. A Whale's Piſſel is ſix Feet long, and at the Root is ſeven or eight Inches Diameter, and tapers away till it comes to about an Inch Diameter: his Stones would fill half a Barrel, but his Genitals are not open or viſible, like thoſe of the true Bull. The Calf, or young Whale, has been found perfectly form'd in the Cow, when not above ſeventeen Inches long, and white; yet, when brought forth, is uſually twenty Feet, but of a black Colour; it is ſuppoſed they go with their Young about nine or ten Months, and are very fat in that Time, eſpecially when they bring forth. When the Female ſuckles her Young, ſhe turns herſelf almoſt upon her Back, upon the Rim of the Water, ſhe has two Teats of ſix or eight Inches long, and ten or twelve Inches round. The Milk is white, like that of a Cow; and upon opening a young ſucking Whale, the Milk was found curdled in his Bag, juſt like that of a Calf.

* 3. The Care of their Young is very remarkable, they not only carrying them on their Tails, and ſuckling them, but often riſing with them for the Benefit of the Air;

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* στόργη.

and however they are chaſed or wounded, yet as long as they have Senſe, and perceive Life in their Young, they will never leave them, nor will they then ſtrike with their Tail, and if, in their running, the young one loſes his Hold and drops off, the Dam comes about, and paſſing underneath, takes it on again. And therefore Care is taken by thoſe who kill theſe Mate Fiſh (as they are called) only to faſten the Calf, but not to kill her, till they have firſt ſecured the Cow. For ſo ſoon as ever the Calf is dead, the Cow perceives it, and grows ſo violent, that there is no managing her.

4. The Whales are very gregarious, being ſometimes found a Hundred in a Scull, and are great Travellers. In the Fall of the Year, the right or Whale-bone Whales go Weſtward, and in the Spring they are headed Eaſtward. But here it muſt be noted, that the ſeveral Kinds of Whales don't mix with one another, but keep by themſelves.

5. Reſpiration. Their Way of Breathing is by two Spout-Holes in the Top of the Head. The *Sperma Ceti* has but one, and that on the left Side of the Head. Once in a Quarter of an Hour, when not diſturbed, they are obſerved to riſe and blow, ſpouting out Water and Wind, and to draw in freſh Air: but, when purſued, they will ſometimes keep under Water half an Hour or more; tho' 'tis obſerved when any Cow has her Calf on her Tail, ſhe riſes much oftener (through a natural Inſtinct) for the young one to breath, without breathing herſelf. Out of their breathing Holes they ſpout great Quantities of Blood, when they have receiv'd their Death Wound.

6. Food or Suſtentation. For the firſt Year, as has been already obſerv'd, they all ſuck the Dam. After they are weaned, the right Whales, as is generally ſuppoſed, live upon ſome ouzy Matter, which they ſuck up from the

Bottom of the Sea. The Triers, that open them when dead, acquaint me, that they never obſerved any Graſs, Fiſh, or any other Sort of Food in the right or Whalebone Whale, but only a greyiſh ſoft Clay, which the People call *Bole Armoniac*; and yet an experienced Whale-man tells me, that he has ſeen this Whale in ſtill Weather, skimming on the Surface of the Water, to take in a Sort of reddiſh Spawn or Brett, as ſome call it, that at ſome Times will lie upon the Top of the Water, for a Mile together. Here alſo it may be obſerv'd, that tho' the Body of this Whale is ſo very bulky, and ſo exceeding fat; yet when cut open, they are ſeldom found to have much more Draught than that of an Ox, and they dung much as neat Cattle do. Their Swallow is not much bigger than an Ox's; but the Finback Whale has a larger Swallow: for he lives upon the ſmaller Fiſh, as Mackarel, Herring, *&c.* great Sculls of which they run thro', and, with a ſhort Turn, cauſe an Eddy or Whirlpool, by the Force of which, the ſmall Fiſh are brought into a Cluſter; ſo that this Fiſh, with open Mouth, will take in ſome Hundreds of them at a Time. The *Sperma Ceti* Whale, beſides other Fiſh, feeds much upon a ſmall Fiſh that has a Bill; our Fiſhermen call them Squid Fiſh. The ſmall Peices of theſe Squid Bills are plainly to be diſcerned in the Ambergris, and may be pick'd out of it; they appear glazy, and like little Pieces of broken Shells.

7. The Way and Manner of killing Whales.

Mr. *Harris* in his *Bibliotheca Navigantium*, &c. has given a very particular Account of the Method of taking Whales at *Greenland*, and though our Way in *New England* differs very much from that, yet I ſhall wave it, as not ſo ſtrictly appertaining to Philoſophy. Only I would take notice of the Boats our Whale-men uſe in going from the Shoar after the Whale: They are made of Cedar Clapboards, and ſo very light, that two

Men can conveniently carry them, and yet they are twenty Feet long, and carry ſix Men, *viz.* the Harpooneer in the Fore-part of the Boat, four Oar-men, and the Steerſ-man. These Boats run very ſwift, and by reaſon of their Lightneſs can be brought on and off, and ſo kept out of Danger. The Whale is ſometimes killed with a ſingle Stroke, and yet at other Times ſhe will hold the Whale-men in Play, near half a Day together, with their Lances, and ſometimes they will get away after they have been lanced and ſpouted blood, with Irons in them, and Drugs faſtened to them, which are thick Boards about fourteen Inches ſquare. Our People formerly uſed to kill the Whale near the Shore; but now they go off to Sea in Sloops and Whale-boats, in the Months of *May*, *June*, and *July*, between *Cape-Cod* and *Bermudas*; where they lie by in the Night, and ſail to and again in the Day, and ſeldom miſs of them; they bring home the Blubber in their Sloops. The true Seaſon, for taking the right or Whale-bone Whale, is from the Beginning of *June*, to the End of *May*; for the *Sperma Ceti* Whale, from the Beginning of *June*, to the End of *Auguſt*. And it has been obſerved by our Fiſhermen, that when a *Sperma Ceti* Whale is ſtruck, he uſually, if not always, throws the Excrements out of the *Anus*.

8. The wonderful, and even prodigious Strength of this Creature, which lies principally in their Tails, that being both their offenſive and defenſive Weapon. Many Inſtances of this Kind I have had from credible Perſons, who were Eye-witneſſes; I will mention but a few. A Boat has been cut down from Top to Bottom with the Tail of a Whale, as if cut with a Saw, the Clap-boards ſcarce ſplinter'd, tho' the Gunnel upon the Top is of tough Wood. Another has had the Stem, or Stern-poſt of about three Inches through, and of the

togheſt Wood that can be found, into which the Ends of the Cedar Clap-boards are nailed, cut off ſmooth above the Cuddee, without ſo much as ſhattering the Boat, or drawing the Nails of the Clap-boards. An Oar has been cut off with a Stroke upwards, and yet not ſo much as lifted up out of the Thole-Pin. One Perſon had an Oar cut off, while in his Hand, and yet never felt any Jarring.

A few Years ſince, one of the Finback Whales came into an Harbour near *Cape-Cod*, and tow'd away a Sloop of near forty Tun, out of the Harbour into the Sea. This Accident happened thus: It is thought the Whale was rubbing herſelf upon the Fluke of the Anchor, or going near the Bottom, got the Fluke into her Niſket, or the Orifice of the *Uterus*, and, finding herſelf caught, tore away with ſuch Violence, and tow'd the Sloop out of the Harbour, as faſt as if ſhe had been under Sail with a good Gale of Wind, to the Aſtoniſhment of the People on Shore, for there was nobody on board. When the Whale came into deep Water, ſhe went under, and had like to have carried the Sloop with her, but the Cable gave Way, and ſo the Boats that were out after her, recover'd it. This Whale was found dead ſome Days after on that Shore, with the Anchor ſticking in her Belly.

After a Whale is dead, it has been obſerved, that the ſame way the Head lies, ſo the Head will lie if not forcibly turned, and let the Wind blow which Way it will, that Way they will ſcull a Head, tho' right in the Eye of the Wind, and they are much eaſier tow'd to the Shore, if they die that Way with their Head, than any other.

9. The Enemies of the Whale, or the Fiſh that prey upon the Whales, and often kill the young ones, for hey won't venture upon an old one, unleſs much wounded.

Our Whale-men have given this Fiſh the Name of Killers. Theſe Killers are from twenty to thirty Feet long, and have Teeth in both Jaws that lock one within another. He has a Fin, near the Middle of his Back, four or five Foot long. They go in Company by Dozens, and ſet upon a young Whale, and will bait him like ſo many Bull-dogs; ſome will lay hold of his Tail to keep him from threſhing, while others lay hold of his Head, and bite and threſh him, till the poor Creature, being thus heated, lolls out his Tongue, and then ſome of the Killers catch hold of his Lips, and if poſſible of his Tongue; and after they have killed him, they chiefly feed upon the Tongue and Head, but when he begins to putrify, they leave him. This Killer is without doubt the *Orca*, that Dr. *Frangius* deſcribes in his Treatiſe of Animals. His Words are theſe, *Quando Orca inſequitur Balænam, ipſa Balæna horibilem edit Mugitum, non aliter quam cum Taurus mordetur a Cane.* Theſe Killers are of ſuch invincible Strength, that when ſeveral Boats together have been towing a dead Whale, one of them has come and faſtened his Teeth in her, and carried her away down to the Bottom in an Inſtant. And ſometimes again, they have bit out a Piece of Blubber of about two Foot ſquare, which is of that Toughneſs, that an Iron, with little Beards being ſtruck into it, will hold it till it draws the Boat under Water. The Killers are ſometimes taken, and make good Oil, but have no Whale-bone. The Carcaſes, or Bodies of dead Whales in the Sea, ſerve for Food for Gulls, and other Sea-Fowl, as well as Sharks, for they are not very nice.

* I come now to what I firſt and principally propoſed, in this Eſſay on the Natural Hiſtory of Whales, to give

an Account of the Ambergris. Many and various have been the Opinions even of the learned World, as to the Origin and Nature of this precious Perfume; ſome have reckoned it a *Bitumen*, and to iſſue from the Entrails of the Earth; others, that it was produced from ſome Inſect, as Honey, Silk, *&c.* The famous Mr. *Boyle*, as I find it, in the Second Volume of *Lowthorp*'s *Abridgment of the Philoſophical Tranſactions*, communicates an Account of Ambergris, from a *Dutch* Merchant, who firſt denies it to be the Scum or Excrement of a Whale, and then gives it, as his Opinion, that it is a fat Gum that iſſues from the Root of a Tree, and that you may raiſe it in Quantities by planting thoſe Trees by the Shore, and ſo the Stream will caſt it up to great Advantage. But Truth is the Daughter of Time; it is now at Length found out, that this *Occultum Naturæ* is an Animal Production, and bred in the Body of the *Sperma Ceti* Whale, analogous to what is found in ſome Animals of the Land, as the Muſk Hog or *Taiacu*, the Muſk Deer, the Bezoar Sheep, and ſome Amphibious Animals, as the Muſquaſh, *&c.* who have their valuable Scent in a particular *Cyſtis* or Bag. I am apt to think, that which firſt gave Occaſion to the Notion of Ambergris being the Production of the Whale, was becauſe it was found in conſiderable Quantities on the Shores of the Summer Iſlands, and among the *Bahama's*, where the dead Whales are frequently wreck'd and broke up with the Sea, and the Ambergris found floating, or on the Shore; but here again, the Ingenious, until very lately, were at a Loſs, and divided in Opinion, for tho' they agreed it to come from the Whale, yet ſome took it to be the true and proper *Semen*, being found only in the Ball, at the Root of the *Penis*, near the Teſticles; others again thought it was the Ordure or Excrement of the Whale.

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* *Of Ambergris.*

The beſt and moſt exact Account of Ambergris, that I have been able to procure (and I may truly ſay, I have taken a great deal of Pains for it) I very lately received from one Mr. *Atkins*, now an Inhabitant at *Boſton* in *New England*, who uſed the Whale Fiſhery, for ten or twelve Years together, and was one of the firſt that went out a fiſhing for the *Sperma Ceti* Whales, about the Year 1720, and then began to diſcover the Ambergris; and being a ſober ingenious Man, what he ſays may ſafely be depended on; tho', for Subſtance, I have had it from ſeveral of the Whale-men.

His Relation, which was taken a few Days ſince from his own Mouth, is as follows:

" The Ambergris is found only in the *Sperma Ceti* " Whales, and conſiſts of Balls or globular Bodies, of various Sizes, from about three Inches to twelve Inches " Diameter, and will weigh from a Pound and an Half " to twenty two Pounds, lying looſe in a large oval " Bag or Bladder of three or four Foot long, and two " or three Foot deep and wide, almoſt in the Form of " an Ox's Bladder, only the Ends more acute, or like " a Blackſmith's long Bellows, with a Spout running " tapering into and thro' the Length of the *Penis*, and " a Duct, or Canal, opening into the other End of the " Bag, and coming from towards the Kidneys; this Bag " lies juſt over the Teſticles, which are above a Foot " long, and is placed lengthways at the Root of the " *Penis*, about four or five Foot below the Navel, and " three or four above the *Anus*. This Bag or Bladder " is almoſt full of a deep Orange colour'd Liquor, not " quite ſo thick as Oil, and ſmelling ſtrong, or rather " ſtronger of the ſame Scent with the Balls of Ambergris, which float and ſwim looſe in it; the Inſide of " the Bag is very deeply tinged with the ſame Colour " as the Liquor, which may alſo be found in the Canal

" of the *Penis* ; the Balls ſeem to be pretty hard while
" the Whale is alive, inaſmuch as there are many Times
" found, upon opening the Bag, large concave Shelves,
" of the ſame Subſtance and Conſiſtence, that have ſcaled
" off from them, and the Balls themſelves ſeem to be
" compoſed of ſeveral diſtinct Coats incloſing one another,
" ſomething like the Coats of an Onion.

As to the Number of Balls, Mr. *Atkins* never found above four Balls in a Bag, and in the Bag where he found one that weighed twenty one Pounds, which was the largeſt he ever ſaw, there was no other in the Bag but that.

He further ſays, " That to one *Sperma Ceti* Whale
" that has any of theſe Balls, there are two, that have
" nothing but the deep Orange-colour'd Liquor afore-
" ſaid in their Bags." This Remark confirms what another Whale-man told me, " that the Ambergris was
" found only in ſuch *Sperma Ceti* Whales as are old and
" well grown." It is the general Opinion of the Whalemen, that the Ambergris is produced only by the Male or the Bull *Sperma Ceti* Whale. As to this Particular, Mr. *Atkins* ſays, " He never ſaw, nor certainly heard
" of a *Sperma Ceti* Female taken in his Life, the Cows
" of that Species of Whales being much more timorous
" than the Males, and almoſt impoſſible to be come at,
" unleſs when haply found aſleep on the Water, or de-
" tained by their Calves." This is certain, the Boats can never come near them, when they are awake, they are ſo very ſhy and fearful.

Mr. *Atkins*'s Method of getting the Ambergris out of the Whale was thus ; after the Fiſh is killed, he turns the Belly upwards, and fixes a Tackle to the *Penis*, then cuts a Hole round the Root of the *Penis*, through he Rim of the Belly, till he comes to the Entrails, and hen ſearching for the Duct or Canal at the further End of the Bag, he ties it pretty near to the Bag, and cuts the Duct off beyond it, upon which he draws forth the *Penis* by the Tail, and the Ambergris Bag, entirely follows it, and comes clean and whole out of the Belly.

* A very worthy Divine, and one of my intimate Acquaintance, in a neighbouring Town, who took the preceding Relation from Mr. *Atkins*, apprehends the Bag aforeſaid to be the urinary Bladder, and the Ambergris Ball to be a certain Concretion, formed out of the greaſy odoriferous Subſtance of the Liquor aforeſaid contained within it. As for my own Part, I dare not pretend to give any Opinion upon the Point, but content myſelf with relating Matter of Fact.

I doubt not but that, in Proceſs of Time, ſome farther Particulars may be procured with reſpect to Ambergris, and I ſhall be proud to tranſmit them; in the mean Time I hope the Society will accept of this firſt Eſſay, and allow my poor Country the Honour of diſcovering, or at leaſt aſcertaining the Origin, and Nature of Ambergris.

I cannot conclude this Hiſtory of Whales, without acknowledging that I have been much aſſiſted herein by the Reverend Mr. *Greenleaſe* of *Yarmouth*, near *Cape-Cod*, and Mr. *J. Coffin*, ſometime of the Iſland of *Nantucket*, both of them Places famous for the Whale-Fiſhery.

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* *The Reverend Mr.* Prince *of* Boſton.

III. *An Enquiry into a Diſcovery, ſaid to have been made by Signor* Valſalva *of* Bologna, *of an Excretory Duct from the* Glandula Renalis *to the* Epididymis. *By Mr.* John Ranby, *Surgeon, F. R. S.*

THE late learned Signor *Valſalva* having ſome Time ago diſcover'd a Veſſel, which he took to be an Excretory Duct going from the *Glandula Renalis*, to the *Epididymis* in Men, and the *Ovarium* in Women; an Account whereof was publiſh'd in the *Giorn. de Litterati*, 1719. and afterwards inſerted into the *Philoſophical Tranſactions* N° 385. I lately procur'd a human Body, in order to ſearch for it, which I did with all the Diligence I was capable of. I was not ſo happy as to diſcover any Duct of this Kind, but, having injected the *Aorta*, I found the Arteries going to the *Glandulæ Renales* diſpos'd, as hath been repreſented according to the Life, in the annexed Figure. Now whether that Branch of the Artery, which from the *Glandulæ Renales* goes down on both Sides towards the Teſticles, without ſupplying any of the neighbouring Parts, might not have been miſtaken by the abovementioned learned Anatomiſt for an Excretory Duct, all Arteries being generally in dead Bodies free from Blood, and of a whitiſh Colour, I will not preſume at preſent to determine, for Want of farther Experiments.

EXPLANATION.

Fig. VI.

A A The right Kidney.
B B The left Kidney.
C C The deſcending Trunk of the *Aorta.*
D The right emulgent Artery.
E. The left emulgent Artery.
F The right *Glandula Renalis.*
G G The right ſpermatick Artery.
H The left *Glandula Renalis.*
I I The left ſpermatick Artery.
K K The Ureters.

a a A ſmall Artery ariſing out of the deſcending Trunk of the *Aorta*, a little above the right emulgent Artery. It ſends two Branches *b b* upwards to ſupply the right *Glandula Renalis*. A third Branch *c c* goes downwards towards the right ſpermatick Artery G G, and then farther in Company with the ſame to the right Teſticle.

d d A ſmall Artery ariſing out of the left emulgent Artery, near the deſcending Trunk of the *Aorta*, and going directly upwards to the left *Glandula Renalis* H.

e e A ſmall Artery ariſing out of the deſcending Trunk of the *Aorta*, a little below the left emulgent Artery. It divides into two Branches, one whereof *f f* goes upwards between the emulgent Artery and Vein to the left *Glandula Renalis*, the other *g g* downwards, towards the left ſpermatick Artery, and in Company with the ſame to the left Teſticle.

IV. *An Account of a Book entituled,* Prodromus Cryſtallographiæ. De Cryſtallis impropriè ſic dictis Commentarium. A Mauritio Antonio Cappeler, M. D. & Centumviro Lucernenſi. Lucernæ 1723. 4o. *By* J. G. Scheuchzer, *M. D. Coll. Med. Lond. Lic. R. S. S.*

THE Author of this Treatiſe takes notice in the Preface, that it is only a ſmall Part of a larger Work, which he promis'd the Publick ſome Years ago, under the Title of *Cryſtallographia*, and hath now almoſt ready for the Preſs. This greater Work, as he intimates at the Cloſe of this Diſcourſe, is to conſiſt of three Parts, the firſt of which contains the Definition of Cryſtal, with the Synonyms given it by ſeveral Authors both ancient and modern, and an Account of its Properties, Figure, Pellucidity, ſpecifick Gravity, and Bigneſs; as alſo the Place of its Growth, chiefly in regard to *Swiſſerland*, the moſt plentiful Country in *Europe*, as to this Sort of natural Productions; the Signs, whereby hidden Cryſtal Mines may be diſcover'd, and the Way of working them. In the ſecond Part will be examined the Opinions of ſeveral Natural Hiſtorians, about the Origin of Cryſtal, and the Author's own eſtabliſh'd and prov'd. In the third Part will be ſhewn the Uſes of Chryſtal, both Phyſical and Mechanical, and ſome few Hints given, relating to the juſt Value the World has at all Times put upon this beautiful Production of Nature.

The Author divides this preſent Eſſay into two Parts, *viz.* a ſhort Commentary upon the *Cryſtallos*, as he calls them, *impropriè ſic dictos*, in the firſt; and an Account of ſuch as he found mentioned in ſeveral Authors, with a Reduction of them under certain Heads, in the ſecond. *Cryſtalli impropriè ſic dicti*, according to the Author's Definition, are ſuch Bodies, either Stones, Metals, or Salts, as have any Reſemblance with the true Cryſtal; either, as to their multangular, regular, or irregular Figure, or as to their Pellucidity, or any other of its eſſential Properties. As the Number of theſe Bodies is very extenſive, ſo an exact Enquiry into them cannot but be both uſeful and agreeable. I juſt now obſerved, that the *Cryſtalli impropriè ſic dicti* are taken out of Stones, Metals, and Salts. To the firſt belong amongſt other Things ſuch precious Stones, as, in all Probability, have their certain, determined Cryſtal like Figures, as Diamonds, Amethyſts, *&c.* To the ſecond belong all Sorts of *Pyritæ*, as alſo the Growth of Silver, and other Metals, in Form of Trees, or other Things. By the third are underſtood all Chymical Preparations of Salts, and ſaline Bodies, the Figure of which is, generally ſpeaking, more accurate, than in any of the two former. Several Authors of Note have endeavour'd to explain how, Cryſtalliſation is perform'd, or how it comes, that certain Subſtances ſhoot into Cryſtals. Dr. *Cappeler* mentions the Hypotheſis of the learned *Gulielminus*, and that of a late *Swediſh* Author, *Swedenbergh*; and though he ſeems more to favour the latter, than he doth the former; yet he thinks, that they are both liable to Exceptions. But whatever the Cauſe or Method of Cryſtalliſation be, our Author takes it for granted, that three Things are abſolutely requir'd for it, *viz.* *Salt*, which muſt always be an Acid, as is evident by Chymical Experiments, and the very Taſte of ſaline Cryſtals, *Water* and *Earth*. Cryſtalliſation, as far as can be gueſs'd by Chymical Obſervations, is perform'd thus: Particles of certain determined Shapes, ſwimming in a Fluid of a certain Conſiſtence, are, either by the inteſtine Motion of this Fluid, or by the Motion of the Air, ſuppoſed to circulate perpetually through its Pores, or by ſome other Cauſe, brought together, ſo as to form larger Bodies of a Figure proportionable both to the Degree of Impulſion, and the primitive Shape of the conſtituent Parts, or determined by theſe two Things. This Act of Cryſtalliſation, though uniform, as to the Union of Particles conſider'd in itſelf, is yet obſerved to be very different, and to have different Effects, with reſpect to the different Nature of the Fluid, in which Cryſtals are formed, and the Degree of Perfection, to which they are brought. Our Author mentions ſix different Kinds of Cryſtalliſation, each of which, he intends to explain more fully in his larger Work. The firſt Sort of Cryſtalliſation, which hath been examin'd with a great deal of Accuracy by *Gulielminus*, is performed in an aqueous Fluid, wherein ſaline Particles have been diſſolved, boil'd to a certain Degree of Conſiſtence, commonly that of a thin *Pellicula* covering its Surface. This aqueous Fluid muſt be afterwards repos'd in a cool Place, that the ſaline Particles contain'd in it may form themſelves into Cryſtals, which is done in more or leſs Time, according to the different Nature both of the Fluid and Salts. All Chymical Preparations of Salts, the Origin of precious Stones, and of the Cryſtal itſelf, belong to this firſt Kind. Our Author obſerved, that in the *Spiritus aperitivus Glauberi*, (which is a Preparation of *Ciner Clavell.* p. ii. & *Sal Armon.*

Armon. p. i.) after a Year's ſtanding, form'd themſelves artificial Cryſtals, in Figure and Pellucidity exactly like the true ſexangular Cryſtal, and pointed on both Sides. The ſecond Kind of Cryſtalliſation differs from the firſt only in this, that it is performed not in a thin, pellucid, aqueous Fluid, but between thick, mineral, or metallical Mixtures, corroded by acid Salts in the Bowels of the Earth. The third Sort is of a middle Kind between the firſt, and a Coagulation, *viz.* when the Fluid, in which Salts have been diſſolved, is, by degrees, entirely evaporated. This Way of Cryſtalliſation is more proper for diſcovering the primitive Shapes of ſaline Particles. Our Author hath examined ſeveral Salts, both Mineral and Vegetable, and ſeveral Chymical Preparations after this Manner, and hath given us their Figures, as they appear'd to him under a good Microſcope, in two Tables annexed to this Treatiſe. The fourth Sort of Cryſtalliſation is perform'd in a ſtill thinner Fluid than Water is, or in the Air; the Sublimations of the Chymiſts, the Diſtillation of volatile Salts; Snow, whoſe wonderful Figure hath been thought worthy the Amuſement of ſeveral eminent Men; Hail, which is again of very different Shapes; Froſt, and that admirable Variety of Trees, Landſkips, and other inimitable Figures, which, in very froſty Weather, appear upon Glaſs-Windows, or other pellucid Bodies, muſt be all reduced to this fourth Sort. The fifth Sort of Cryſtalliſation is perform'd upon the Surface of a thicker Fluid, as Water, between that and a thinner one, as Air; of this Kind is chiefly Ice. The ſixth and laſt Sort of Cryſtalliſation, mention'd by our Author, differs from all the former, in that it is perform'd neither by the Riſe of Vapours, nor by the repoſing of any Fluid, but on the contrary in a Fluid, which is in a perpetual Motion. That Sort of icy Concretions, which is obſerv'd near ſwift running Waters, and is commonly very porous, not unlike the Tophus, and the ſtony Concretions in ſubterranean Cavities, call'd *Stalactitæ*, belong to this laſt Sort. Thus far what is contained in the firſt Part,

The ſecond gives an Account of ſuch *Cryſtalli improprie ſic dicti*, or Cryſtalline Bodies, which are not properly Cryſtals, as have been mentioned by ſeveral Natural Hiſtorians both ancient and modern. The Author diſtributed them into the following Claſſes, each of which comprehends Stones, Metals and Salts.

Corpora Cryſtalliſata, quæ improprie Cryſtalli vocantur.

Claſſ. I. Globoſa, rotundata & ſphæroidea.
——— II. Conica, Conoidea & fuſiformia.
——— III. Cylindrica, ſolida aut tubulata.
——— IV. Pyramidalia & cuneiformia.
——— V. Priſmatica, Parallelepipeda, Rhomboidea, Trapezoidea.
——— VI. Polyedrica, & Polygona, regularia & minus regularia.
——— VII. Racemoſa, Arbuſculorum in modum, & filamentoſa, filorum, aut capillorum inſtar naſcentia & ſtriatim contexta.
——— VIII. Cruſtis, ſquamis, lamelliſque contexta, ſine, vel cum figura propria.
——— IX. Corpora, quorum cum Cryſtallo affinitas in pelluciditate potiſſimum conſiſtit, figurâ eorum nativâ, vel incertâ vel nondum perſpectâ.

FINIS.

Numb. 388.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *May* and *June*, 1725.

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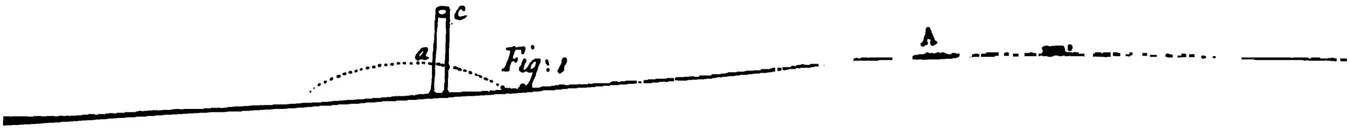
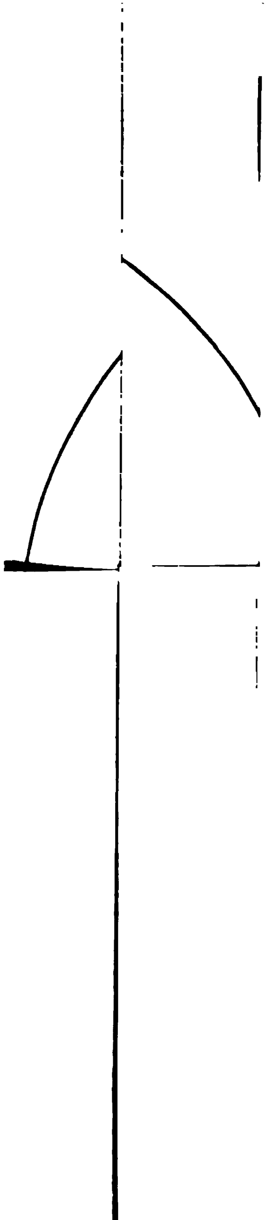


Fig: 1



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* *Vid.* Philoſ. Tranſ. Nº. 386, 387.

I. *A Diſſertation concerning the Figure of the Earth. Part the Second. By the Reverend* J. T. Deſaguliers, *LL. D. F. R. S.*

SINCE my * Paper concerning the Figure of the Earth was read before the Royal Society, I met with a Diſſertation of Monſ. *Mairan* (in the Memoirs of the Royal Academy of *Paris*, for the Year 1720.) wherein the learned and ingenious Author has taken a great deal of Pains to reconcile the Obſervations made on Pendulums, (found to be ſhorter at the Æquator than at *Paris*, when they ſwing Seconds) with the oblong ſpheroidical Figure of the Earth, deduced from Monſ. *Caſſini*'s Meaſures. And tho' upon a ſtrict Examination of his Conjectures, and what he gives for Demonſtrations, I do not find Reaſon to alter my Opinion concerning the oblate or flatted Spheroid, which Sir *Iſaac Newton* has ſhewn to be the Figure of the Earth; yet ſince it might be thought by ſome, who have read Monſ. *Mairan*'s Treatiſe, and afterwards may read mine, that I have not conſider'd all the Circumſtances that He has done, and that I have not been exact enough in the Mathematical Part of my Diſſertation, becauſe I have drawn ſome Concluſions from ſuppoſing the Figure of the Earth ſpherical, when I ſhould have ſuppos'd it an oblong Spheroid; I beg Leave to ſhew here, wherein I think Monſ. *Mairan* is miſtaken, and to give thoſe additional Proofs of my Aſſertions, which I promis'd the Society when I gave in my laſt Paper.

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* *Vid.* Philoſ. Tranſ. N°. 386, 387.

Firſt then I begin with the Conjectures.

Monſ. *Mairan* ſays, that it is as reaſonable to ſuppoſe the Earth (if it was once fluid) to have been an oblong Spheroid at firſt, as a Sphere; and that, in ſuch a Caſe, the Centrifugal Force of the ſeveral Parts of the Earth, ariſing from its Revolution about its Axis, which might convert a Sphere into an oblate Spheroid, wou'd only change an oblong Spheroid into one leſs oblong.

If the Earth was at firſt a Fluid, (ſuppoſed homogeneous, and of any given Form,) and left to thoſe Laws, which we find to obtain at preſent; it muſt put on a ſpherical Figure, for the ſame Reaſon that Drops of Mercury, of Water, and other Fluids, put on ſuch a Figure. And to ſuppoſe any Change made in that Figure from the Preſſure of an external Fluid, filling up all Space, is contrary to what has been demonſtrated by Sir *Iſaac Newton* in his *Principia Lib.* 2. *Prop.* 19. where he ſhews, *That if any Portion of a Fluid be compreſs'd by the ſame or any other homogeneous Fluid, that Portion will not have its Figure alter'd by that Preſſure.*

And indeed we ſee, that in the Receiver of the Air-Pump, Lumps of Butter, coagulated Oil, or Honey, Drops of Quickſilver or Water, &c. have the ſame Figure, whether the Preſſure of the Air acts upon them, or be taken off by exhauſting the Receiver.

That a fluid Subſtance, of any Figure, will by the Gravity of its Parts become ſpherical, is plain by the following

DEMONSTRATION. *Fig.* I,

Let A B C D E be a Portion of an homogeneous Fluid, whoſe Parts tend towards one another, and whoſe Figure is not ſpherical. If in ſuch a Fluid we ſuppoſe a Syphon as ACE (or which is the ſame thing, if all the Fluid ſhou'd be frozen, except the Canal ACE) whoſe Legs AC and CE are unequal, and meet at C, the Center of the Fluid, towards which there is the greateſt Tendency; the Fluid will run out at A in the Leg AC, till it be come down as far as *g* in the Leg CE, ſuppoſing C*g* equal to AC. But if the Leg AC be lengthened as far as *c*, then the Fluid will only come down as far as *e* in the Leg CE, and at the ſame time riſe up to *a* in the Leg C*a*, C*a* being equal to C*e*.

If ſuch another Canal or Syphon be ſuppos'd at BCD, the Fluid in it will come down from D to *d*, and riſe from B to *b*. And ſince ſuch Syphons may be ſuppos'd all over the Fluid ABDE; that Fluid, by the mutual Tendency of its Parts towards one another, muſt be reduc'd to the ſpherical Figure *a b d e*. *Which was to be demonſtrated.*

Now, without conſidering the Unreaſonableneſs of the Suppoſition, let us imagine the Earth to have been an oblong Spheroid at firſt, and then to have a diurnal Revolution given to it, which ſhou'd by Degrees ſhorten its Axis, to bring it to what Meſſieurs *Caſſini* and *Mairan* ſuppoſe it at preſent to be. If in ſuch a Caſe the Earth be ſuppos'd fluid enough to change its Figure, by the Revolution about its Axis, why ſhould it ſtop when the Æquatorial Diameter comes to want juſt one 96th Part of the Length of the Axis? ſince two Powers act upon it to ſhorten its Axis, *viz.* Gravity, and the Centrifugal Force; the firſt of which has already been ſhewn capable to reduce it to a Sphere, and the Centrifugal Force is acknowledged by Monſ. *Mairan* to be (as Sir *Iſaac Newton* has prov'd it) at the Æquator equal to $\frac{1}{289}$ Part of the Gravity there. Certainly the Alteration of Figure wou'd not have ſtopp'd, before the

Earth came to be a Sphere; nay, and it muſt have riſen at the Æquator; and how much, I have already ſhewn in my former Paper.

Again, if we ſuppoſe the Earth of an heterogeneous Fluid, before the diurnal Revolution, the heavieſt Parts wou'd go towards the Center, and the lighter towards the Surface; and that Way the Terraqueous Globe wou'd alſo become a Sphere. Then if, when the Central Parts are fix'd, and the ſuperficial *Strata* are ſtill fluid, the Earth receives a diurnal Motion; it will riſe at the Æquatorial Parts, and that to a greater Height than what I have ſhewn in my former Paper, where I ſuppos'd the Earth of uniform Matter. And that ſomething like this muſt be the Caſe, appears from what Sir *Iſaac Newton* has ſaid upon this Subject. For after having ſhewn, from ſuppoſing the Earth of uniform Matter, that the Centrifugal Force of all its Parts wou'd bring it to be $17\frac{1}{6}$ *Engliſh* Miles higher at the Æquator than at the Poles, and after having given a Table of the proportionable Decreaſe of the Length of the Degrees of a Meridian of the Earth, going from the Poles to the Æquator, in ſuch a Figure of the Earth, with the Lengths that Pendulums muſt have to ſwing Seconds in ſeveral Latitudes; from a Compariſon of the Lengths of Pendulums (obſerv'd by different Perſons to be ſhorter towards the Æquator, than in greater Latitudes (when they ſwing Seconds) he ſhews that the Earth muſt be $31\frac{7}{12}$ Miles higher at the Æquator than at the Poles; and therefore that it muſt be denſer towards the Central than the Superficial Parts to produce a flatted Spheroid, where the Æquatorial Diameter muſt exceed the Axis ſo much more; that is, be longer ſomething more than $\frac{1}{185}$ Part.

Laſtly, let us ſuppoſe the Earth, at its firſt Creation, to have been made of Land and Water, the firſt as ſolid, and the laſt as fluid as it is now, but of Monſ. *Caſſini*'s Figure, and examine the Conſequence. Since in that Figure the Axis is $\frac{1}{96}$ Part longer than the Æquatorial Diameter; the Gravity will be ſo much greater at the Æquator than at the Poles, that the Waters will all flow to the Æquatorial, and leave the Polar Regions; which will happen ſtill more by the Centrifugal Force, which the Earth in its diurnal Motion will give to the Fluid; and therefore the Sea wou'd be $43\frac{85}{100}$ Miles (reckoning 5000 Feet to a Mile) higher at the Æquator than at the Poles, which muſt overflow all the Torrid Zone, and leave the Polar Regions dry.

I am very well aware, that it may be objected by ſuch as have read Monſ. *Mairan*'s Diſſertation, and have not read Sir *Iſaac Newton*'s *Principia*, or have not read that Book with due Attention — " That I have not ar-
" gued fairly in drawing Conſequences from a greater
" Gravity at the Æquator than at the Poles, in an ob-
" long Spheroid; becauſe Monſ. *Mairan* has ſhewn,
" that, in ſuch a Figure of the Earth, the Gravity is greater
" at the Poles than at the Æquator; and that I ſhou'd
" have drawn my Conſequences from theſe Principles."
To which I anſwer, that his Demonſtrations about Gravity are built upon wrong Suppoſitions, as I ſhall ſhew by and by. Nevertheleſs, ſuppoſing that Gravity was greater towards the Poles than towards the Æquator, in the Proportion that he aſſigns, namely of the Ray of Curvature drawn into the Perpendicular to the Curve, terminated at the Axis; let us conſider what will follow from his Principles.

* Let us then ſuppoſe the Earth at firſt in a fluid State; A A the Axis, *d*Æ the Æquatorial Diameter, *o b* a Ray of Curvature, *d n* another, *a c* and *d*C two Lines

of Tendency or Perpendiculars to the Curve, intercepted by the Axis at *c* and C; and *d* C, A C, two Tubes or Canals of the Fluid, gravitating towards, and communicating at C. I ſay that, according to Monſ. *Mairan*'s Principles of Gravity, the Earth cannot preſerve its oblong ſpheroidical Figure. For ſince the Gravity at *a* : Is to the Gravity at *d* :: As *d n* × *d* C : to *a b* × *a c*, it will follow (from the Nature of the Ellipſe) that the Gravity at A : will be to the Gravity at *d* :: As AC^4 : to dC^4 : and therefore the Forces, with which the Columns of Fluid A C and *d* C tend towards C, will be as their Maſſes drawn into the Forces driving towards C, that is, as $AC \times AC^4$ to $dC \times dC^4$. Now by the Principles of Hydroſtaticks, it is evident that the Fluid, in the Canal A C, will cauſe the Fluid in the Canal *d* C to run out at *d* as long as $AC \times AC^4$ is greater than $dC \times dC^4$: And if the Canal C *d* be continued quite to *δ*, the Surface of the Fluid in A C will ſink to *α*, whilſt the Surface of the Fluid in *d* C riſes up to *δ*, in which Caſe as $\alpha C = C\delta$, the Point A will come to *α*, and the Point *d* to *δ*, and the Curve A *d* being chang'd into *α δ*, the oblong Spheroid will be chang'd into a Sphere, the only Figure conſiſtent with the *Æquilibrium* of the fluid Parts, according to Monſ. *Mairan*'s own Principles; becauſe then you will have $AC^4 = dC^4$, and $AC \times AC^4 = dC \times dC^4$. If we make uſe of Sir *Iſaac Newton*'s Principles in this Reaſoning, we ſhall alſo ſhew, that an oblong, ſpheroidical, fluid Earth will be chang'd into a Sphere; but not ſo faſt as it does by Monſ. *Mairan*'s Laws; for, according to Sir *Iſaac Newton*, the Gravity at A : Is to the Gravity at *d* :: As $\sqrt[3]{\overline{Cd}}$: $\sqrt[3]{\overline{AC}}$. *Q. E. D.*

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* Fig. II.

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* Fig. II.

NB. *Here we have ſuppos'd no diurnal Revolution, for as ſoon as that begins, the Centrifugal Force will raiſe the Æquatorial Parts, and change the Sphere into a flatted Spheroid, as has been before ſhewn, and is allow'd by Monſ.* Mairan.

Now if we ſuppoſe the ſame Figure of the Earth, but the Land (at its firſt Creation) as firm as it is now; it will in that Caſe follow from Monſ. *Mairan*'s Principles, that the Sea muſt riſe and overflow all the Æquatorial Regions, tho' the Earth had no diurnal Revolution; and much more ſo, when the Centrifugal Force, ariſing from the diurnal Motion, helps to carry the Water the ſame Way.

DEMONSTRATION.

Let P *æ* P Æ* repreſent the Plane of a Meridian, P P the Axis of the Earth (ſuppos'd an oblong Spheroid) *æ* Æ the Diameter of the Æquator, *d e a æ* Part of the Surface of the Earth, *æ* A and *e* B two Perpendiculars to the Surface of the Earth (which are here two Rays of Curvature) *f c* the Surface of the Sea, and *f d e g*, *b a æ c* two Cylinders of Sea-Water of equal Baſes and equal Heights.

Since Gravity acts on the two equal Columns of Water *b a c æ*, *f d e g* in the reciprocal Ratio of the Ray of Curvature (at the reſpective Places of the Columns) drawn into that Part of it which Monſ. *Mairan* calls the Line of Tendency, (that is, in the Ratio of *e* B × *e* Z to *æ* A × *æ* C) the Weight of *f e* : will be to the Weight of *b æ* : : As *æ* A × *æ* C : to *e* B × *e* Z. Therefore if there be a Communication between the fluid Columns fe and $b\ae$, there cannot be an *Æquilibrium*, till the Quantity of Matter in fe, becomes to the Quantity of Matter in $b\ae$, reciprocally as the Gravity at the Place $\ae$ is to the Gravity at e; and in that Caſe the Height ge will be reduc'd to ke, if $ke : c\ae :: eB \times eZ : \ae A \times \ae C$. And conſequently the Surface of the Sea will go thro' the Points $ikbc$, where bc under the Æquator is higher than ik towards the Poles. *Q. E. D.*

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* Fig. III.

NB. *That the Centrifugal Force will ſtill add to the Height of the Sea at* b c, *is plain from what we have ſaid before. And if we apply theſe Principles to determine the differeut Lengths of Pendulums, ſwinging Seconds at* Paris *and at the Æquator; from the Gravity at* Paris, *compar'd to the Gravity at the Æquator (in this Suppoſition of the Action of Gravity and Figure of the Earth) a Pendulum muſt be ſhorter at the Æquator by more than* 10 *Lines, without conſidering the Centrifugal Force; and if the Centrifugal Force be taken into Conſideration, the Pendulums muſt be ſhortened near a whole Inch. But this being about five Times more than agrees with Obſervation; what proves too much, proves nothing at all.*

Having thus ſhewn, that Monſ. *Mairan*'s Account of the Action of Gravity, on ſeveral Places upon the Earth's Surface, can be of no Service for reconciling the Experiments made on Pendulums, with the Figure of the Earth deduc'd from Monſ. *Caſſini*'s Meaſures: I proceed to ſhew that his Demonſtrations are founded upon wrong Principles. And firſt, in Relation to Gravity.

This Gentleman has follow'd Sir *Iſaac Newton*, in ſaying, that Gravity increaſes in a duplicate reciprocal

Proportion of the diminiſh'd Diſtance from the Center of the Force, and ſo *vice versâ*; but he has follow'd Sir *Iſaac Newton* no farther than ſerv'd his preſent Purpoſe; otherwiſe he wou'd have known.—That in reſpect to a Central Body (as a Planet) towards which others are (*attracted* or) impell'd by Gravity, this Law obtains only,as Bodies attracted, are remov'd from the Surface of the Planet, to greater Diſtances from the Center compar'd with that Diſtance; or as from greater Diſtances they approach nearer to the Planet. —That the greateſt Action of Gravity is at the Surface of the Planet.—That afterwards in advancing towards the Center, the Force of Gravity,on the Body attracted, continually grows-leſs, decreaſing directly as the Diſtance; and that this holds true in a Spheroid as well as a Sphere. —That on different Parts of the Surface of the Earth (in the Condition it is now) the Gravity on Bodies is reciprocally as their Diſtance from the Center of the Earth.—That though at a conſiderable Diſtance we look upon the Earth, or any Planet, or even the Sun, as a Point (in the Center of the Forces tending towards it) endued with an abſolute Force, proportional to its Quantity of Matter; yet when we come ſo near the Body as to conſider the Space it takes up, we are to take notice, that the whole Attraction or Gravity of the Body, is made up of the Sum of the Attraction of all its Parts properly combin'd; and therefore, that when a Corpuſcle, or Body attracted, comes to be within the Planet, or Body attracting, the Matter above it draws it back in ſuch a Manner, that it leaves it only a Force to go on towards the Center, which is directly as the Diſtance, as we have already ſaid; juſt as if a Body concentric to the Planet (whether ſpherical or ſpheroidical) had its Surface juſt where the Corpuſcle is, and all the exterior Cruſt or Shell was annihilated.

I do not doubt but Monſ. *Mairan* will be of this Opinion, when he has carefully and impartially examin'd the 12th and 13th *Sections* of the Firſt Book of Sir *Iſaac Newton*'s *Principia*, and the 18th, 19th, and 20th *Prop.* of the Third Book. And if he will be at the Pains to compare the 38th and 39th *Propoſition* of the Third Book with the 66th of the Firſt, he will find that the Preceſſion of the Æquinoxes is owing to the broad ſpheroidical Figure of the Earth; and that if it had Monſ. *Caſſini*'s Figure, the Æquinoctial Points wou'd move *in Conſequentia* faſter than they do now *in Antecedentia*.

Further, Monſ. *Mairan* demonſtrates, that in an oblong Spheroid, the Diminution of Gravity, by the Centrifugal Force, encreaſes faſter in going from the Poles to the Æquator, than it wou'd do in a Sphere, and faſter in a Sphere than it wou'd do in a broad Spheroid; and therefore wou'd ſhew, "That notwithſtanding the Sur-
" face of the Earth is nearer to the Center in Monſ. *Caſ-*
" *ſini*'s Figure than in Sir *Iſaac Newton*'s, yet the
" Centrifugal Force will diminiſh the Gravity ſo faſt in
" going from *Paris* to the Æquator, that the ſhortening
" of Pendulums, to make them ſwing Seconds at the
" Æquator, may very well be accounted for that Way."

Now let us examine into this Matter, to ſee whether the Cauſe is adequate to the Effect.

If the Diſtance from the Surface of the Earth at the Pole to the Center be 96, and the Diſtance of the Surface at the Æquator be 95, the Diſtance of the Surface at *Paris*, in the Latitude of 48° 50', will be 95,562, *&c.* by the Property of the Ellipſe. Now ſince the Force of Gravity, in different Places on the Earth's Surface, is reciprocally as the Diſtance from the Center, and the Lengths of Pendulums, that perform their Vibrations in the ſame Time, are directly as the Force of Gravity;

therefore the Length of Pendulums at *Paris*, will be to their Length at the Æquator, as 95 to 95,562, *&c.* that is, as 440,555, *&c.* to 443,165, *&c.* and conſequently they muſt be lengthen'd 2,61 *&c.* Lines. But as from Monſ. *Mairan*'s Principles, the Diminution of Gravity by the Centrifugal Force, is greater at the Æquator than at *Paris*, hardly $\frac{1}{440}$ Part of the whole Gravity at the Æquator, the Pendulums muſt be ſhortened in that Proportion; ſo that then the Length of a Second-Pendulum, will be 440,555 + 2,61 — 1 Lines. But as that Quantity is greater than 440,555, *&c.* therefore the Pendulums upon the Whole muſt be lengthen'd: Nay, though we ſhou'd allow a ſhortening of two Lines; ſince by Obſervation Pendulums are found to be about two Lines ſhorter at the Æquator, the oblong ſpheroidical Figure of the Earth cannot be conſiſtent with the Experiments on Pendulums.

I beg Leave to ſet down Monſ. *Mairan*'s aforeſaid Demonſtration here; that we may ſee whether he has aſſum'd true Principles.

PROPOSITION V.

" * XI. *The Centrifugal Force at any Degree of*
" *Latitude, taken upon the oblong Spheroid, between*
" *the Æquator and the Pole, is leſs in Compariſon*
" *to the Centrifugal Force at the Æquator, than it*
" *wou'd be at the ſame Degree of Latitude taken upon*
" *a Sphere; or, which is the ſame thing, the Centri-*
" *fugal Force encreaſes more, going from the Poles*
" *towards the Æquator, upon an oblong Spheroid,*
" *than upon a perfect Sphere; and conſequently Gra-*
" *vity diminiſhes more, and a Pendulum muſt be more*

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* *See Monſ.* Mairan's *Diſſertation, Article* XI. &c.

“ *ſhortened under the Æquator, in the Hypotheſis of*
“ *the oblong Spheroid, than in that of a perfect*
“ *Sphere.*

‘ Having deſcrib’d an oval Curve of any Kind, as
“ for Example, the Ellipſe * ADBE abovementioned,
“ and inſcrib’d the Circle DHE, whoſe Radius is DC
“ = half the ſhorter Axis DE; upon AD take any
“ Point as R, between the Æquator and the Pole, and
“ from that Point to the *Evoluta* OTX draw the *Ray*
“ *of Curvature* RT, which gives the *Line of Tendency*
“ RP (*Art.* IV.) Draw likewiſe from the common Center
“ C, to the Circumference of the Circle DH, a Radius
“ CV, parallel to PR, and meeting the Circle at V;
“ then from the Points R, V, draw the Lines RN, VZ,
“ perpendicular to the Axis AB.

“ It muſt be obſerv’d, *Firſt*, That as the Ellipſe AD
“ repreſents a Meridian of the oblong Spheroid, the
“ Circle DH repreſents a Meridian of a Sphere in the
“ ſame Plane.

“ *Secondly*, That the Point V, on the Circular Meridian,
“ anſwers to the ſame Degree of Latitude as
“ the Point R, upon the elliptical Meridian; becauſe
“ the Lines PR, CV, being parallel to each other, and
“ perpendicular, the one to the Ellipſe and the other
“ to the Circle (*by Conſtruction*) the touching Planes,
“ or Horizons of the Points R, V, will alſo be parallel.

“ *Thirdly*, Whence it follows that the Diminution of
“ the Centrifugal Force (acting againſt Gravity) on account
“ of its Obliquity to the Horizon (*Art.* X.) of

Fig. IV.

“ the ſame Degree of Latitude on the Elliptical and on
“ the Circular Meridian, is the ſame in both Caſes, and
“ in the ſame Ratio as the abſolute Centrifugal Forces
“ repreſented by the Perpendiculars R N, V Z, (*Art.* IX.)
“ Therefore to know whether the Centrifugal Force
“ (whether abſolute or relative) of the Point R, upon the
“ oblong Spheroid A D B E, be leſs or greater in reſpect
“ to the Centrifugal Force under the common Æquator
“ D E, than the Centrifugal Force (whether abſolute or
“ relative) of the correſpondent Point V upon the Sphere;
“ nothing more is requir'd than to ſee which is the
“ longeſt of the two Perpendiculars, namely, R N in
“ the oblong Spheroid, or V Z in the Sphere; ſince
“ theſe two Lines expreſs the Radii of the Circles of
“ Revolution, and conſequently the abſolute Quantity
“ of the Centrifugal Forces.

“ *4thly and laſtly,* That the Ratio of the Centrifugal
“ Forces of two correſpondent Points upon the oblong
“ Spheroid A D B E, and the inſcrib'd Sphere DHE, to the
“ Centrifugal Force of their Æquators is the ſame, ſuppo-
“ ſing the Sphere of any other Bigneſs; and that it has
“ been determin'd here of the Diameter D E, only to
“ render the Demonſtration eaſier, by giving the ſame
“ Conſequent to the Antecedents R N and V Z. For
“ if about the Center C and with the Radius C *d*, the
“ Circle *d h e* be deſcrib'd equal (for Example) to a
“ Meridian of a Sphere of the ſame Solidity as the ob-
“ long Spheroid A D B E; and the Radius C V be pro-
“ duc'd till it meet the Circle *d b* at the Point *u*, and
“ *u z* be let fall perpendicular to the common Axis of
“ Revolution, and parallel to V Z: It is plain, that
“ we ſhall always have V Z : D C :: *u z* : *d* C, or
“ $\frac{VZ}{DC} = \frac{uz}{dC}$, and conſequently $\frac{RN}{DC}$ will have the

“ ſame Ratio to $\frac{VZ}{DC}$ as to $\frac{uz}{dC}$.

“ Therefore, in order to demonſtrate that the Centrifugal
“ Force of a Point, taken in any Latitude upon
“ the oblong Spheroid, is leſs when compar'd to the Centrifugal
“ Force of the like Point, taken upon a Sphere
“ in reſpect to the Centrifugal Force at the Æquator;
“ there is nothing more requir'd than to ſhew that R N
“ $< VZ$, becauſe by that means we ſhall have $\frac{RN}{DC}$
“ $< \frac{VZ}{DC}$.

“ This being obſerv'd; from the Point R, draw the
“ Line R I, parallel to the Axis A B, and meeting the
“ Circle D H at K, and the Diameter D E of the Æquator
“ at the Point I. From the Point K having let fall
“ the Perpendicular $KL = RN$, upon the Axis A B,
“ and drawn K C to the Center C; the Queſtion will be
“ brought to this, *viz.* To know whether the Point V
“ coincides with the Point K; or whether it is above it
“ towards D, or below towards H.

“ But $CK = CV = CD > PR$ (*Art.* VIII.) therefore
“ CK and P R being both between the Parallels
“ A C, R I, the greateſt C K is more inclin'd to them
“ than the leaſt P R, and the Angle K C A is leſs than
‘ the Angle $RPA = VCA$. And ſince theſe two
“ Angles have each of them one of their Sides coinciding
“ with the Line A C, namely, the Side A P of the Angle
“ R P A, and the Side A C of the Angle K C A, it follows
“ that the Side V C of the Angle $VCA = RPA$
“ $> KCA$, will go above C K between C K and C D,
“ and meet the Line R I at the Point G, between K and
“ I, and the Circle D H at the Point V, which conſe-

“ quently will be above RI, between K and D. Therefore $CV = CG + GV$ is $= PR + GV$, and conſequently VZ, which meets RI at the Point F, is $= ZF + FV = RN + FV$; and therefore $RN = VZ - FV$. Therefore $RN < VZ$.

“ And becauſe the ſame Thing may be demonſtrated in reſpect of any other Point, taken between the Æquator and the Pole; and that Gravity, and conſequently the Length of a Pendulum diminiſhes, as the Centrifugal Force encreaſes. Therefore *&c.* *Q. E. D.*

COROLLARY.

“ XII. From what has been demonſtrated, and from *Prop.* 3. *Art.* VIII. it follows, that the Perpendicular which is drawn from any Point of an oval Meridian to the Axis, will be ſo much ſhorter, in Compariſon to the Perpendicular drawn from the correſpondent Point of an inſcrib'd circular Meridian, as the Latitude is greater; and conſequently (by *Art.* XI. *Num.* 3.) the Centrifugal Force will be ſo much the leſs, and Gravity ſo much the greater, upon the oblong Spheroid, in reſpect to the Centrifugal Force, and the Gravity under the Æquator.

“ For as the Line RP does always decreaſe, as the Point R is taken nearer to the Pole A, it is evident, that the Angle VCK will continually encreaſe, in reſpect to the Angles VCA, KCA, as it is their Difference, and conſequently that the Perpendicular VZ will be ſo much greater than the Perpendicular $KL = RN$.

I paſs over the Demonſtration of the latter Part of his Propoſition above-mentioned, which he deduces juſtly from his Conſtruction, if what he ſays (*Num.* 2.) be right; becauſe in ſuch a Caſe it cannot be call'd in Queſtion, and proceed to an Obſervation that he makes afterwards, *viz.* " *We muſt take care to obſerve in the " foregoing Propoſitions and Corollaries, that the " Compariſon is always made between two ſimilar " Points of Latitude, taken upon the two Spheroids, " or upon one of the Spheroids and the Sphere, " between the Æquator and the Poles, in reſpect to " the Centrifugal Force upon the Æquator of any " one of theſe Spheroids, or of the Sphere. For if " we only compar'd abſolutely the Centrifugal Force " of a Point of the Æquator of the one, to the Centrifugal Force of a correſpondent Point of the Æquator of the other, it is plain that it wou'd be " greater upon a flatted Spheroid than upon a Sphere, " or than upon an oblong Spheroid of the ſame Solidity, in the Ratio of the great Axis of the generating Ellipſe of the flatted Spheroid, to the Diameter of the Sphere, or to the ſhorter Axis of the " generating Ellipſe of the oblong Spheroid. And in " all Likelihood, this muſt be the Reaſon that has " made others, who have treated of this Subject, to " imagine the very contrary of what I have demonſtrated.*

As Monſ. *Mairan* conſiders the Earth at reſt, in the Conſtruction for his Demonſtration above quoted, and afterwards obſerves what Effect the Centrifugal Force will have upon Bodies on its Surface, to diminiſh the Gravity, with which they endeavour to deſcend in their Line of Tendency R P: He ſhou'd not only have taken notice (as he has done) that the whole Centrifugal Force N R is not to be ſubſtracted from the Gravity at R, as the whole Centrifugal Force CD is to be ſubſtracted from the whole Gravity at D, becauſe of the Obliquity of R N to P R; but he ſhould have obſerv'd alſo, that the Obliquity of the Plane of the Parallel N R, in which the Centrifugal Force acts, muſt alter the Line of Tendency R P, and change the Direction R P into R W, ſomewhere between the Point P and the Center C; for if there be a heavy Body as a Plummet, hanging by a Thread in the Line S R, or S P, the Line of Tendency which has been ſuppos'd perpendicular to the Curve A R D, without taking in the Effect of the Centrifugal Force; as ſoon as the Spheroid revolves about its Axis, the Body which wou'd fall in the Line S R, acted upon only by one Force, namely, that of Gravity, will now be acted upon by another Force, at the ſame Time puſhing it in the Line S *s* (which is the ſame as R *r*) and conſequently will move in the Line S *r*, diagonal of the Parallelogram *s* S R *r*; or, which is all one, a Body plac'd at R will have its Line of Tendency in R W, as I have already ſhewn in my firſt Diſſertation on this Subject; only I did not ſuppoſe the Earth a Spheroid before the diurnal Motion, and therefore made uſe of the Line Z V inſtead of the Line N R; ſo that it may be objected that the Angle *r* S R will not be ſo great in a Spheroid as in a Sphere, becauſe the Centrifugal Force which acts with the ſame Obliquity (ſince N R P = Z V C) is as much leſs in the Spheroid as N R is leſs than Z V: But I was aware of that, and therefore made the Angle R S *r* only of 5 Minutes, when it really appears to be of almoſt ſix Minutes, when the Earth is ſuppos'd ſpherical; and therefore, without coming to give the exact Quantity of the ſaid Angle, one may eaſily perceive, that Monſ. *Caſſini*'s Difference of the Axis and Æquatorial Diameter will produce a Figure, in which the Angle R S *r*, will not be leſs than of 5 Minutes.

Such an Obliquity, caus'd in the Direction of Gravity, will render the oblong ſpheroidical Figure of the Earth impoſſible, becauſe then Fluids wou'd not have the Lines of their Gravity perpendicular to the Horizons of the

Places where they are, (ſuppoſing the Horizons of Places to be Planes touching the Curve of the Earth in thoſe Places) and Plumb Lines wou'd be ſo far out of the Perpendicular to Lines of Level, as to make an Angle eaſy to be obſerved, as I have ſhewn in my former Paper.

But if the ſame Cauſe be ſuppos'd to act upon the Sea to make it level, as makes heavy Bodies to fall (which certainly muſt) then indeed Lines of Level will be perpendicular to Plumb Lines, and the Level of the Sea, taken always for the Horizon of a Place, will not be a Plane touching the Earth, but cutting it towards the Poles, and conſequently the Water will be carried towards the Æquator, as was before ſhewn.

Beſides, the Difference of the Action of the Centrifugal Force wou'd not be ſo great between correſpondent Points of the ſame Latitude in the Spheroid and in the Sphere; for when the Line of Tendency R P is by the Centrifugal Force chang'd into R W, the Point R upon the Spheroid does no longer correſpond in Latitude with the Point V upon the Sphere, but muſt be taken nearer to V, ſo that the Line R W may become parallel to V C, and R W A = V C A.

If it be alledged here, that Monſ. *Mairan* ſuppoſes the Earth in Motion, and takes in the Effect of the Centrifugal Force, when he makes the Line of Tendency to be R P; I anſwer, that if he had conſider'd the Earth as revolving upon its Axis, he wou'd not have made V C the Line of Tendency of a ſpherical Earth in Motion, ſince it is the Line of Tendency of ſuch an Earth at reſt.

In Monſ. *Mairan*'s Obſervation above-mentioned, he ſays, " that we are not to compare the Centrifugal Force at the Æquator of an oblong Spheroid, with the Centrifugal Force at the Æquator of a Sphere, or at the

“ Æquator of a flatted Spheroid of the ſame Solidity ;
“ allowing that then it wou’d be greater in the Sphere,
“ and ſtill greater in the flatted Spheroid: but only the
“ Centrifugal Forces in ſeveral Latitudes upon the ſame
“ Figure.” —But I beg Leave to differ from him for the following Reaſons.

Firſt, Becauſe the Force of Gravity is not the ſame at the Æquator of the flatted Spheroid, as it is at the Æquator of the Sphere, or as it is at the Æquator of the oblong Spheroid.

Secondly, Becauſe it is not the ſame in different Latitudes, in either of the Spheroids. (See Sir *Iſaac Newton* Lib. 3. Prop. 19 and 20.) And Monſ. *Mairan*’s Way of arguing will only ſerve, in Caſe the Gravity ſhou’d be the ſame in all the Points of the Surface of the Earth in his Figure, and alſo in the two other Figures. For Example, let the uniform Gravity be call’d g; and *Firſt*, let the Centrifugal Force at the Æquator of the flatted Spheroid be call’d $c+2$; and the Centrifugal Force in any Latitude, as for Example, the Latitude of *Paris* (as it is diminiſhed on Account of a ſhorter Co-ſine of Latitude, and likewiſe on Account of its Obliquity to the Line of Tendency,) be call’d $c+2-l$; the Difference of the Diminution of Gravity at *Paris*, and at the Æquator will be $\overline{g-\overline{c+2}}-\overline{g-\overline{c+2-l}}$ $=l$.

Secondly, Let the Centrifugal Force at the Æquator of the Sphere be call’d $c+1$, and the Centrifugal Force at the Latitude of *Paris* be call’d $c+1-\overline{l+m}$; the Difference of the Diminution of Gravity at

Paris and at the Æquator in a ſpherical Earth, will be $\overline{g-\overline{c+1}}-\overline{g-\overline{c+1-\overline{l+m}}}=l+m$.

Thirdly, Let the Centrifugal Force at the Æquator of the oblong Spheroid be call'd c, and the Centrifugal Force at *Paris* be call'd $c-\overline{l+m+n}$; the Difference of the Diminution of Gravity at *Paris*, and at the Æquator, in an oblong ſpheroidical Earth, will be $g-c-\overline{g-\overline{c-\overline{l+m+n}}}=l+m+n$.

Now, if Gravity ſhou'd in every Caſe be equal to g, it is evident, that the ſhortening of Pendulums, at the Æquator, wou'd be greater in the oblong Spheroid, than in the Sphere, or in the flatted Spheroid; becauſe as the Lengths of Pendulums diminiſh with the Gravity, thoſe Lengths will be at *Paris* and at the Æquator, when compar'd, as $g-\overline{c+2-l}$ to $g-\overline{c+2}$ in the flatted Spheroid; as $g-\overline{c+1-\overline{l+m}}$ to $g-\overline{c+1}$ in the Sphere, and as $g-\overline{c-\overline{l+m+n}}$ to $g-c$ in the oblong Spheroid; and conſequently, from what M. *Mairan* has demonſtrated this Ratio of $g-\overline{c-\overline{l+m+n}}$ to $g-c$, being greater than either of the others, the Pendulums muſt be ſhortened in the oblong Spheroid.

But as the Force of Gravity is leſs at the Æquator of the flatted Spheroid, than at the Æquator of the Sphere, or of the oblong Spheroid of the ſame Solidity: let us expreſs its Quantity in the three Caſes by $g-s$, g, and $g+s$, and we ſhall then find the Lengths of the Pendulums, at the Æquator of the three Solids, as $g-s-\overline{c+2}$, $g-\overline{c+1}$, and $g+s-c$; conſequently the Lengths of

Pendulums will be greateſt at the Æquator of the oblong Spheroid, becauſe $g + s - c$ is the greateſt Quantity.

Laſtly, To compare the Lengths of Pendulums at the Æquator of the oblong Spheroid, thus found, with their Lengths at the Latitude of *Paris* upon the ſaid Spheroid — Let us expreſs the Exceſs of Gravity at the Æquator, whereby it is greater than at *Paris* (becauſe in this Figure, *Paris* is farther from the Center of the Earth, than the Æquator, by $\frac{1}{230}$ Part) by the Letter s, and the Exceſs of the Centrifugal Force at the Æquator, above that Part of it which acts directly againſt Gravity at *Paris*, by $l + m + n$, the Gravity at *Paris* by g, and the Centrifugal Force at the Æquator by c; then $g + s - c$ will ſtill repreſent the diminiſh'd Gravity, and anſwer to the Length of Pendulums at the Æquator, whilſt $g - \overline{c - \overline{l + m + n}}$ or $g - c + l + m + n$ repreſents the diminiſh'd Gravity, and conſequently the Length of Pendulums at *Paris*. If s be equal to $l + m + n$, Pendulums will be as long at the Æquator as at *Paris*; and if s be greater than $l + m + n$, Pendulums will be longer at the Æquator. But making all poſſible Allowance, in Favour of Monſ. *Mairan*'s Hypotheſis, no Calculation will bring $l + m + n$ to be greater than, or ever equal to s. Therefore Monſ. *Mairan*'s Demonſtrations, above-mentioned, are of no Force to prove the Earth to be an oblong Spheroid.

And now, I think, I have anſwer'd all that relates to the Figure of the Earth in Monſ. Mairan's *Diſſertation; in ſhewing, That his Conjectures can neither be ſupported by thoſe Phyſical Principles which Sir* Iſaac Newton *has Mathematically deduc'd from un-*

queſtioned Obſervations and Experiments accurately made; nor even by thoſe Principles which He (M. Mairan) *has aſſum'd to ſerve his intended Purpoſe — That his Demonſtrations, relating to the Difference of the Action of the Centrifugal Force, are of no Service to him, for reconciling the Experiments made on Pendulums, with Monſ.* Caſſini's *Meaſures; — becauſe, when applied to Sir* Iſaac Newton's *Principles, they will make Pendulums longer at the Æquator than at* Paris, *and when applied to Monſ.* Mairan's *own Principles, they will make them a whole Inch ſhorter at the Æquator than at* Paris, *contrary to all Obſervations, which, at a Medium, make Pendulums but about two Lines or* $\frac{166}{1000}$ *of an Inch longer at the Æquator than at* Paris. — *That he has built his Demonſtrations upon a wrong Notion of Gravity — And that he has not conſider'd what is moſt material in the Effect of the Centrifugal Force, acting on Bodies deſcending by their Gravity, between the Æquator and the Poles, namely, the Alteration of their Line of Direction, which wou'd make them fall out of the Perpendicular towards the Æquator.*

I ſhall add one more Philoſophical Argument, given me by a Friend, to whom I communicated my Thoughts on this Subject; becauſe it is wholly independent on thoſe Principles of Philoſophy, concerning which, ſome of the Gentlemen that believe the oblong ſpheroidical Figure of the Earth, and the *Engliſh* Philoſophers, are not yet agreed; and it is this.

If the Earth was of an oblong ſpheroidical Figure, higher at the Poles than the Æquator; the Axis of its Revolution, wou'd either go thro' one of its ſhort Diameters, or be continually changing unleſs the ſaid Axis did exactly coincide with the Axis of the Figure.

DEMONSTRATION.

Suppoſe ſuch an oblong Figure as A *a* * fix'd to the Axis P*p* at the Center C, but capable of moving freely round it towards P or towards *p*, yet ſo as to be oblig'd to move with the Axis, when it is turned round. Suppoſe now the Poles P and *p* to be fix'd, and the Body, thus conſtituted, to be turn'd ſwiftly round the Axis P*p*; then if the Angle A C P be oblique, and the Figure A D *a* E be oblong, the Parts A C and C *a* will acquire a Centrifugal Force, which will enlarge the Angle *p* C A, till it comes to be a right one. Beſides this, a Velocity will be generated in the Motion, while A is going towards the Perpendicular *a* C, which will make it go farther on towards P, as to B, with a Motion which will after that be retarded, till the Centrifugal Force has Strength enough to ſend it back again the contrary Way; and ſo it will move continually with a reciprocal Motion, like the Oſcillation of a Pendulum; and if a little of this Motion be loſt at every Oſcillation, then the oblong Figure AD*a*E will at laſt move quietly about its leſſer Axis DE coinciding with P*p*.

If A a did not at firſt exactly coincide with P p, the Centrifugal Force will have the above-mentioned Effect; and that this is not the Caſe in the Earth is more than probable, becauſe the unequal Diſtribution of Sea and Land, beſides the Phænomena *of the Tides muſt make the Axis of its Gravity, and conſequently the Axis of its Revolution, to differ from the Axis of the oblong Spheroid, if the Earth had ſuch a Figure; without conſidering that every Earth-quake wou'd alter ſo nice an* Æquilibrium, *which once loſt, wou'd never be recover'd again.*

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* Fig. V.

To leave nothing unexamin'd,relating to the Controverſy, I have again conſider'd the Meaſures and Obſervations,mentioned in the Account of the Meridian drawn thro' *France*, in the Memoirs of the *Royal Academy*, for the Year 1720; and I find them to want a great deal of the Accuracy requir'd in ſo nice a Point, as determining the different Lengths of Degrees upon the Surface of the Earth. To prove my Aſſertion, I beg that the Reader will examine the following Tables, whereby it appears, that if any thing certain can be deduc'd from the ſaid Obſervations and Meaſures, (either taken as they are, or reduc'd to the Level of the Sea, by the Rules given by Monſ. *Caſſini*[*]) it will be in Favour of Sir *Iſaac Newton*'s Figure of the Earth, rather than theirs.

In the following Table, the firſt Column gives the Names of Places; the ſecond,the Diſtances from *Paris*, according to the Meaſures taken by the *French* Gentlemen; the third, the Latitudes obſerv'd by the ſame; the fourth, the Latitudes, ſuch as the meaſur'd Diſtances will give them, ſuppoſing the Earth ſpherical; the fifth, the Differences between theſe and the Latitudes obſerv'd, expreſs'd in Seconds of a Degree, where when the Latitude computed, exceeds the Latitude obſerv'd, the Letter N (North) ſhews that Difference to be in Favour of Monſ. *Caſſini*'s Figure, and the contrary Difference mark'd by the Letter S (South) is in Favour of Sir *Iſaac Newton*'s Figure.

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[*] *Memoirs for the Year* 1720. Vol. I. P. 1. Ch. 13.

Names of Places.	Diſtances from *Paris* meaſur'd.	Latitudes obſerv'd.			Latitudes in a ſpherical Earth computed from the meaſur'd Diſtances.			Differences in Seconds.
I.	II. Toiſes.	III.			IV.			V.
Dunkirk.	125552	51°	2′	25″½	51°	2′	25″½	0″
Amiens.	60370	49	53	48	49	53	48	0″
Sourdon.	49970½	49	42	42	49	42	52,1	10,1 N
Paris.		48	50	10	48	50	20,3	10,3 N
Malvoiſine.	18838	48	30	47	48	30	32,1	14,9 S
Vouſon.	67962	47	39	17	47	38	53,6	23,4 S
Bourges.	100192	47	4	31	47	04	58,7	27,7 N
S. Sauvier.	139934	46	23	24	46	23	12	12,0 S
Croc.	169540	45	51	43	45	52	4,6	21,6 N
Bort.	196484	45	23	27	45	23	45,2	18,2 N
Aurillac.	223606	44	55	13	44	55	14,5	1,5 N
Rodès.	256575	44	20	54	44	20	35,1	19,9 S
Alby.	280612	43	55	32	43	55	19	13,0 S
Carcaſſone.	321430	43	12	55	43	12	24,5	31,5 S
Collioure.	360604	42	31	13$\frac{2}{8}$	42	31	13,8	0.

In this Table it is to be obſerv'd that there is an equal Number of Differences mark'd N (North) and S (South) and if the Differences on each Side be added together, there will be 89″,4 on the North Side, and 114″,7 on the South: This laſt agrees beſt with Sir *Iſaac Newton*'s Figure, which muſt be ſuppoſed for the Correction of ſo great a Difference.

In the next Table, the firſt Column gives the Names of Places; the ſecond, the Latitudes obſerv'd; the third, the Diſtances in the Meridian from *Paris*, reduc'd to the Level of the Sea; the fourth, the Differences of the ſecond Column expreſs'd in Seconds of a Degree; the fifth, the Differences of the Numbers in the third Column; and the ſixth, the Meaſure of a Degree by the fourth and fifth Columns compar'd.

I. Names of Places.	II.			III. Toiſes.	IV. Seconds of a Degree.	V. Toiſes.	VI. Toiſes.
Dunkirk.	51°	2'	19''	125454			
Amiens.	49	53	56	60444	4103''	65010	57040
Clermont.	49	22	57	31028	1859	29416	56965
The R. Obſervatory.	48	50	10	0	1967	31028	56787
Vouſon.	47	39	17	67959	4253	67959	57525
☞ S. Sauvier.	46	23	24	139937	4553	71978	56912
Croc.	45	51	43	169539	1901	29602	56058
Bort.	45	23	46	196480	1677	26941	57834
Aurillac.	44	55	13	223616	1713	27136	57028
Rodés.	44	20	53	256474	2060	32858	57422
Alby.	43	55	32	280612	1521	24138	57131
Carcaſſone.	43	12	55	321430	2557	40818	57468
Collioure.	42	31	13	360614	2502	39184	56380

In this Table in the third Column, over-againſt *St. Sauvier*, the Number which was 139944 is corrected to make it 139937, to the Advantage of the oblong Figure. In the ſixth Column, the Numbers appear ſo irregular, as to be unfit to decide this Controverſy: Then if a Compariſon be made between *Dunkirk*, *St. Sauvier* (which is very near the Middle of *France*, and almoſt in the Meridian of *Paris*) and *Collioure*, the Meaſurement is abſolutely in Favour of Sir *Iſaac Newton*'s Theory; the mean Degree between *Dunkirk* and *St. Sauvier* being larger by about 64 Toiſes, than between *S. Sauvier* and *Collioure*; and to reduce them even to an Equality, there muſt be a greater Alteration made in the Situation of thoſe three Places, than it is reaſonable to ſuppoſe their Obſervations to be capable of admitting. Here follows the Compariſon.

Dunkirk and *Collioure*	A mean Degree is	57061
Dunkirk and *Paris*		56960
Paris and *Collioure*		57097
Dunkirk and *S. Sauvier*		57090,4
S. Sauvier and *Collioure*		57026,5
According to Monſ. *Picard*,		57060

To conclude, I will propoſe a Method of obſerving the Figure of the Shadow of the Earth in Lunar Eclipſes, whereby the Difference between the Diameters in the oblong ſpheroidical Figure, if there be ſuch an one as Monſ. *Caſſini* affirms (*viz.* of 96 to 95) may be diſcover'd.

Let P Æ P Æ * repreſent the Earth, ſeen from the Sun at the Time of the Summer Solſtice; it is evident, that the ſame Figure will expreſs the Section of the Earth's Shadow at the Moon's Diſtance, as ſeen from the Earth. If E E repreſents the Ecliptick, Æ Æ will be the ſhorteſt Diameter of the Section; and if L L be taken for the Moon's Way, in a total and central Eclipſe of the Moon, by obſerving the Time which is ſpent in the Paſſage of the Center of the Moon, thro' the Shadow, and reducing that Time to Seconds of a Degree of a great Circle of the Heavens, we ſhall have the leaſt Diameter of the Shadow.

Again, let the ſame Letters † repreſent the ſame Things, only here the Section of the Shadow is ſuch, as the Earth will caſt at the Æquinox, and the Eclipſe of the Moon is here ſuppoſed partial, its Center juſt touching the Shadow. When the Moon's Center is got to *c*, if the Latitude of its Center or its Diſtance from the Ecliptick be obſerv'd, we ſhall have the Length *c* C nearly equal to the longeſt Semi-diameter of the Shadow.

Now, comparing *c* C * in this Figure to L C * in the former (the Difference between *c* C and C P (*Fig.* 7.) and between C L and C Æ (*Fig.* 6.) not being worth notice) they ought to be to one another, as 96 to 95, which in ſuch a Shadow will give a Difference of about 25″ at a Medium, ſenſible enough to be obſerved, notwithſtanding the *Penumbra*. If therefore thoſe Aſtronomers who have Inſtruments nice enough, and ſufficient Skill in the Management of them, to take Angles to 3 or 4 Seconds of a Degree, will obſerve what I have been mentioning in total and partial Eclipſes of the Moon; by ſuch Obſervations they will eaſily convince us, that the Figure of the Earth is ſuch as Monſ. *Caſſini* ſuppoſes it, or convince him that he has been miſtaken.

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* Fig. VI. † Fig. VII. * Fig. VII. * Fig. VI.

The Semi-diameter of the Earth's Shadow, when the Earth is in Perihelio, *and the Moon in* Apogæo *is* $38'$, *or* $2280''$, *without conſidering the Encreaſe of the Shadow, on account of the Atmoſphere of the Earth, which wou'd make it* $39'$ or $2340''$ (*allowing one Second for a Mile*;) *and the Semi-diameter of the Shadow, when the Earth is in* Aphelio, *and the Moon in* Perigæo *is* $46', 20''$, *or* $2780''$, *which encreaſed on account of the Atmoſphere of the Earth, will bring it to* $47', 20''$ *or* $2840''$. *Now if the Proportion of 95 to 96 be taken in both Caſes you will have theſe Analogies,*

$$\left\{\begin{array}{l} 95 : 96 :: 2340'' : 2364''.6 \\ 95 : 96 :: 2840'' : 2869'',8 \end{array}\right\}$$

So that $2364'',6 - 2340'' = 24''6$ *will be the Difference of the Semi-diameters, when the Section of the Shadow is the leaſt, and* $2869'',8 - 2840'' = 29'',8$ *will be the Differenee of Semi-diameters, when the Section of the Shadow is the greateſt; the Sum of thoſe Differences* $24'',6 + 29'',8$ *halved, will give the Difference, when the Section of the Shadow is at a Medium* $= 27''4$; *from which if we take* $2'',4$ *becauſe in* Fig. 7. C c *is a little leſs than* CP, *and in* Fig. 6. L C *is ſomething greater than* ÆC, *we ſhall have* C *c in* Fig. 7. *to compare with* L C *in* Fig. 6. *which will exceed it by* $25''$, *if Monſ.* Caſſini's *Figure of the Earth be the true one.*

II. *A Catalogue of the* 50 *Plants, from* Chelſea *Garden, preſented to the* Royal Society, *for the Year* 1724. *by the Company of Apothecaries, purſuant to the Direction of Sir* Hans Sloane *Bart. Pr. Coll. Med. S. P. V. Pr. By Mr.* Iſaac Rand, *Apothecary, F. R. S.*

101. GEranium lucidum, ſaxatile. C.B. 318.

102. Geranium folio Malvæ rotundo. C. B. Ib.

Majus, foliis in breviores & latas lacinias diviſis.

103. — Columbinum, vulgare, petalis florum bifidis, purpureis.

— Secundum *Camer. Epit.* 600.

104. — — Petalis florum bifidis albis.

An Ger. Malacoides, flore albo. *H. Reg. Blæſ.*

105. — — Humile, flore cæruleo, minimo. *Raii Syn. Ed.* 3. p. 359.

— — Tenuius laciniatum. C. B. 318. *Prod.* 138.

106. — — Majus, flore minore, cæruleo. *Raii H.* 1058.

107. — — Maximum, diſſectis foliis. *Plot. Hiſt. Ox.*

108. — Orientale, columbinum, flore maximo, Aſphodeli radice. *T. Cor.* 20.

109 — Sanguineum, maximo flore. C. B. 318.

110. — — Cauliculis erectis, flore minore. *H. L. Bat.* 286.

111. — Hæmatodes, foliis majoribus, pallidioribus & altius inciſis, D. *Dale. Raii Syn* 219.

112. Geranium hæmatodes Lancaſtrenſe, flore eleganter ſtriato. *Raii Hiſt.* 1061.

113. — Batrachoides. Gratia Dei, Germanorum. C. B. 318.

114. — — Montanum noſtras. *Raii Hiſt.* 1062.

115. — — Odoratum. C. B. 318.

116. — Annuum minus Batrachoides, Bohemicum, purpureo-violaceum. *H. Ox.* p. 2. 511.

117. — Phæum, flore reflexo I. B T. 3. 477.

118. — Magnum, folio trifido I. B. T. 3. 478.

119. — Romanum verſicolor, ſive ſtriatum. *Park. Parad.* 229.

120. — Robertianum primum. C B. 319.

121. — — Rubens, flore albo.

122. — Lucidum, ſaxatile, foliis Geranii Robertiani. *D. Sherard. Raii Syn.* 218.

123. — Folio Althææ. C. B. 318.

124. — — — Floribus ſaturatiùs rubentibus. *H. L. Bat.* 290.

125. — Chium Vernum, Caryophyllatæ folio. *T. Cor.* 20.

126. — Folio Alceæ, tenuiter laciniato; flore pentapetalo; purpuraſcente, ſemine tenui. *Boerh. Ind. alt.* 266.

127. — Cicutæ folio, moſchatum. C. B. 319.

128. — — —, Minus & ſupinum C. B. 319.

129. — Arvenſe, album. *Tabern. Ic.* 57.

130. — Myrrhinum, tenuifolium, amplo flore purpureo. *Barrel.* ob. 55.

131. — Latifolium, longiſſimâ acu. C. B. 319.

132. — Cicutæ folio, acu longiſſimâ. C. B. ib.

133. — Acu longiſſimâ, Cicutæ folio tenuiùs laciniato.

— Cicutæ folio, viſcoſum, acu longiſſima. D. *Juſſieu.*

134. —— Triſte *Cornuti*. 109.

135. —— Æthiopicum, noctu olens, radice tuberoſa, foliis Myrrhidis, anguſtioribus. *Breyn. Cent.*

136. —— Africanum, Coriandri folio, floribus incarnatis, minus. *H. L. Bat.* 279.

137. —— Africanum fruteſcens, Chelidonii folio, petalis florum anguſtis, albidis; carnoſo caudice.

138. —— —— Noctu olens, tuberoſum & nodoſum, Aquilegii foliis. *H. L. Bat.* 284.

139. —— ——Betonicæ folio, procumbens, floribus parvis, eleganter variegatis. *Pluk. Alm.* 169.

140. —— —— Foliis inferioribus Aſari, ſuperioribus Staphydis agriæ, maculatis, ſplendentibus, & Acetoſæ ſapore. *Com. Præl.* 52.

141. —— Africanum fruteſcens, folio craſſo & glauco, Acetoſæ ſapore. *Com. Præl.* 54.

142. —— —— Alchimillæ hirſuto folio, floribus albidis. *H. L. Bat.* 282.

143. —— —— Folio Malvæ craſſo, molli, odoratiſſimo, floſculo pentapetalo, albo. *Boerh. Ind. alt.* 263.

144. —— —— Fruteſcens, Malvæ folio laciniato, odorato. *H. L. Bat.* 277.

145. —— —— —— —— —— —— Inſtar Meliſſæ, flore purpuraſcente. *Boerh. Ind.* 110.

146. —— —— Arboreſcens, Malvæ folio mucronato, petalis florum inferioribus vix conſpicuis.

147. —— —— —— Alchimillæ hirſuto folio, floribus rubicundis. Com. præl. 51.

148. —— —— —— Folio Malvæ plano, lucido, glabro; flore Kermeſino, Di van Leur. *Boerh. Ind.* 109.

149. —— —— —— Ibiſci folio rotundo; Carlinæ odore. *H. L. Bat.* 274.

150. —— —— ——, —— —— Anguloſo; floribus amplis purpureis.

—— —— Fruteſcens, Alchimillæ folio, hirſuto, cum fimbriis purpureis. *Boerh. Ind.* 110.

III. *Obſervations concerning the Height of the Barometer, at different Elevations above the Surface of the Earth, in a Letter to the Publiſher from the Learned Dr.* Nettleton.

SIR,

BEING curious to learn by Obſervation, how far the Mercury will deſcend in the Tube at any given Elevation, for which there is ſufficient Opportunity hereabouts, I propoſed to take the Altitude of ſome of our higheſt Hills; but, when we attempted it, we found our Obſervations ſo diſturbed by Refractions, that we cou'd come to no Certainty. Having meaſur'd one Hill of a conſiderable Height, in a clear Day, and obſerved the Mercury at the Bottom and at the Top, we found, according to that Eſtimation, that about 90 Feet, or upwards, were required to make the Mercury fall one Tenth of an Inch; but coming afterwards to repeat the Experiment on a cloudy Day, when the Air was ſomewhat groſs and hazy, we found the ſmall Angles ſo much augmented by Refraction, as to make the Hill much higher than before, tho' they were taken carefully with very good Inſtruments, both at that Time and before. I afterwards frequently obſerv'd at home, by pointing the Quadrant to the Tops of ſome of our neighbouring Mountains, that they wou'd appear higher in the Morning before Sun-riſe, and alſo late in the Evening, than at Noon, in a clear Day, by ſeveral Minutes: Particularly, one Morning in *December* laſt, when the Vapours lay condens'd in the Vallies, and the Air above was very pure, the Top of a Mountain, at ſome Diſtance from hence, appear'd more elevated, by above 30 Minutes, than it had done in the Beginning of *September* about Noon, on a very clear Day. From whence it appears, that the Refraction is at ſome times greater than at others; but probably 'tis always very conſiderable, and, as there is no certain Rule to make Allowance for it, it ſeems likely, that all Obſervations made on very high Hills, eſpecially when view'd at a Diſtance, and under ſmall Angles, as they commonly are, are uncertain, and ſcarce to be depended on, generally erring in making the Heights greater than they really are.

I then proceeded to obſerve, as near as I was able, the Variation of the Mercury in ſome ſmaller perpendicular Elevations, which we cou'd meaſure with a Line, and alſo on the Tops of ſome Hills of a moderate Height, whoſe Altitude we cou'd obſerve moſt commodiouſly, and, by taking the Angles large, avoid the Danger of any conſiderable Refraction.

At the Bottom of the Tower of *Halifax* Church, the Mercury ſtood at 29 *Inch.* 78 *Dec.* At the Top it ſubſided to 29. 66. The Height of the Place, where the Obſervation was made, was found to be 102 Feet.

At the Bottom of a Coal-Mine, near this Place, the Mercury ſtood at 29. 48. At the Top, it fell to 29. 32. The Depth of the Mine, being meaſured, was found to be 140 Feet.

At the Bottom of another Mine, the Mercury was obſerved to ſtand at 29. 50. At the Top, it fell to 29. 23. The Depth of this Mine was 236 Feet.

At the Foot of a ſmall Hill, whoſe Height we cou'd meaſure very exactly, the Mercury ſtood at 29. 81. At the Top it fell to 29. 45. The Height of the Hill was 312 Feet.

At the Bottom of *Halifax* Hill, commonly call'd the Bank, the Mercury was obſerved to ſtand at 30. 00.

At the Top, it fell to 29. 41. The Height of this Hill was found to be 507 Feet.

Our Mathematicians do demonſtrate, that the Denſity of the Air decreaſes in a Geometrical Progreſſion, as the Elevation encreaſes in an Arithmetical one, and conſequently, that the Logarithms of the Denſities are as the Elevations reciprocally. But the Weight of the Air being as its Denſity, and the Height of the Mercury in the Barometer being always proportional to the Air's Weight, it follows, that the Logarithms of the Heights of the Mercury are, reciprocally, as the Elevations: Whence having found by Obſervation, what Elevation is requir'd to make the Mercury ſtand at any Height, it will be eaſy to determine, how much is requiſite to reduce it to any other Height propos'd. If we make 30 Inches the Standard Height of the Mercury, equal to Unity, and ſuppoſe an Elevation of 85 Feet be requir'd to make it fall one Tenth of an Inch from that Height, as by theſe Obſervations it is very nearly; then as the Logarithm of $\frac{30{,}0}{29{,}9}$ is to 85, ſo is the Log. $\frac{30{,}0}{29{,}5}$ to the Number of Feet requir'd to make it fall Half an Inch, and ſo of the reſt. When the Mercury ſtands above 30 Inches, the Numbers will be negative, and ſhew the Spaces deſcending; by which Method I computed the following Tables.

The latter, which contains the Differences of the Numbers in the former, was of very great Uſe to me, when, in theſe Experiments, the Mercury ſtood at any other Height in the Tube, beſides 30 Inches, and fell any Number of Tenths, or Parts of a Tenth, by adding the Numbers anſwering thereto, or proportionable Parts of them, to find the Elevation requir'd in the Table, to make the Mercury fall ſo much, and thereby readily to compare the Heights found by Obſervation therewith. And though ſome ſmall Errors, in the Obſervations, do make them vary a little from each other, yet in the main, they agree as near as poſſible with the Numbers of the Table; as did alſo ſeveral other Experiments too long to mention, which makes me believe thoſe Numbers are not far from the Truth; but of that you will be beſt able to judge, by comparing theſe Experiments with others of the ſame Kind.

That the Air is colder, as well as more light and rare, in Places that are ſituated high, than it is in the Vallies and low Grounds, is generally known; and in order to learn, how much it might be ſo, I got a Friend of mine, who lives higher than we do here, to obſerve the portable Barometer and Thermometer, at his Houſe, for ſome Days, being plac'd as near as poſſible in the ſame Circumſtances with mine; and we found his Barometer ſtood at a Medium for 20 Days, 3 Tenths lower than mine, and the Thermometer 3. *deg*. 2. lower; allowing for the Difference of the Inſtruments, which had been obſerved before.

At another Place the Barometer, at a Medium for 14 Days, ſtood lower by 4. 46. and the Thermometer was lower by 4. *deg*. 4. At another Place, which was very high upon the Moors, the Barometer, at a Medium for ten Days, ſtood lower by 0. 65. and the Thermometer fell 7°.

A Table ſhewing the Number of Feet aſcending, requir'd to make the Mercury fall to any given Height in the Tube, from 30 to 26 Inches. As alſo the Number of Feet deſcending, requir'd to make the Mercury riſe, from 30 to 31 Inches.

In.	Dec.	Feet	Dec.	In.	Dec.	Feet	Dec.
31	0	834	79	27	9	1847	55
30	9	752	53	27	8	1938	97
30	8	670	01	27	7	2030	72
30	7	587	21	27	6	2122	80
30	6	504	15	27	5	2215	21
30	5	420	82	27	4	2307	95
30	4	337	21	27	3	2401	02
30	3	253	32	27	2	2494	44
30	2	169	10	27	1	2588	20
30	1	84	72	27	0	2682	33
30	0	00	00	26	9	2776	80
29	9	85	00	26	8	2871	62
29	8	170	29	26	7	2966	79
29	7	255	87	26	6	3062	32
29	6	341	73	26	5	3158	21
29	5	427	89	26	4	3254	46
29	4	514	34	26	3	3351	07
29	3	601	08	26	2	3448	05
29	2	688	11	26	1	3545	41
29	1	775	44	26	0	3643	14
29	0	863	08				
28	9	951	01				
28	8	1039	25				
28	7	1127	80				
28	6	1216	66				
28	5	1305	83				
28	4	1395	32				
28	3	1485	13				
28	2	1575	26				
28	1	1665	70				
28	0	1756	47				

A Table ſhewing the Number of Feet requir'd to make the Mercury fall one Tenth of an Inch from any given Height in the Tube, from 31 to 26 Inches.

In.	Dec.	Feet	Dec.	In.	Dec.	Feet	Dec.
31	0	82	26	27	9	91	42
30	9	82	53	27	8	91	75
30	8	82	79	27	7	92	08
30	7	83	06	27	6	92	41
30	6	83	33	27	5	92	74
30	5	83	61	27	4	93	07
30	4	83	89	27	3	93	41
30	3	84	16	27	2	93	76
30	2	84	44	27	1	94	12
30	1	84	72	27	0	94	47
30	0	85	00	26	9	94	82
29	9	85	29	26	8	95	17
29	8	85	58	26	7	95	53
29	7	85	86	26	6	95	89
29	6	86	16	26	5	96	25
29	5	86	45	26	4	96	61
29	4	86	74	26	3	96	98
29	3	87	03	26	2	97	36
29	2	87	33	26	1	97	73
29	1	87	63	26	0	98	10
29	0	87	93				
28	9	88	24				
28	8	88	55				
28	7	88	86				
28	6	89	17				
28	5	89	49				
28	4	89	81				
28	3	90	13				
28	2	90	45				
28	1	90	76				
28	0	91	09				

IV. Experimentum, in Argenti-Fodina Salana, circa aſcenſum Mercurii in Barometro, inſtitutum. Per *And. Celſium.* Ex Actis Literariis Sueciæ Ann. 1724. Trimeſtr. IV.

AD variationem columnæ mercurialis obſervandam, pro diverſa altitudine regionum atmoſphæræ, fodinæ noſtræ profundiores in primis idoneæ cenſeri debent. Harum enim profunditatem non modo omni exactitudine metiri; ſed & brevi temporis ſpatio obſervationem totam abſolvere licet. Quo ſane commodo haud raro deſtituuntur, quibus circa altiores montes hæc talia experiri animus eſt. Si itaque quamplurima in variis fodinis inſtituerentur experimenta; nulli dubitamus, quin vera progreſſio, quâ denſitates aëris decreſcant, tandem ſua ſponte ſe proderet.

In *magna fodina Cuprimontana* argenti vivi aſcenſum a viro ampliſſimo *Georgio Vallerio* dudum obſervatum eſſe, ex litteris ipſius ad Cl. *De La Hire* d. 15 Julii, A. 1711. Fahlunæ datis, certiores reddimur. *

In *argenti* vero *fodina Salana*, ſeptem fere milliaribus ab Upſalia verſus occidentem diſſita, hujus rei periculum a me factum eſt die 28 Aug. currentis anni. Scilicet juxta limen *putei Reginæ Chriſtinæ*, (*Drottning Chriſtina Schatcht*,) hydrargyri altitudinem 30 digit. & 38 enteſ. ſeu $\frac{1018}{1200}$ pedis Suecani obſervavi. Cum Barometro deinde in tonna, quæ funi adpenſa machina hydraulica trahitur, ad profunditatem 636 pedum me demiſi; ubi mercurius ad 30 dig. 98 centeſ. aſcendiſſe de-

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* *Vid.* Memoires de l'Acad. R. des Sc. l'ann. 1712. p. 108.

prehenſus

Fig. 1.

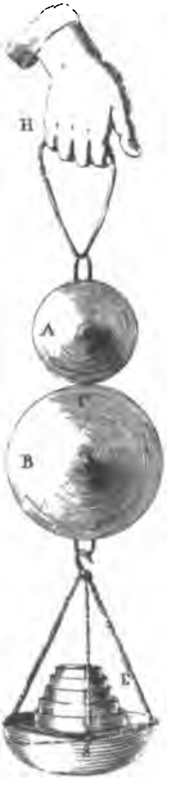
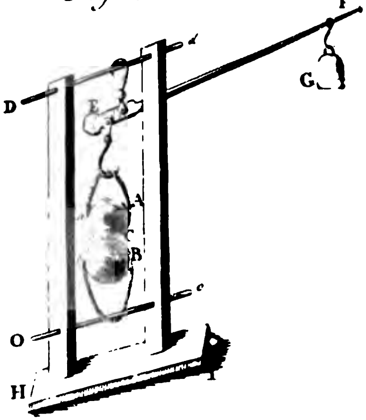


Fig. 2.



J. Parker sculp.

Numb. 389.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *July* and *Auguſt*, 1725.

The CONTENTS.

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II. De Camphora. Auctore *Dno. Carolo Neuman*, Chym. Prof. Reg. *Berolin.* S. R. S.

III. *Obſervations of the* Dipping *Needle, made at* London, *in the Beginning of the Year* 1723. *By Mr.* George Graham, Watchmaker, F. R. *S.*

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VII. *Extract of ſeveral Letters and Certificates ſent to his Majeſty the King of* Great Britain, *concerning a very particular* Nævus Maternus, *or* Mole, *Communicated by Dr.* Steigertahl, *Phyſician to his Majeſty,* F. R. S.

VIII. *An Account of a Book, entitled,* Hiſtoria Cœleſtis Britannica, tribus Voluminibus contenta, Authore *Joanne Flamſteedio* Aſtronomo Regio.

ERRATA.

No. 387. Page 263. line 20. for *June*, read *February*. p. 268. l. 3. for *Shelves*, r. *Shells*. p. 269. l. 3. for *Tail*, r. *Tackle*. No. 389. p. 318. l. 7, r. *Degrees*. p. 324. l. 30. r. *in ſatis magna*.

I. *Remarks upon the Obſervations made upon a Chronological Index of* Sir Iſaac Newton, *tranſlated into* French *by the Obſervator, and publiſh'd at* Paris.

ON *Thurſday November* 11, 1725. A ſmall Tract in print was deliver'd to me, as a Preſent, from Mr. *William Cavelier,* Jun. a Bookſeller at *Paris,* a Perſon unknown to me, entituled, *Abregé de Chronologie de M. le Chevalier Newton, fait per lui même, & traduit ſur le Manuſcript Anglois.* And the Bookſeller hath premiſed an Advertiſement, in which he endeavoureth to defend himſelf for printing it without my Leave, ſaying, that he had writ three Letters to me for my Leave, and in the Third had told me, that he would take my Silence for a Conſent; and that he had alſo charged one of his Friends in *London* to ſpeak to me, and procure my expreſs Anſwer; and that having long expected my Anſwer, he thought that he might take my Silence for a Sort of Conſent, and ſo procured a Privilege, and printed it, and then received my Anſwer from his Friend, which was as follows.

I Remember that I wrote a Chronological Index for a particular Friend, on condition that it ſhould not be communicated. As I have not ſeen the Manuſcript which you have under my Name, I know not whether it be the ſame. That which I wrote was not at all done with Deſign to publiſh it. I intend not to meddle *with that which hath been given you under my Name, nor to give any Conſent to the publiſhing of it,*

I am,

Your very humble Servant,

ISAAC NEWTON.

London, May 27,
1725, *St. Vet.*

The Privilege was granted *May* 21, and regiſter'd *May* 25 Old Style, my Letter was dated *May* 27, and the Chronological Index, or Abridgement as he calls it, was printed before the Arrival of my Letter, and kept ever ſince to be publiſh'd at a convenient Time. The Bookſeller knew that I had not ſeen the Tranſlation of the Abridgement, and without ſeeing it could not in reaſon give my Conſent to the Impreſſion. He knew that the Tranſlator was unknown to me, and was againſt me; and therefore he knew that it was not fit that I ſhould give my Conſent, nor be ask'd to do it. He knew that the Tranſlator had written a Confutation of the Paper tranſlated, and that this Confutation under the Title of *Obſervations*, was to be printed at the End of it, and he told me nothing of all this, nor ſo much as the Name of the Obſervator, and yet asked my Conſent to the publiſhing; as if any Man could be ſo fooliſh as to conſent to the publiſhing of an unſeen Tranſlation of his Papers, made by an unknown Perſon, with a Confutation annexed, and unanſwered at their firſt Appearance in Publick.

After the Recital of my Letter, he adds, that the Author of the Tranſlation, and of the Obſervations upon it, pretends to have an entire Certainty that this Index, or Abridgement of Chronology, is the ſame with the Writing own'd by me in my Letter, and is perſuaded that the Manuſcript, which hath been communicated to him, hath been copied from that of this Friend, that is, from that of the particular Friend above-mention'd in my Letter. And therefore the Manuſcript, which hath been communicated to him, is that of *Abbè Conti*, a noble *Venetian* now at *Paris*. He, being about ſeven Years ago in *England*, gave me notice, that the Friend above-mention'd deſir'd to ſpeak with me. And this Friend then deſir'd a Copy of what I had written about Chronology. I replied that it was imperfect and confus'd, but in a few Days I could draw up an Abſtract thereof, if it might be kept ſecret. And ſome Time after I had done this, and preſented it, this Friend deſir'd that Signor *Conti* might have a Copy of it. He was the only Perſon who had a Copy, and he knew that it was a Secret, and that it was at the Deſire of this Friend, and by my Leave, that he had a Copy, and he kept it ſecret, while he ſtaid in *England*; and yet, without either this Friend's Leave or mine, he diſpers'd Copies of it in *France*, and got an Antiquary to tranſlate it into *French*, and to confute it; and the Antiquary hath got a Printer to print the Tranſlation and the Confutation; and the Printer hath endeavour'd to get my Leave to print the Tranſlation, without ſending me a Copy thereof to be perus'd, or telling me the Name of the Tranſlator, or letting me know that his Deſign was to print it with a Confutation unanſwer'd and unknown to me.

The Tranſlator near the End of his Obſervations (Page 90) ſaith, I believe that I have ſaid enough concerning the *Epocha* of the *Argonauts*, and the Length of *Generations* to make People cautious about the reſt. For theſe are the two Foundations of all this new

Syſtem of Chronology. What he ſaith, concerning the *Epocha* of the *Argonauts*, is founded on the Suppoſition that I place the Equinox in the Time of the *Argonautic* Expedition, fifteen Degrees from the firſt Star of *Aries*, Pag. 75, 79. I place it in the Middle of the Conſtellation, and the Middle is not fifteen Dergees from the firſt Star of *Aries*. The Obſervator grants that the Conſtellations were formed by *Chiron* (Pap. 70, 71, 79.) and that the Solſtices and Equinoxes were then in the Middle of the Conſtellations (Pag. 65, 69, 75.) and that *Eudoxus*, in his *Enoptron* or *Speculum* cited by *Hipparchus*, followed this Opinion, Pag. 62, 63, 65, 69, 79. And * *Hipparchus* names the Stars, through which the Colures paſſed in this old Sphere, according to *Eudoxus* and ſaith expreſly that *Eudoxus* drew one of theſe Colures through the Middle of *Cancer* and the Middle of *Capricorn*, and the other through the Middle of *Chelæ* and the Back of *Aries*. And the Colurus, paſſing through the Back of *Aries*, paſſes through the Middle of *Aries*, and is but eight Degrees from the firſt Star of *Aries*. I follow *Eudoxus*, and, by doing ſo, place the Equinoctial Colure about 7*gr.* 36′ from the firſt Star of *Aries*. But the Obſervator repreſents, that I place it fifteen Degrees from the firſt Star of *Aries*, and thence deduces that I ſhould have made the *Argonautic* Expedition 532 Years earlier than I do. Let him rectify his Miſtake, and the *Argonautic* Expedition will be where I place it.

As for the Length of *Generations*, the Obſervator ſaith, that I reckon them one with another at 18 or 20 Years a piece (Pag. 52, 55.) which is another

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* *See* Hipparchus *publiſh'd by* Petavius, Vol. 3. Pag. 116, 117, 119, 120.

Miſtake. I agree with the Antients in reckoning three Generations, at about an hundred Years. But the Reigns of Kings I do not equal to Generations, as the antient *Greeks* and *Egyptians* did; but I reckon them only at about 18 or 20 Years a piece one with another, when ten or twelve Kings, or more, are taken in continual Succeſſion. So the firſt 24 Kings of *France* (*Pharamond*, &c.) reigned 458 Years, which is one with another 19 Years a piece. The next 24 Kings of *France* (*Ludovicus Balbus*, &c.) reigned 451 Years, which is one with another 18¾ Years a piece. The next 15 Kings (*Philippus Valeſius*, &c.) reigned 315 Years, which is one with another 21 Years a piece. And all the 63 Kings of *France* reigned 1224 Years, which is 19½ Years a piece. And, if the long Reign of *Lewis* XIV. be added, the 64 Kings of *France* will reign but 20 Years a piece. And they, that examine the Matter, will find it ſo in other Kingdoms: And I ſhorten the Duration of the antient Kingdoms of *Greece*, in the ſame Proportion that I ſhorten the Reigns of their Kings, and thereby place the *Argonautic* Expedition about 44 Years, and the Taking of *Troy* about 76 Years after the Death of *Solomon*, and find *Seſoſtris* contemporary to *Seſac*.

So then the Obſervator hath miſtaken my Meaning, in the two main Arguments on which the Whole is founded, and hath undertaken to tranſlate and to confute a Paper which he did not underſtand, and been zealous to print it without my Conſent; tho' he thought it good for nothing, but to get himſelf a little Credit, by tranſlating it to be confuted, and confuting his own Tranſlation.

The Obſervator ſaith, that I ſuppoſe that the *Egyptians* began, about 900 Years before Chriſt, to form their Religion, and deify Men for their inventing of

Arts, notwithſtanding that it appears by the Scriptures, that their Idolatry and Arts were as old as the Days of *Moſes* and *Jacob*, Pag. 82, 83. But he is again miſtaken. I deny not that the Kingdom of the lower *Egypt* called *Mizraim*, had a Religion of their own, till they were invaded and ſubdued by the Shepherds, who were of another Religion: but I ſay, that, when the *Thebans* expelled the Shepherds, they ſet up the Worſhip of their own Kings and Princes. I ſay alſo, that Arts were brought into *Europe* principally by the *Phœnicians* and *Curetes*, in the Time of *Cadmus* and *David*, about 1041 Years before Chriſt; and do not deny that they were in *Phœnicia*, *Egypt*, and *Idumea*, before they came into *Europe*.

The Obſervator ſaith alſo, that, 884 Years before Chriſt, I place the Beginning of the canicular Cycle of the *Egyptians* upon the vernal Equinox, although that Cycle never begins in Spring, Pag. 84, 85. But he is again miſtaken. I meddle not with that Cycle, but ſpeak of the *Egyptian* Year of 365 Days.

The Obſervator repreſents, that I have a great Work to come out: but I never told him ſo. When I lived at *Cambridge*, I us'd ſometimes to refreſh myſelf with Hiſtory and Chronology for a While, when I was weary with other Studies: but I never told him, that I was preparing a Work of this Kind for the Preſs.

Abbè Conti came into *England* in Spring 1715, and, while he ſtaid in *England*, he pretended to be my Friend, but aſſiſted Mr. *Leibnitz* in engaging me in new Diſputes, and hath ſince acted in the ſame Manner in *France*. The Part he acted here may be underſtood by the Character given of him in the *Acta Eruditorum* for the Year 1721, Pag. 90. where the Editor, excuſing himſelf from repeating ſome Diſputes which had been publiſh'd in thoſe *Acta*, ſubjoins: *Suffecerit itaque annotaſſe Abbatem quendam Italum de Conti nobilem Venetum (de quo admiratione digna ſibi præſcripta eſſe ab Hermanno fatetur Leibnitius) cum ex Gallia in Angliam trajeciſſet, mediatoris vices in ſe ſuſcipere voluiſſe, atque literas Newtoni ad Leibnitium deferri curaſſe, Leibnitianas cum Newtono communicaſſe.* And how Mr. *Leibnitz* by this Mediation, endeavour'd to engage me, againſt my Will, in new Diſputes, about occult Qualities, univerſal Gravity, the Senſorium of God, Space, Time, Vacuum, Atoms, the Perfection of the World, Supramundane Intelligence, and Mathematical Problems, is mention'd in the Preface to the ſecond Edition of the *Commercium Epiſtolicum.* And what he hath been doing in *Italy*, may be underſtood by the Diſputes raiſed there by one of his Friends, who denies many of my Optical Experiments, though they have been all tried in *France* with Succeſs. But I hope that theſe Things, and the perpetual Motion, will be the laſt Efforts of this Kind.

II. De Camphora. Auctore *Dno. Carolo Neuman*, Chym. Prof. Reg. *Berolin.* S. R. S.

CUM jam plures, quorum aliqui ipſas quoque Indias peragrarunt, de Camphora tam copioſe ſcripſerint, ut nihil eorum, quæ ad integram hiſtoriam Camphoræ dilucidandam pertinere videri poterant, intermiſerint, ſiquidem & originem vocis explicarunt, & ſynonyma cuncta collegerunt, arborem, unde petitur Camphora, & loca, in quibus arbores hujuſmodi proveniunt, indagarunt, modum quo

Camphora ibi colligitur, & in Europa depuratur, denique & uſus, in quos adhiberi poteſt Camphora, docuerunt; ſupervacaneum plane foret, iſta omnia hic repetere, chartaſque meris citatis complere. Cuivis etiam ſuam de Camphora opinionem & ſententiam liberam eſſe volo, ſive eam pro ſucco nativo, ſive pro gummi, pro reſina, ſale volatili, bitumine, ſive pro ſuccini ſpecie, ſive pro quocunque quivis eam reputet. Etenim nihil jam aliud in propoſitis habeo, quam peculiarem quandam obſervationem recenſere & quid de ea ſentiam, explicare, denique novas quaſdam ex tali meo judicio deductas doctrinas communicare, ſperans fore, ut alii veriorem ex inde ſententiam de Camphora, quid ea proprie ſit, dicant, alii vero occaſionem capiant, in rem inſtitutis deſuper experimentis convenientibus inquirere, eamque dilucidare.

Traditum equidem eſt in ſcriptis de Camphora editis, poſſe etiam præter ordinariam, quam copioſam ſatis habere poſſumus, aliam ex radice Cinnamomi, Zedoaria Ceylonica, Roremarino Camphorato, Abrotano aliiſque vegetabilibus aromaticis parari, quod certo reſpectu negari nequit, & hac ipſa obſervatione mea confirmatur; nihilo tamen minus confiteri me oportet ante omnia, me nuſquam locorum, quæ peregrinando adivi, talem ex aliis & jam dictis vegetabilibus paratam, licet diligenter perquiſitam, ne in minima quidem quantitate vidiſſe Camphoram, quæ in ſolida & vere ſicca conſiſtentia cum orientali conveniret; ſed quicquid hujuſmodi mihi monſtratum eſt, Amſtelodami fuit illud oleum, quod in India ex radice Cinnamomi deſtillatum dicitur & Camphoram non minus quam Cinnamomum redolet, quamvis dubius adhuc hæreo, genuinum ne illud deſtillatum fuerit, an oleum Cinnamomi ordinarium copioſa Camphora impregnatum? Si quidem multa experientia comprobatum habeo, ſolere vulgo & imprimis in loco jam dicto, varias merces, potiſſimum ad rem medicam pertinentes, fuco inducto effingi, et per miſcelam adulterari: ſed quoquo modo ſe res cum iſto oleo habeat, poſito etiam, nihil fuci ſubfuiſſe, & oſtendi poſſe Camphoram, etiam in ſolida conſiſtentia, ex radice, & ex Zedoaria Ceylonica, aliiſque orientalibus vegetabilibus elicitam nemo tamen, quoad ſcio, ſolidum quid Camphoræ æmulum ex vegetabilibus Europæis unquam produxit; licet de Roremarino multi multa dixerint, & de aliis quoque plantis noſtratibus conjectura præſumſerint; imo licet etiam *Dn. Hofmannus* Profeſſor *Hallenſis* Celebr. & Eques *Angl. Boyle* memoriæ prodiderint, quod ille ſimile quid ex floribus Roſarum & Ligno Aloes * hic vero ex oleo Aniſi impetraverit, † penitus tamen ego ſum perſuaſus, quicquid Camphoræ ſimile ex his tribus productum fuerit, molle quid & unctuoſum, vel ad ſummum butyraceæ conſiſtentiæ, minime vero ſiccum, cryſtallinum, fragile, & Camphoræ duritie æmulum fuiſſe, id quod etiam ex ipſorum deſcriptionibus quivis facile poteſt colligere. Taceo, quod in vulgus notum eſt, oleum Aniſi in frigore butyri inſtar condenſari, minime vero in Camphoram eo ipſo abire. Præter hæc, quæ in ſcriptis commemorata legimus, fuerunt aliqui, & in his certus quidem Pharmacopoeus, cæteroquin rerum ſuarum ſatagens, qui ore tenus mihi affirmarunt, poſſe ſe ex variis plantis indigenis Camphoram parare, cujus modi tamen itidem ne tantillum quidem unquam videre licuit; interim aſſentior illis & lubens credo, potuiſſe hos eadem facilitate, qua ego meam, impetrare Camphoram.

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* *Vid.* Ejus Diſſertat. de Camphora & Obſervat. Phyſico-Chym.
†† *Vid.* Ejus Tr. de Firmitate & Fluiditate.

Placuit igitur ad Inclytam Reg. Societatem referre, acquiſiviſſe me ex noſtrati Thymo vulgari, verum, denſum, cryſtalliformem & in omnibus qualitatibus conſpirantem, nec niſi odore differentem Camphoram, quæ cuncta experimenta huc uſque cognita, & pro juſto examine excogitata poſſibilia, æque ac Orientalis ſuſtinet, atque ita cum hoc in omnibus, ſolo odore excepto, convenit.

Deſtillaveram *Anno* MDCCXIX. ex dicta herba olei copiam bene multam, inſtitutaque hujus ab aqua ſeparatione cum goſſypio, cernebam tam ad orificium vitri, quam in goſſypio oleo imbuto parvos figuratos cryſtallulos hærentes, qui ipſam deſtillationem alias ſatis celerem, magis magiſque remorabantur, adeo, ut neceſſarium eſſet goſſypium, contra morem, aliquoties renovare. Mirabar quid rei novæ & extraordinariæ contigerit, variaſque cogitationes agitabam, parum tamen vel nihil certi ea de re unde evenerit, & quid eſſet, poteram colligere; lagenam igitur oleo plenam, bene munitam, reponebam. Interim ſemel excitatæ & conceptæ de rei novitate cogitationes inquietum me tenebant; quare ſciendi cupiditate ductus, poſt paucos dies, reſumo lagenam olei, viſurus, an denuo cryſtallini quid ad orificium vitri hæreat, & quantulumcunque demum id fuerit, per experimenta tamen parvula probaturus, cujus indolis illud ſit, ſed circa orificium nihil ejuſmodi reperiebam, circa fundum vitri vero, quod me tanto magis attentum, ne dicam attonitum habuit, conſediſſe vidi cryſtallos in ſi ſatis magna quantitate, diverſæ magnitudinis, partim avellanas exæquantes, pleraſque cubiformes, ſaccharo cando ſimiles. Nondum ſuſpicabar Camphoram, ſed potius ſal volatile, quod pro copia herbæ ſucceſſive ſimul tranſcenderit, & in parte aquoſa ſolutum, poſtquam hæc fundum petiiſſet, purumque oleum ſupernataret, tandem ita coaluerit. Effundo igitur oleum, ut ſal, quod eſſe putabam, impetrarem, & judicii mei certior fierem. Sed denuo in admirationem rapiebar, quod ne guttulam quidem aquæ in fundo invenirem, ubi conſultius nihil erat quam magma ſalinum a fundo liberare, & ab oleo ſeparatum ſervare. Quo me igitur ex dubio extricarem, vel potius, ut de ſale, quod judicando præſumſeram, certior eſſem, tentabam experimentum quo cuncta ſalia, cujuſcunque indolis & nominis fuerint, probantur. Magnam ſcilicet quantitatem aquæ deſtillatæ infundebam ſuper materiam cryſtallinam, ſat diu ſimul agitabam, hinc per aliquot horas quieti dabam, ſolutionem cryſtallini per omnes horæ quadrantes cupide, ſed fruſtra expectans, neque enim ſolutionem, neque ullam mutationem ejus animadvertebam, præter quod minimæ particulæ in ſuperficiem elevarentur, majores vero quantumvis bene interagitatæ fundum ſemper repeterent, & aqua particulis oleoſis cryſtallinæ materiæ adhærentibus, ſed agitatione liberatis, imprægnata eſſet. Transfundo aquam, & nova adfuſa, denuo tentabam cryſtallorum pro indole ſalium ſolutionem, poſtquam hi ab adhærente oleo, quod forſan ſolutionem ejus in aqua impediret, liberati eſſent, ſed pertinaciter ſolutioni renitebantur, quam neque adhuc dum, ſæpius licet tentatam admiſerunt. Dehinc, præoccupatione de ſalino concremento plane abecta conjecturare cepi, quod plane quid aliud, ſcilicet mixtum oleoſum, vel quaſi reſinoſum volatile ex oleo condenſatum eſſe debeat. Hoc mecum perpendens, ſub examen revoco varia, alias vere exiſtentia, mihique nota ſubjecta, ſi quod eorum cum novo hoc comparari queat, & mox ob ſimilitudinem cogitabam Camphoram. Sine mora igitur nova quædam, per quæ me magis certum de re fore ſperabam, adornavi, & ecce rei certus eram, ſi quidem omnia quæ modo feci experimenta, non alia quam quæ communis Camphora ſuppeditat, Phænomena exhibuerunt, unde tandem, utpote veritatis convictum, nullo plane dubio relicto, concludere me oportuit, corpus hoc cryſtalliforme aliud nihil quam veram eſſe Camphoram, quæ ab Orientali non niſi in eo differat, quod hæc Thymum, illa vero aliud vegetabile redoleat, quod, quia Camphoram copioſam ſuppeditat, huc uſque nullo alio quam arboris Camphoriferæ nomine indigitatum fuit.

Ex his omnibus ſequentia mihi concepi dogmata, quæ ulteriori cujuſvis indagationi, cum libertate ſentiendi, proponam.

I. Nomen hoc Camphora, vel Caphura, non ſpeciale, ſeu proprium eſt, quod uni ſoli ex India venienti Camphoræ ſemper competat, ſed pro generali, ſeu appellativo potius haberi debet (uti *v. g.* nomen ſalis, olei, aquæ, ſpiritus, &c.) ita ut pro diſtinctione ſpeciei Camphoræ nomen illius vegetabilis addatur, ex quo parata eſt, ſicuti mea Camphora jure meritoque Camphora Thymi dicenda venit.

II. Sub hoc nomine Camphoræ concepta ceſſat omnis controverſia inter autores hactenus agitata de ſpecie, ad quam Camphora pertineat, ſi quidem neque reſinis, neque ſalibus volatilibus, neque oleis & ſuccis accenſenda eſt, multo minus ad gummi vel bitumina referenda, quod cum his omnibus per omnia non conveniat, ſed jam in hac, jam in alia qualitate, jam plus, jam minus præcipui, vel tamen plane diverſi quid habeat.

1. Reſina dici nequit, quia, quod etiam jam Dom. Prof. *Hofmannus* adnotavit.

(a) *Omnes reſinæ poſt combuſtionem, cineres, vel aliud terrei aliquid relinquunt, Camphora e contra plane nihil, ſed tota deflagrat & in auras avolat.*

(b) *Nullæ reſinæ in vaſe clauſo penitus & in totum elevantur ab igne, ſeu ſublimantur, quod tamen cum Camphora ſuccedit.*

(c) *Reſinæ, ubi deſtillantur, phlegma præbent, & oleum, partim etiam ſpiritum ſic dictum, Camphora vero horum nihil.*

(d) *Nulla plane reſina eſt, quæ ad inſtar Camphoræ in ſpiritu nitri ſolvi queat.*

(e) *Nec ulla datur, quæ æque cito & in tanta copia in ſpiritu vini colliqueſcat, neque*

(f) *Talis, quæ in aqua calida Camphoræ inſtar avolet.*

2. Multo minus Camphora ſal volatile dici poteſt, quia in aqua non ſolvitur, nec cum aqua intime, quin videri poſſit, miſcetur, quam tamen ſolutionem inviſibilem cuncta ſalia admittere debent neceſſario, uti ſupra jam commemoratum eſt.

3. Quamvis autem Camphora oleoſum mixtum, vel ex oleo condeſnatum corpus ſit, ideo tamen ſimpliciter oleum vocari nequit, æque ac alia mixta ſolida oleo prægnantia, ſiquidem olei nomen liquidis præcipue, vel ad ſummum tenuiter unguinoſis, minime vero ſolidis & ſiccis ſubjectis tribuitur & convenit; neque ullum oleum in ſicca ſubſtantia, ceu Camphora, ſublimari poteſt.

4. Succi nomen Camphoræ non convenit, quia ſucci proprie dicti mixtionem,

(a) *Vel gummoſam, quæ in aqua facile totaliter liqueſcit,*

(b) *Vel gummi reſinoſam, quæ partim in aquoſo, partim in oleoſo, inflammabili, ſpirituoſo & ſimili ſolvitur,*

(c) *Vel reſinoſam obtinuerunt, qui poſteriores reſinæ propriæ dicuntur, de quibus jam ſub numero* 1. *dictum eſt quarum proprietatum nulla Camphoræ convenit. Nam ſi omnia ea, quæ ex arboribus aliiſque vegetabilibus partim ſponte proveniunt, vel a natura & ſole, partim etiam per artem parantur, quamvis in tota planta ejuſque ſucco & partibus aliis diſperſa & commixta fuerint, ſuccos vocare quis vellet, quantam non confuſionem ineptam & conceptu impoſſibilem pareret in deſcribendis rerum eſſentiis? Sic enim omnis aquæ, ſucci, gummi, olei, balſami, reſinæ ſimiliumque diverſitas plane periret, verbaque hæc diſtinctiva nullius plane uſus forent.*

5. Camphoram etiam gummi vel bitumen non eſſe, ex prioribus jam ſatis apparet, quicquid enim gummi vocatur, penitus in aqua colliqueſcit, ſicuti bitumina maximam partem in ea ſolvuntur, ambo vero ſublimationi refragantur, quam Camphora tamen admittit.

Cum igitur nihil in orbe terrarum cognitum ſit, quod cum Camphora comparari poſſit, æquum etiam eſt, ut ſuo quoque nomine ſingulari & ſpecifico inſigniatur, per quod, ſi res quædam dicatur Camphora, ſtatim intelligatur, eam neque oleum, neque gummi, ſal volatile, reſinam, ſpiritum, neque bitumen, ſed plane aliam & a dictis his diverſam eſſe materiam, mixtum nempe ex ingredientibus, Camphoram veram conſtituentibus, concinnatum, indolis ſpecialiſſimæ. Unde quæſtio, an ſal, vel gummi, reſina, vel ſuccus ſit, æque inconveniens & ſupervacanea judicabitur, ac ſi quis quærere velit, an ſal ſit oleum, vel an gummi ſit Camphora.

III. Tertium dogma, quod ex prioribus deduco, hoc eſt, quod omnis Camphora certum ſuum ſpecificum odorem, & quidem oleum illius plantæ, ex qua parata eſt, conſtanter retineat, quodque odor iſte ſine deſtructione totius Camphoræ ſeparari ab ea nequeat.

IV. Quarto conſequitur, quod experimentum noſtrum de Camphora Thymi orientalem Camphoræ fabricationem ſatis ſuperque confirmet, nempe quod deſtillando paretur, & acquiratur, in quo negotio vix alia intercedit diverſitas, quam quod ex ſic dicta arbore Camphorifera ibidem in magna copia & ita longe plus Camphoræ quam olei haberi queat, cum e contrario vegetabilia *Europæorum* plurimum olei, parum vero Camphoræ largiantur.

V. Quintum dogma eſt, quod certo credere poſſimus, alias quoque plantas *Europæas*, copioſo oleo eſſentiali prægnantes, æque ac Thymum, Camphoram daturas eſſe, quamvis nullam adhuc viderim, neque pro ea elaboranda juſtam occaſionem nactus fuerim, vel otio abundarim.

VI. Tandem ſi qui fuerint, qui plane ſingularem vel peculiarem Camphoræ indolem diſtincte nequeunt concipere, & ex quibus denique conſtet, ſcire velint, in horum gratiam pro mea quidem qualicunque experientia, amice mentem meam ita declarabo; quod Camphora ſit ſiccum quid, album, pellucidum & fragile, valde acute olens, non ſolum in igne, ſed etiam in aere ſubcalido prorſus volatile corpus, quod ratione mixtionis, ex copia principii inflammabilis rarefacti, pauca aquoſitate, & ſumme attenuata vel ſubtiliſſima terra, intime inter ſe unitis conſtat.

1. Conſtat Camphora rarefacto Phlogiſto, quia non ſolum cito & tenuiter, imo etiam in aqua & nive ardet, ſed etiam cum aliis de eodem principio copioſe

participantibus celerrime unitur, ex quibus ſpiritus vini commemoratione, nitri vero ſpiritus meditatione dignisſimus eſt, ſiquidem de poſteriore paucisſima ſunt cognita.

2. Aquoſi aliquid continet Camphora, cum alias principium inflammabile, licet copioſius, flammam exhibere non poſſet.

3. Terram ſubtilem nemo negabit, quia extra hanc ſiccum corpus cryſtallinum non ſiſteret.

4. Aeque minus in dubium vocabitur, terram hanc ſubtilisſimam, & cum coeteris intime, perque minimas, quas vel cogitando concipere poſſumus, partes unitam eſſe, ſi perpendimus, quod Camphora in ſolo aere, aliquantulum tamen ſubcalido in tota mixtura ſua, plane nihil poſt ſe relinquendo, avolet.

Definitio hæc per ea ipſa, quæ hactenus de Camphora Thymi diximus, confirmatur, conſiderando, quod ea ſolummodo ex ſubtili oleo eſſentiali ortum duxerit, in quo oleo itidem non alia, quam ſuperius dicta tria conſtituentia, phlogiſton, aqua & terra demonſtrari posſunt, quamvis ſub notabili hoc diſcrimine, quod

1. Camphoræ mixtura multo ſubtilior quam olei, quinimo (ſi ita loqui liceat) quinta eſſentia & extractum olei ſit.

2. Camphora parum ſaltem aquei principii in ſui mixturam recepit, oleum vero intuitu hujus longe plus.

3. Terra Camphoræ ſumme attenuata, & adeo ſubtilis eſt, ut ſponte ſimul cum coeteris conſtitutivis avolet in auras, cum contra terra olei multo eſt craſſior, id quod etiam, ut alia taceam, in rectificatione vel exhalatione olei videre eſt, ubi primo

reſinæ vel bituminis inſtar, tandem etiam craſſiori ſpecie apparet.

Quod ſupereſt, diſſimulare non poſſum, hæc aliaque experimenta de Camphora facta dubium me habere, an etiam ullo alio quam deſtillationis modo Camphora acquiratur. Præ cœteris, de veritate ſuſpecta mihi videntur, quæ de collectione Camphoræ in Inſula *Borneo* aſſeruntur; dicunt vero quod ibi locorum ſponte Camphora inſtar gummi, vel reſinæ, ex arbore exſudet, atque ita ab incolis colligatur, multo pretioſior quam Japonica, quæque præ raritate tam cara habeatur, ut incolæ illius inſulæ nihil ejus concedant exteris. Sed quoquo modo res cum hac traditione ſeſe habeat, juſtas tamen de ea dubitandi cauſas habeo, & in his potiſſimum Camphoræ volatilitatem, naturæ ejus penitius inſitam; namque ſi ex arboribus exſudaret, non adeo diu ſub ſole fervidiore, quo terræ illius climatis incaleſcunt, perſiſteret, ſed ſat cito in aere libere exhalando fugeret, idque tanto facilius, quia cum exſudaret, multo liquidior eſſe deberet, nec conſiſtentiam Camphoræ duriorem habere poſſet, quæ vero liquiditas ad tanto celeriorem exhalationem diſponeret, ſi quidem per ſe Camphora in molli vel liquida conſiſtentia retineri plane non poteſt cum accepto calore, & ubi colliqueſcit, mox avolat, qua de cauſa etiam cum reſina comparari nequit. Quod vero Camphora nativa, ad inſtar deſtillatæ, in oleo ſuo deſtillato quaſi ſoluta ex arbore profluat, & dein ad arboris corticem Cryſtallorum in modum concreſcat, tanto minus perſuadeor, cum pauca vel nulla potius exempla comperta ſint, quod natura olea deſtillata eſſentialia, aliaque arte facta, inſtrumentis debitis quippe deſtituta, produxerit. Et parum abeſt, quin credam totam traditionem de tali

collectione Camphoræ non niſi ex auditu commemorari, & viros cœteroquin eruditos *Ten Rheine, Breynium, Hermannum* cœteroſque, alium ex alio tranſcripſiſſe, nullum vero, quantum credere par eſt, rem oculis in arbore ipſa conſpexiſſe, quæ quidem fallacia in Hiſtoria naturali prima non foret. Hinc denique concludo, eſſe omnino hanc ſpeciem Camphoræ tam raram, ut eam oculis cernere nemini liceat, nec forſan unquam contingere queat.

C. NEUMAN.

III. *Obſervations of the* Dipping Needle, *made at* London, *in the Beginning of the Year* 1723. *By Mr.* George Graham, Watchmaker, F. R. S.

ABOUT the Time I was obſerving the Variation of the Horizontal Needle, I made likewiſe ſome Experiments with the Dipping Needle, to try, if the Dip and Vibrations were conſtant and regular. The Needle I made for this Purpoſe was 12 Inches and one Tenth long, half an Inch broad in the Middle, but not above one Tenth near the Ends; the Ends themſelves being filed to fine Edges; and in Thickneſs it was about one Third of a Tenth. The Ends of the Axis, upon which the Needle turn'd, were very ſmooth, and not bigger than was neceſſary for the Support of the Needle, which weighed nine Pennyweights twenty one Grains, or about half an Ounce *Troy*. The Ends of the Axis were placed upon the Edges of two thin Plates of Steel, that were hard and well poliſh'd, and parallel to the Horizon, that the Needle, when vibrating, might roll, and not ſlide upon the Edges of the Plates, to avoid the

Friction they would have been ſubject to, by moving in Holes. A Braſs Semicircle was provided, and from the loweſt Point graduated each Way, and a few of the Degrees, about that Part of it which anſwer'd to the Dip, were divided into ſix equal Parts. By the Help of Screws, the Semicircle could be brought to a due Situation; and by two ſpirit Levels, placed at right Angles to each other, any Change of Situation was eaſily perceiv'd, and by the Screws it could be readily reſtor'd to its former Poſition; all was inclos'd with Glaſs to ſecure the Needle from being diſturb'd by the Motion of the Air. I muſt here take Notice of the great Difficulty there is of peiſing the Needle ſo exactly, before it is touch'd with the Loadſtone, as to take any Poſition indifferently: for, when it is pretty near the Truth, it is extremely troubleſome to place it at reſt in the Poſition deſir'd, in order to try which Way it is inclin'd to move. It cannot be done in the open Air; for the leaſt Motion of it will diſturb the Needle, and when it is ſhut up, it is no eaſy Matter to ſettle it in the Place intended. And that there will be a ſenſible Difference of the Dip, upon ſhifting the Sides of the Needle, whatever Pains be taken to prevent it, I am fully ſatisfied from the following Experiments.

March 20, 1722.

EXPERIMENT I.

I Touch'd both Sides of that End of the Needle which I deſign'd to point South, upon the North-pole of a ſmall *Terrella*; after which I cauſed it to vibrate in an Arch of ten Degrees, and counted the Time by a Pendulum Clock, ſhewing Seconds, till the Needle had performed 50 Vibrations.

			'	"
It perform'd the firſt 25 Vibrations in			2	58
The next 25 Vibrations in			2	27
The 50 in			5	25
Which gives for each Vibration at a Medium				6, 5
The Needle dipp'd	73°	15'		

EXPERIMENT II.

Then I ſhifted the Needle ſo as that Side, which before reſpected the Eaſt, was now turn'd Weſt, and cauſing it to vibrate in the ſame Arch, as before, it perform'd

			'	"
The firſt 25 Vibrations in			2	49
The next 25 in			2	39
The 50 Vibrations in			5	28
That is, each Vibration in				6, 56
The Dip	73°	50'		

EXPERIMENT III.

I now touch'd the ſame End of the Needle, a ſecond Time, on both Sides, upon the ſame Stone, and ſuffering it to vibrate, as before,

It perform'd 25 Vibrations in			2'	49"
That is, one Vibration in				6, 76
The Dip	73°	20'		

EXPE-

EXPERIMENT IV.

The Needle was now ſhifted, and ſtood as in the ſecond Experiment.

It perform'd 25 Vibrations in	2'	41''
That is, one Vibration in		6,44
Dip	73°	45'

EXPERIMENT V.

The ſame End of the Needle being now touch'd twice on each Side, with the Loadſtone preſented by Right Honourable the Lord *Paiſley* to the Royal Society, in the Armour,

	'	''
It perform'd the firſt 25 Vibrations in	1	58
The next 25 in	1	46
The 50 Vibrations in	3	44
That is, each Vibration in		4,48
The Dip	73°	55'

EXPERIMENT VI.

The Needle being turn'd, and ſtanding as in the ſecond and fourth Experiments, it perform'd

	'	''
The firſt 25 Vibrations in	2	00
The next 25 in	1	57
The 50 Vibrations in	3	57
That is, each Vibration in		4,74
The Dip	74°	10'

EXPERIMENT VII.

I now touch'd the Needle at both Ends with the ſame Stone, with which it was touch'd in the fifth Experiment, after which it perform'd

			'	''
The firſt 25 Vibrations in			1	35
The next 25 in			1	34
The 50 in			3	9
That is, each Vibration in				3, 78
The Dip	74	20		
The Dip repeated with the Needle taken off and replaced	74	20+		

EXPERIMENT VIII.

Upon ſhifting the Needle, it perform'd

			'	''
The firſt 25 Vibrations in			1	33
The next 25 in			1	34
The 50 in			3	7
The Dip	74°	25'		
The Dip repeated	74	30—		

NB. *The Needle had the ſame Side to the Eaſt in the firſt, third, fifth, and ſeventh Experiments; and had that Side turn'd Weſtward in the ſecond, fourth, ſixth, and eighth; and I began to count the Vibrations, when I obſerv'd it to vibrate juſt* 10 *Degrees, as near as I could gueſs. All theſe Experiments were made with ſufficient Care in every Particular, excepting the Quantity of the Dip, which requires the Diviſions of the Semicircle to be very*

ſmall

equal, and the 90th Degree to be perpendicularly under the Axis of the Needle; this laſt I found was a little faulty, the Dip being in Reality greater than the Semicircle ſhewed it. After I had rectified this Error, and new touch'd the Needle, upon that Part of the Armour to which Iron is applied, when it is to be lifted by the Stone, it perform'd the ſame Number of Vibrations in leſs Time than in any of the former Trials. I now determin'd to obſerve, for ſome Space of Time, both the Dip and Vibrations, without freſh touching the Needle.

The Obſervations follow, by which it appears there is a very conſiderable Difference, both in the Quantity of the Dip, and in the Quickneſs of the Vibrations.

N.B. *In all theſe Experiments, the Needle was placed, ſo as to vibrate exactly in the Plane of the Magnetick Meridian; and ſufficiently diſtant from all Iron that could affect it, as far as I could perceive, till I had Occaſion to put up a very large Iron Rod in the Room above it, which immediately alter'd the Dip of the Needle, and thereby put an End to theſe Trials.*

1723.	Dip	Time of the Day.
	° ′	h ′
Mar. 29.	75=00	at 10=00.
	74=53	4=15
30.	74=55+	1=00
	74=50—	4=00
31.	74=50—	10=00
	74=50—	12=30
	74=50—	2=15
April 1.	74=25	6=45
	74=25—	7=15
	74=20+	9=00
2.	74=20+	7=30. A.M.
	74=20+	7=30. P.M.
3.	74=20+	9=30
	74=20+	12=30
	74=50	4=15
4.	74=55+	10=00
	74=50+	11=15
	74=40	12=45
	74=35	7=30
5	74=40	9=15
	74=40	1=45
	74=40+	5=30
	74=30+	8=15
6.	74=35	10=00
	74=35	12=00
7.	74=35+	10=20
	74=35+	12=30
	74=35	4=00
	74=35	6=30
8.	74=40—	12=15
	74=40—	3=30
9.	74=40—	10=00
	74=40—	4=15
10.	74=40—	10=00
	74—30+	8=00
Apr. 11.	74=35+	10=00. A.M.
13.	74=40—	
14.	74=40—	10=45
	74 40—	11=15
	74=35	5=10
	74=35	8—17
15.	74=35	9=10. A.M.
16.	74=35	11=00
	74=30+	8=45
17.	74=45	12=25
18.	74=40+	9=15
	74=45	5=00
19.	74=45	9=00. A.M.
20.	74=45	
21.	74=50	10=30
23.	74=50	12=00
26.	74=50+	2=30
27.	74=55	
28.	75=00	1=00. P.M.
	75=00	3=15
	74=58	5=15
30.	74=40	3=15
May 1.	74=45	1=30
2.	74=45	12=00
	74=45+	1=00
	74=40+	3=50

The Weight of the dipping Needle 9pt. 21gr. *Troy*.

N.B. The Mark + ſignifies ſomething more than is here ſet down, and — ſignifies ſomething leſs, but the Difference could ſcarce amount to more than two Minutes.

Experi-

Experiments of the Vibrations of the Dipping Needle, beginning with an Arch of 10 Degrees, with the Times in which 100 Vibrations were perform'd,

1723 h ′
April 1. about 7=15 Afternoon.
′ ″
Firſt 50 in 3= 2
Laſt 50 in 2=45

The 100 in 5=47. Dip 74°=25′—

April 2. in the Evening
Firſt 50 in 3= 3
Laſt 50 in 2=43

The 100 in 5=46. Dip 74=20+

April 3. about 4 in the Afternoon.
Firſt 50 in 2=52
Laſt 50 in 2=39

The 100 in 5=31. Dip 74=50

Repeated about an Hour after.
Firſt 50 in 2=53
Laſt 50 in 2=35

The 100 in 5=28. Dip 74=50+

April 4. about 11=15 in the Morn.
Firſt 50 in 2=54
Laſt 50 in 2=30

The 100 in 5=24. Dip 74=50+

April 28. about 5=15 Afternoon.
Firſt 50 in 2=48
Laſt 50 in 2=16

The 100 in 5= 4. Dip 74=58

Repeated
Firſt 50 in 2=47
Laſt 50 in 2=16

The 100 in 5= 3. Dip 74=58

May 20. ′ ″
Firſt 50 in 3=11
Laſt 50 in 3= 1

The 100 in 6=12

Repeated the Needle being new touch'd.
Firſt 50 in 2=38
Laſt 50 in 2=23

The 100 in 5= 1. Dip 74=35

Repeated again about an Hour after
Firſt 50 in 2=38
Laſt 50 in 2=20

The 100 in 4=58. Dip 74=30+

May 21. about Noon
Firſt 50 in 2=41
Laſt 50 in 2=28

The 100 in 5= 9. Dip 74=30

May 23. about 12=45
Firſt 50 in 2=40
Laſt 50 in 2=27

The 100 in 5= 7. Dip 74=40

May 25. about 3=30
Firſt 50 in 2=41
Laſt 50 in 2=30

The 100 in 5=11. Dip 74=40+

May 27. about 6=30 Afternoon.
Firſt 50 in 2=41
Laſt 50 in 2=28

The 100 in 5= 9. Dip 74=50

An extraordinary Caſe in Surgery, communicated by Mr. Joſeph Atkinſon *Senior*, *Surgeon.*

A Maiden, about 20 Years of Age, was brought to me about *Chriſtmas* 1723; who had a Tumour on the inſide of her right Thigh, extending from the Groin to the Knee, which was ſo large, that I judg'd it contained at leaſt the Quantity of a Gallon; the *Cutis* was exceedingly diſtended, but of the natural Colour, only, the capillary Veins appear'd varicous, and very numerous; ſhe had alſo a large Tumour on the Buttock of the ſame Side, of the Bigneſs of a Quartern Loaf; but when the Tumour on the Thigh was preſſed, the Tumour above very much encreas'd, which ſhew'd a Communication, and prov'd afterwards ſo to be: She had alſo another Tumour on her right Side, ſtretching from the left Side of the *Vertebræ* of the Back to the *Hypochondrium*, about the Bigneſs of a Penny Loaf, her Body was very much emaciated, and ſhe could hardly breathe, and the little Victuals ſhe eat very difficultly paſſed out of the Stomach. She had had the *Menſes* but twice or thrice, about 12 Months before the Beginning of thoſe Tumours, and it is to be marked, that the Tumour of her Thigh began firſt, and increaſed to near the Magnitude I firſt ſaw it, before the Tumour of the Buttock and Hip began; after that, the Tumour of her Back began, which, as it increas'd, brought on great Difficulty in Breathing. She had been with ſeveral other Perſons, who adviſed againſt opening the Tumour of her Thigh, moſt of them being of the Opinion it was from Blood, and that her Caſe was incurable: I confeſs, I was of a contrary Opinion; but being told what ſo many others had ſaid, I declin'd meddling with it at that Time, though her Parents and herſelf were willing to it: At this Time I diſmiſſed her, ſaying, that, if ſhe liv'd, a little Time wou'd diſcover more of her Caſe. About two Months after I was deſired to viſit her again, which I did, but the Tumours were ſo monſtrouſly increaſed, and her Body ſo waſted, that I wonder'd ſhe could live under ſuch Circumſtances; the Tumour of the Thigh was every Way yielding to the Preſſure of the Finger, nor was there the leaſt Hardneſs about the Extremities of it, ſo that it might be eaſily miſtaken for an *Aneuriſma*, had it not wanted the grand Characteriſtick, Pulſation, which ſome ſay, is not to be felt when thoſe are very large, the Middle of this then look'd a little red, and ſhining, and ſeem'd to point a little; I told them I believed it would break with a ſmall Orifice, and ſhew what was contain'd, willing them to notify it to me if ſuch a Thing happen'd: Three Days after they call'd me in haſte to this Patient, ſaying, the Swelling of her Thigh was broke; when I came, I found there had been diſcharg'd a ſmall Quantity of matteriſh Subſtance, much like what is contain'd in a *Meliceris*, but the Opening was ſo ſmall or cloſed, that I could not enter my Probe; however, though ſhe ſeem'd to me (and to Mr. *Wall*, an Apothecary then preſent) to be ready to expire, yet, at the Deſire of her Parents, I open'd this Tumour with my Launcet, making an Inciſion about an Inch and an half long, through which poured three Pint Baſons full of Matter, beſides ſeveral ſmaller, which together contain'd about five Quarts, it was very fœtid and bloody towards the latter End of this Diſcharge; upon this, the Tumour wholly ſubſided, inſomuch that the Thigh inſtantly became as ſmall as the other; I put my Finger into the Wound, and found the *Faſcia lata* quite conſum'd, the Muſcles lay all looſe, ſo that I fairly touch'd the Thigh-Bone between them, as I would.

But I ſhould acquaint you, that, immediately upon the Diſcharge of this Humour, the Tumour on the Buttock was conſiderably abated, but there followed about two or three Spoonfuls of florid Blood; I dreſs'd it up for this Time with a proper Digeſtive, and a ſuitable Bandage; the Day following I viſited her again, and found ſhe had ſlept pretty well, and was much refreſh'd, and not the leaſt Faintneſs had attended her, during my Abſence, which ſhews the imaginary *Synocope*, that is feared to follow ſuch, Evacuations, to be groundleſs; the Day following, taking off my Dreſſings, I found the Limb, in its Figure, bigger than the other; at the third Dreſſing there appear'd a ſmall hard Swelling a little below the Orifice, which was made by ſome grumous Blood that lay there, which I turn'd out with my Finger, in Quantity about four Ounces, this was follow'd by a florid Blood, which much ſurpriſed me; I then judg'd this proceeded from ſome Hypogaſtrick Veſſel that ſupplied this Tumour, and I deſpair'd of Succeſs, unleſs I could meet with this Spring; ſo, in order to it, I laid open the Sinews to the Groin, and, though I could not diſcover the Veſſel, yet I ſo ſucceſsfully applied my Aſtringent, that from that Time it bled no more; however, there was, for a Week, a great Flowing of a ſerous Matter, which wholly ſunk the Tumour of the Buttock and Hip, and, by boulſtering and compreſſing with ſuitable Bandage, the ſo long ſeparated *Cutis* clos'd with the

Muſcles, and all Things, in about a Fortnight ſeem'd to be in a fair Way of healing, yet, it was near three Months, before this Cure was compleated.

But ſtill (that is, a Fortnight after the Opening of the Thigh) the Tumour on her Back continu'd, and ſhe much ſtraitened for Breath; ſaying, if that was opened, ſhe ſhould be preſently reliev'd; this Tumour I then open'd (which was, I ſaid, as big as a Penny white Loaf, and there iſſued out about two Quarts of Matter, or rather more: I was again greatly ſurpriſed that ſuch a Quantity, ſo much more than what poſſibly this viſible Tumour could contain, ſhould run out of this Orifice; I enter'd my Probe, and found it penetrate into the Cavity of the *Thorax*, between the ſecond and third ſpurious Rib, reckoning from beneath, on which ſhe reſpired with all Freedom, but there was a *Halitus* at this Wound; I continu'd to dreſs this and believe, before this tended towards Healing, not leſs (at Times) than a Gallon, or rather five Quarts of Matter, was diſcharg'd; but, when I thought all was over, it filled again, the external Tumour became almoſt as big as before, and her Breathing as difficult as ever, ſo that I now thought all my Labour had been in vain; yet I open'd it again with a larger Orifice, and from that Time dreſſed it ſucceſsfully, to the perfect Healing; the *Menſes* are return'd, the Patient continuing well to this Day, and in a good State of Health. This Patient has been viewed by Mr. *John Douglaſs*, and he has had an ample Account of this Affair.

V. *An Experiment to illuſtrate what has been ſaid in the Philoſophical Tranſactions,* N°. 386, 387, 388, *concerning the Figure of the Earth, by the Rev.* J. T. Deſaguliers, L. L. D. F. R. S.

UPON an Axis of Iron, that cou'd be made to turn ſwiftly (by means of a Wheel, whoſe String went round a Pulley fix'd to the ſaid Axis) I ſlipp'd on two Iron Hoops, whoſe Planes interſected at right Angles, repreſenting two Colures, which, being of a ſpring Temper, ſprung in ſuch Manner as to be $\frac{1}{36}$ Part longer in that Diameter that coincided with the Axis, than in their Æquatorial Diameter; this Proportion being the ſame that Mr. *Caſſini* ſuppoſes to be between the Axis and Æquatorial Diameter of the Earth. Two circular Plates, to which the ſaid Hoops were riveted, had ſquare Holes, thro' which the Axis paſs'd, ſo that the two Poles of the oblong Spheroid, which the Hoops deſcribe in their Revolution, might approach together in ſuch Manner, as to let them put on the Form of a true Sphere, when, by the Whirling, the Æquatorial Diameter of the Machine ſwell'd and over-power'd the Elaſticity of the Hoops. A greater Degree of Swiftneſs turn'd the Sphere into an oblate Spheroid of Sir *Iſaac Newton*'s Figure. A Velocity ſtill greater makes the Diſproportion of the Diameters, ſuch as thoſe of *Jupiter*; and ſtill the Æquatorial Diameter encreaſes with the centrifugal Force.

Another Hoop with a Catch, repreſenting the Æquator, ſhews (in the Experiment) the Increaſe of the Æquatorial Circumference, and an Index applied to the Frame, ſhews the Increaſe of the Diameter.

As ſoon as the Revolution of the Machine ceaſes, the Colures, Meridians or Hoops return to their Elliptical Figure, whoſe longeſt Diameter is the Axis of Revolution.

If the Force, by which the Hoops endeavour to keep their Figure, be conſider'd, as the Gravity that keeps together the Parts of the Earth; from this Experiment compar'd with what has been ſaid in the Tranſlations above-mention'd, it will appear that the Earth cannot preſerve its Figure, unleſs it be an oblate Spheroid.

VI. *Some Experiments concerning the* Coheſion *of* Lead, *by the ſame.*

HAVING, on *Thurſday* the 29*th* of *April* laſt, made mention of ſome Experiments, concerning the Coheſion of two Balls of Lead, applied together in a ſmall Part of their Surface (ſo as to require a Weight far greater than what will overcome the Preſſure of the Atmoſphere, to ſeparate them) made by Mr. *Trievall*, at *Newcaſtle*, and at *Edinburgh*; I made the following Experiment to the ſame Purpoſe, before the *Royal Society*.

I took the Leaden Balls A and B (*Fig.* 1.) the firſt weighing one Pound, and the other two Pounds; and having from each of them cut off a Segment of about $\frac{1}{4}$ Inch in Diameter, I preſs'd them together with my Hand, with a little Twiſt, to bring the flat Parts to touch as well as I cou'd. The Balls ſtuck ſo faſt, that when the Hand H, by means of a String, ſuſtain'd the upper Ball A, the lower one B (by reaſon of its Contact at C) was ſuſtain'd, tho' loaded with the

Scale S, and Weights E, which amounted to 16 Pounds. A little more Weight added ſeparated them, and, upon viewing the touching Surfaces, it appear'd that they did not exceed a Circle of $\frac{1}{10}$ Inch Diameter; but this Surface can hardly be meaſur'd exactly, on account of its Irregularity. The Experiment was repeated ſeveral Times, and the Coheſion of the Balls was different every Time.

On *Thurſday* the 6th of this Month, I made the Experiment another Way, at the Meeting of the *Society*, and juſt after their Riſing, before ſeveral of the Members.

On the upper Pin or Bar of the Wooden Frame D *d* l H, I ſuſpended the Steelyard E F, whoſe Hook held up a leaden Ball A of two Inches in Diameter, having an Hole thro' it, at A, to receive a String; the lower Ball B equal to, and prepar'd in the ſame Manner as the firſt, receiv'd the Pin *O o* thro' its String, ſo that G, the Weight of the Steelyard, was made Uſe of to ſeparate the Balls, which happen'd when it was applied at the Number 20, in the firſt Experiment; but, in the three following Experiments, the Balls were not ſeparated till the Weight was remov'd to the Numbers 25, 37, and 45, expreſſing Pounds on the Steelyard.

Laſtly, the Balls being applied together as before, ſtill cleaning the Surface of Contact with my Knife, and never making a Contact ſenſibly greater than what I mention'd before; the Weight G remov'd quite to the End F, where it weigh'd 47 Pounds, was not able to ſeparate the Balls, ſo that I was oblig'd to make Uſe of another Steelyard; but as I intended to begin my Trials with the Weight, at the Number 47 upon this laſt Steelyard, by Miſtake, I applied a wrong Weight inſtead of G, which, being of

7 Pounds, inſtead of 4, ſeparated the Balls. Having found the right Weight afterwards, I made ſeveral Trials, but cou'd not again bring the Force of the Contact to be equal to 47 Pounds. How much greater than 47 Pounds the Force of the Contact was in the 5th Experiment, I cou'd not determine, by reaſon of the above-mention'd Accident; but the Surface was much as before. I ſhall make ſome more Experiments of this Kind, and if I can come to any Certainty in the Meaſure of the touching Surfaces, and utmoſt Force of the Coheſion; I will communicate the ſame, and repeat them before the Society.

VII. *Extract of ſeveral Letters and Certificates ſent to his Majeſty the King of* Great Britain, *concerning a very particular* Nævus Maternus, *or* Mole. *Communicated by Dr.* Steigertahl, *Phyſician to his Majeſty,* F. R. S.

JEREMIAS *Rudolph von Walthauſen*, a Captain of the Garriſon at *Danneberg*, near *Lunebourg*, was born *October* 24, 1680, with a very ſingular Mole upon his right Arm, Shoulder, and Hind-part of his Side, not unlike the Branch of a Vine, with its Leaves and Grapes. It hath been affirm'd and atteſted both by the Deceas'd himſelf, and ſeveral of his Relations and Friends, that his Mother, when big with Child, had an earneſt Deſire for Grapes, and impatient to ſtay, till they were full ripe, went down into the Garden to pull off ſome of thoſe unripe; whereupon it happen'd, that a whole Branch with its Leaves and Grapes ſuddenly fell down upon her right Arm, which Accident much frighten'd her. Some Time after ſhe was brought to Bed, and the Child was obſerv'd to have ſeveral reddiſh or bluiſh Spots, beginning from behind his Shoulder, and from thence running over the ſame, down the right Arm to the Fingers. The Captain's whole right Side was bigger than the Left by an Inch and upwards, and ſo continu'd to his Death. The Veins of the right Arm were much rais'd, lying almoſt immediately under the *Cuticula*, which made them very conſpicuous, they were, beſides, very much diſtended, chiefly between the Elbow and Hand, where they were almoſt as big as a Man's Thumb. On the Inſide of the fore Finger the Vein was extended into a ſmall Tumour, of a reddiſh, or purple blue Colour, about the bigneſs of a Nutmeg, corrugated with ſome lenticular Protuberances, which made it in ſome meaſure reſemble a Grape. The like Tumours, but not ſo big, were obſerv'd in ſeveral other Parts of the Arm, in the Spring Time; and as they thought, when the Sap began to enter the Vines, as alſo when the Vines flower'd, and in *Autumn*, when the Wine was fermenting, the Captain was taken ill with violent and itching Pains in the affected Arm for ſome Days. The whole right Side then ſwell'd more than uſual, and the Veins and Tumours above-mention'd were ſo diſtended with Blood, that at laſt, a ſerous Matter was forc'd out of the Pores of the ſaid Tumours, which as it gave the Patient ſome Relief, ſo he promoted it, by ſcraping the Tumours with the Edge of a Penknife. If the Captain held up his affected Arm, the Running of the Blood backwards in the diſtended Veins was very viſible. If he held his Arm down again, the Blood return'd with ſome Noiſe, and ſenſibly fill'd up again the Veſſels, which by the preceding Action had been emptied. For this Reaſon, the Captain, when he was a Bed, was oblig'd to lay his Arm upwards. In *February* laſt, the Captain was again taken ill with violent Pains, and a ſtrong ſenſible Puſhing of the Blood into the affected Arm; for both which, they being not only more violent, but continuing longer than uſual, he was advis'd to be let Blood on his left Arm; as this did not immediately relieve him, they applied at his Deſire externally *Epithemata* of Plants boil'd in Wine; this eas'd his Pains, and made him ſomewhat more quiet, ſo that he could walk about the Houſe, but he nevertheleſs continu'd reſtleſs for ſome Days, and was on *Saturday* the 17th of *February*, S. N. early in the Morning, ſeiz'd with a violent Oppreſſion of his Breaſt, which in a few Minutes made an End of his Life. The Surgeon, then preſent, did not bleed him, but gave him only ſome Spoonfuls of a ſtrong Cordial Mixture, which he juſt happen'd to have about him, and of which the Patient, with much Difficulty, could ſwallow but a ſmall Part. He was, when he died, 44 Years, 3 Months, and 24 Days old; he was otherways a ſtrong robuſt Man, much given to Fatigue, and diſcharg'd with great Bravery all the Duties of a good Soldier, as appear'd by his Conduct throughout the laſt War.

VIII. *An Account of a Book, entitled,* Hiſtoria Cœleſtis Britannica, tribus Voluminibus contenta, Authore *Joanne Flamſteedio* Aſtronomo Regio.

THE firſt Volume contains the Obſervations of Mr. *William Gaſcoigne* (the firſt Inventor of the Way of meaſuring Angles in a Teleſcope, by the Help of Screws; and the firſt that applied Teleſcopical Sights to Aſtronomical Inſtruments) taken at *Middleton,* near *Leeds,* in *Yorkſhire,* betwixt the Years 1638 and 1643, excerpted from his Letters to Mr. *Crabtree*; with ſome of Mr. *Crabtree*'s Obſervations of the ſame Years; as alſo Obſervations of the Sun's and Moon's Diameters; Configurations and Elongations of *Jupiter*'s Satellites from him, ſmall Diſtances of fixed Stars, with Appulſes of the Moon and Planets to them, obſerv'd with a Teleſcope and Micrometer at *Derby,* by Mr. *Flamſteed,* betwixt the Years 1670 and 1675; with the larger intermutual Diſtances of fixed Stars, and of the Planets from them; Eclipſes of the Sun, Moon, and *Jupiter*'s Satellites, Spots on the Sun, Comets and Refractions, taken with a Sextant of near ſeven Foot *Radius,* a voluble Quadrant, and the above-mention'd Inſtruments, betwixt the Years 1675 and 1689, at his Majeſty's Obſervatory, rank'd under proper Heads, with the Places of the *Moon, Saturn Jupiter Mars, Venus* and *Mercury,* deduced from the Obſervations, and alſo neceſſary Tables to be us'd with them.

The ſecond Volume contains his Obſervations (made with a mural Arch of near ſeven Foot *Radius,* and 140 Degrees on the Limb) of the meridional

Zenith Diſtances of the fix'd Stars, Sun, Moon, and other Planets, with the Time of their Tranſits over the Meridian, together with Obſervations of the Sun's and Moon's Diameters, Eclipſes of the Sun, Moon, and *Jupiter*'s Satellites, Variations of the Compaſs from 1689 to the End of the Year 1719, *&c.*

Tables ſhewing to render the Calculation of the Stars and Planets Places, from the Obſervations, eaſy and expeditious; to which are added the Places of the Moon, (at the Oppoſitions, Quadratures, and on her Limits, *&c.*) and the Places of *Saturn*, *Jupiter*, *Mars*, *Venus* and *Mercury*, derived from the above-mentioned Obſervations.

The third Volume contains a Catalogue of the right Aſcenſions, Diſtances from the Pole, Longitudes, and Magnitudes of near 3000 fixed Stars, with Variations of their right Aſcenſions and Diſtances from the Pole, whilſt they change their Longitudes one Degree, whereby their right Aſcenſions and Diſtances from the Pole may be determin'd for 200 Years paſt, or to come, ſufficiently exact. Large Tables, by which the right Aſcenſions, and Diſtances from the Pole of the Stars and Planets, being given, their Longitudes and Latitudes may be found by Inſpection. To this Volume is prefix'd a very large Preface; containing, an Account of all the Aſtronomical Obſervations made before his own Time, with a Deſcription of the Inſtruments made Uſe of; as alſo an Account of his own Obſervations and Inſtruments, together with a new *Latin* Verſion of *Ptolomy*'s Catalogue of 1026 fixed Stars, from the *Greek*, and *Uleg-beig*'s Places annexed on the *Latin* Page, with the Corrections; ſmall Catalogues of the *Arabs*, *Tycho Brahé*'s of about 780 fixed Stars, in a proper Order; the *Landtgrave* of *Heſſe*'s of 386; *Hevelius*'s of 1534, in a proper Order. A Catalogue of ſome of the Southern fixed Stars, not viſible in our Hemiſphere, reduc'd to right Aſcenſion, Diſtance from the Pole, Longitude, and Latitude, with Variations of right Aſcenſions, and Diſtances from the Pole; calculated from Obſervations made by Dr. *Halley*, at St. *Helena*, and Mr. *Flamſteed*'s Star's Places, and fitted to the Year 1726.

FINIS.

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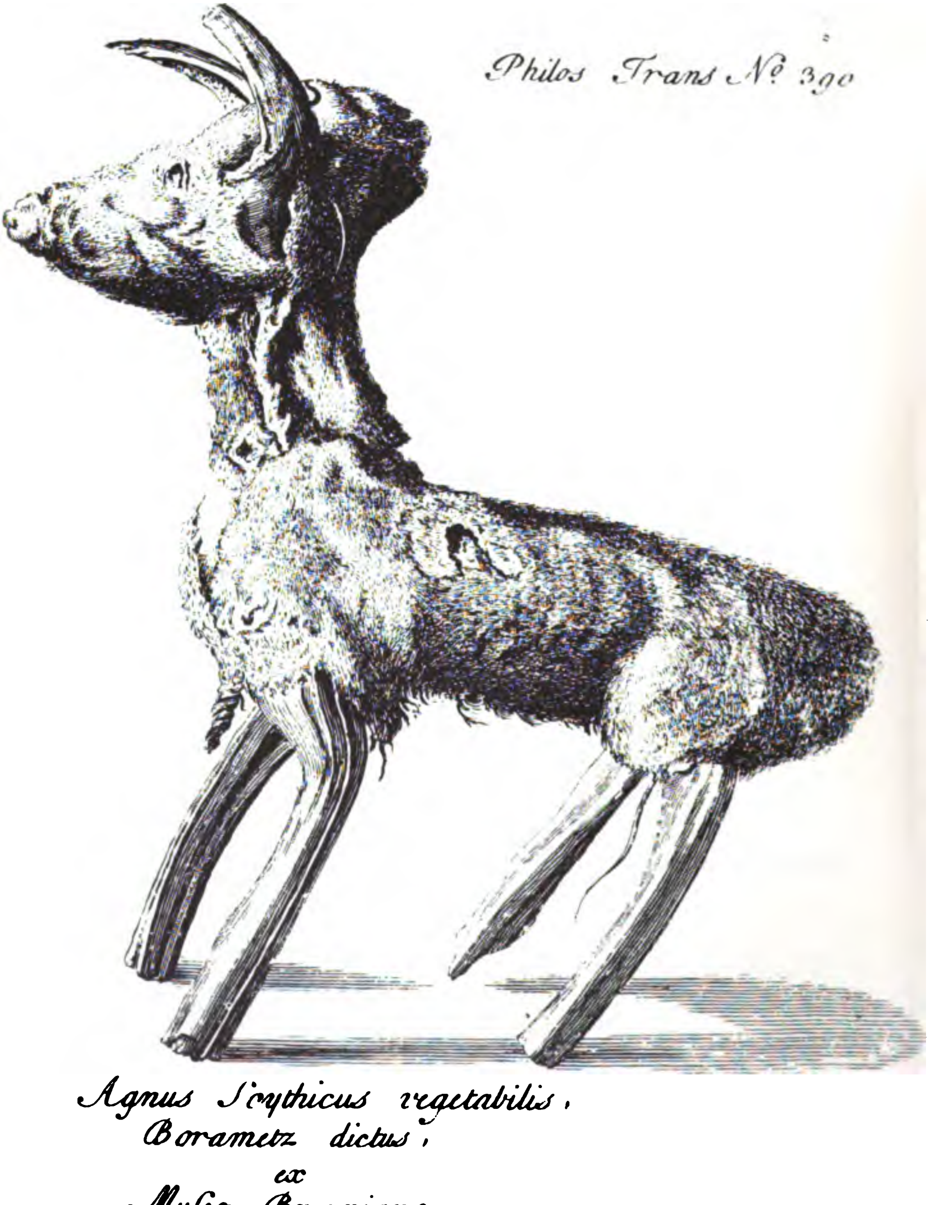

Agnus Scythicus vegetabilis,
Borametz dictus,
ex
Muſeo Breyniano.

Numb. 390.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *September* and *October*, 1725.

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ERRATA.

NO. 389. Page 341. line 31, 32. read *three quart Baſons* p. 342. l. 15. r. *Syncope*. l. 18. r. *but little bigger*. l. 27. r. *Sinus*.

I. *Diſſertatiuncula de Agno Vegetabili Scythico*, Borametz *vulgo dicto. Auctore D^no* Joh. Phil. Breynio, *M. D.* Dantiſc. *R. S. S.*

I.

NON fingendum, ſed inveniendum, quid Natura faciat, aut ferat. Aurea hæc verba utinam continuo animo obverſarentur illorum, qui reconditos Naturæ theſauros quærunt & ejus arcana rimantur. Hoc ſi factum fuiſſet olim, certe Hiſtoriam Naturæ per ſe ampliſſimam, tot ac tantis non haberemus inquinatam anilibus fabulis & opinionum commentis. Cujus rei exemplum nobis inpræſentiarum præbet Agnus dictus Vegetabilis Scythicus, Barbaro nomine *Barametz*, *Borometz* vel *Boranetz* inter Hiſtoriæ Nat. Scriptores notus.

II.

De hoc Agno Vegetabili imprimis egerunt Athanaſius Kircherus in Opere de Arte Magnetica, (*a*) qui citat Sigiſmundum L. B. ab Herberſtein, Haytonem Armenum, Surium & Jul. Cæſ. Scaligerum, Franciſcus Baco de Verulamio, (*b*) Fortunius Licetus, (*c*) Andreas Libavius, (*d*) Euſebius Nierenbergius, (*e*) Adamus Olearius (*f*) & Olaus Wormius, (*g*) ut cæteros, quos inter multi Botanici, qui eandem fere canunt cantilenam, nunc taceam.

(*a*) *p.* 504, & 505. (*b*) Hiſtor. Natur. Cent. 7. N°. 609. (*c*) de Spontaneo Viventium Ortu. C. 45. (*d*) Hiſtoria Agni Scythiæ. (*e*) Hiſtor. Natural. *p.* 34. (*f*) de Itinere Perſico. *p. m.* 155. (*g*) Muſ. *p.* 190.

III.

HUNC Julius Cæſ. Scaliger (*b*) titulo Agni Scythici, Borametz, ſequentem in modum deſcribit: “ Superiora ludum putes, prout eſt admirabilis Tartaricus frutex. Tartarorum horda primaria Zauolha eſt, vetuſtiſſimæ nobilitatis commendatione. In eo agro ſerunt ſemen ſeminis Melonis ſimillimum, ſed minus oblongum. Ex eo ſatu plantam exire, quam Borametz, id eſt, agnum vocant. Creſcit enim agni figura ad pedum fere ternum altitudinem: quem pedibus, ungulis, auribus, toto capite, præterquam cornibus, repræſentat. Pro cornibus pilos gerit, ſingularis cornu ſpecie. Obducitur corio tenuiſſimo: cujus detracti uſus ad capitum tegmina incolis. Ferunt internam pulpam Gammari referre carnes. Cæterum è vulnere quoq; ſanguinem manare. Dulcore eſſe admirabili. Radicem humo exertam ſurrigere ad umbilicum uſq; Illud miraculi fovet magnitudinem. Quandiu vicinis obſidetur herbulis, tamdiu vivere, quaſi agnum in læto paſcuo. Abſumptis illis, tabeſcere, atq; interire. Idque non ſolum vel caſu, vel tractu temporis, ſed etiam experiundi gratia, ſubtractis, atq, ablatis evenire. Quin illud auget admirationem: appeti à Lupis eam, non item ab aliis beſtiis, quæ carne veſcantur. Hoc quaſi condimentum, atq; intritum, ad fabulæ, & agni alluſionem. Illud ſcire velim: Ab uno ſtipite, quatuor diſſita crura cum ſuis pedibus qui poſſint provenire, atque produci.

“ Hæc, quod non ignorarem haud ingrata fore tibi, cæteriſq; Philologiæ ſtudioſis, enarravi partim à nobi-

“ liſſimis,

(*b*) De Subtilitate Contra Cardanum Exerc. 181. *Sect.* 29, & 30.

" liſſimis, partim ab exercitatiſſimis in rerum Natura
" viris non ſolum lecta, ſed etiam audita: quibus in-
" genii tui ampliſſima ſpatia implere aliqua ex parte
" poſſes."

Pari ratione cæteri eum deſcribunt, vel potius ex Scaligero exſcribunt Authores, quorum tamen nonnulli in quibuſdam circumſtantiis variant, & Athanaſius Kircherus ejuſdem addit, vel ut rectius dicam, fingit figuram. Imo in nonnullis Rerum Nat. Muſæis, ut in Wormiano, Swammerdammiano, *&c.* ejus, uti volunt, quondam demonſtrabatur detracta pellis.

IV.

ANTONIUS DEUSINGIUS (*i*) rem accuratiori rationis trutina examinans, fabuloſa eſſe, quæ de Agno hoc traduntur, ſubolfecit, & ipſum Julium Cæſ. Scaligerum, qui inter primos Authores, ut ſupra dictum, ejus meminit, eum ut fabulam tractâſſe evincere conatur: Quemadmodum & alii, minus creduli, eundum in dubium vocârunt.

V.

ET revera totam hanc de hoc Agno Hiſtoriam, ſi ab animo præjudiciis vacuo accurate examinetur, fabulam ſapere, imo eſſe, & Deuſingium recte judicâſſe, ſequentes evincunt rationes.

1°. Quia à nullo fide digno Authore Agnus ejuſmodi Vegetabilis unquam viſus. Quæ enim Olaus Wormius (*k*) narrat ex relatione D. Eovaldi de Kleiſs, Electoris Brandenburgici Legati, (ipſi ſcilicet in confinibus Tartariæ degenti à Tartaro quodam fuiſſe oblatam ejuſmodi plantam exſiccatam, foliis Tabaci, cu-

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(*i*) De Agno Vegetabili, quod extat in Faſciculo ejus Diſſertationum ſelectarum, *p.* 598. & *ſeq.* (*k*) *l. c.*

jus cauli adhærebat fructus, agnellum figura plane referens, magnitudine pedali, vellere criſpo tectus) ſuſpecta ſunt, cum Vir ille Nobiliſſimus, ab aſtuto quodam Tartaro, ipſi fucum faciente facile ſeduci potuit. Quid vero de pellibus, quæ hoc nomine in Muſæis demonſtrantur, ſentiendum ſit, ex infra dicendis apparebit.

2°. Quia à Doctiſſ. & Rerum Nat. Scientiſſimo Engelberto Kæmpfero, M. D. (l) in ſuppoſita hujus Agni patria anxie licet quæſitus, tamen nihil huic ſimile inventum; hæc enim ejus ſunt verba: "Quia de "exiſtentia Zoophyti gramina paſcentis, nullibi Tar- "tariæ apud vulgum, vel Botanicæ peritos extat "notitia & memoria, prout ipſe ad riſum & nauſeam "exploravi, neq; ulla res Borometz dicta, præterquam "ovinum pecus ibidem poteſt reperiri, meram eſſe, "quicquid de hac planta proditur, fictionem & fa- "bulam aſſerimus."

3°. Denique quia tota de hoc Agno relatio fabulæ adeò videtur ſimilis, ac ovum ovo.

VI.

FABULÆ autem hujus originem optime detexit diligentiſſimus Naturæ in Orientalibus Regionibus Scrutator, jam jam laudatus Kæmpferus loc. cit. ubi (præmiſſis de Etymologia vocis Borometz, quod corruptum ait ex Moſcovitico Boranetz, Polonicè Baranek, quod diminutivum eſt vocis Baran Sclavonicæ originis, Polonis Ruſſiſq; ovem ſignificantis) ait, in quibuſdam Provinciis circa Caſpium Mare eſſe quoddam ovium genus, præter vulgare nobis cognitum, quod ab eo in multis diſcrepat, imprimis commendabile

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(l) Obſervat: de Agno Scythico ſ. fructu Borometz qu; exſtat in Diſſert. ſua inaugurali, nec non in Amoenitatum Exoticarum Faſciculo III. Obſ. 1.

bile pellium elegantia, quas deſcribit, & qua ratione ad uſus pro veſtimentis adornandis Tartaris Perſiſq; familiares præparentur, docet, & addit: “ Magnatum
“ divitumq; faſtum ſupra vulgi ſortem amiciri cupientem,
“ pullorum pelliculas expetere, qui annoſis
“ multo teneriores & eo quidem cariores, quo juniores
“ fuerunt agnelli, quibus detractæ ſunt; horum enim
“ pili à pellionibus cogi patiuntur in ſubtiliores &
“ denſiores cirros, qui toti pelliculæ pretium & venuſtatem
“ tribuunt. Inde fit, ut teneritatis & lucri
“ inſidiatores non vereantur ipſam anticipare, crudeli
“ laniena, nativitatem, & matribus uterum gerentibus,
“ diſſecto ventre partum ante partum eximere, ſolius
“ gratia pelliculæ obtinendæ. Hæc rite elaborata,
“ tam dubiam prorſus & delicatiſſimam teneritatem
“ exhibet, ut reſectis extremitatibus, vix agninæ cutis
“ retineat ſimilitudinem, ſed ignaram credulitatem,
“ ſpecie quadam membranæ cucurbitinæ lanugino-
“ ſæ poſſit decipere. His ſubjungit: Pretium pelliculæ,
“ pro bonitatis opinione, exſurgit ad aureos
“ tres, vel amplius; ſervit ea pro duplicatione mitrarum,
“ nec raró, ornatus gratia, togarum & amiculorum
“ limbum conſtituit. Tandem concludit: Sive
“ fabula hæc natales debeat conjecturæ alicujus contemplatoris
“ Philoſophi, ſive inſcitiæ tribuenda ſit primi
“ relatoris, qui per linguæ ignorantiam, vel incuriam,
“ in parergo rem auditam perperam intellexiſſe
“ potuit, ſive cuicunq; acceptam referamus
“ occaſioni alii, qua per longe diſſitas regiones paſſim
“ provecta, ambiguæ teneritatis pellicula, integritatem
“ hiſtoriæ ac nominis ſui amiſerit: donec tandem
“ appulſa nobis cum prodigii lepore, in Virum
“ illuſtrem, curioſum & hujus peregrinæ lanuginis
“ admiratorem incidens, vegetabili vultu ſuo fidem
“ prodigii, ut ſolent mirabilia omnia, nullo negotio
“ fecerit.

« fecerit. Sic authoritate illuſtratus error, mox ſcriptis
“ quoq; firmatus, ſapientiſſimorum ingenia & vulgi
“ opinionem ita occupavit, ut hodie pro Zoophiti
“ ſpecie inter rariora Muſæorum oſtentari ſoleat, quæ
“ veriſſima Cæſarei foetus pellicula eſt.” Hactenus ille.

Ex citatis patet pelles illas agninas Perſicas, quæ à Pellionibus noſtris **Perſianiſche Baranken** ſalutantur, hujus eſſe generis, quamvis non optimæ notæ, illorum ſcilicet agnorum, qui Cæſarea ſectione ex Matrum uteris exſcinduntur, cum earum pretium, teſte Kæmpfero ad aureos tres, vel amplius in ipſa eorum patria exſurgat, noſtræ verò pretio longe minori apud nos, unico ſcilicet aureo, ad ſummum veneant.

VII.

INTERIM ante triennium circiter Vir quidam eruditus & curioſus ex Moſcovia iter faciens & Urbem noſtram tranſiens, Muſæolum meum, inter alia quædam Naturalia, Agno ejuſmodi, ut vocabat, Scythico, quod pro genuino Borometz, ceu magnum Κειμήλιον venditabat, ditavit. Hic ſex pollicum præter propter erat longitudinis, capite cum auribus & quatuor cruribus inſtructus, coloris ferruginei & totus lanugine quadam inſtar panni holoſerici villoſi **Sammet** vulgo dicti, tectus, auribus & cruribus, quæ glabra erant, coloris ad fuſcum magis vergentis, exceptis. (*) Ad examen vocatum cognovi, non eſſe animalis naturæ, neq; fructum alicujus plantæ, ſed radicem cujuſdam vegetabilis craſſam, reptantem & villoſam, vel potius caulem ſcandentem plantæ alicujus, qui artè obſtetricante figuram aliqualem animalis quadrupedis acquiſiverat. Crura enim quatuor erant tot reliquiæ caulium,

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(*) *Vid.* Figuram naturali dimenſione expreſſam.

lium, vel ſi mavis pediculorum abſciſſorum, qui folia ſuſtentaverant, quemadmodum & aures, qui tamen cornibus ſimiliores; præterea hinc inde emergentes fibræ, per quas more cæterarum nutrimentum radix craſſa, vel potius caulis & per hunc planta ceperat, nullum amplius relinquebant dubium. Quartum quoq; crus anterius, non ut cætera corpori continua, ſed arte erat intruſum, quemadmodum & ipſum cum collo caput ſubtili modo annexum, accuratiori examine deprehendi. Ut adeò hic Agnus eodem artificio ex radice hac vel caule formatus fuerit, quo Homunciones ex Mandragoræ & Bryoniæ radicibus, qui **Allraunen** vulgo dicuntur ab Agyrtis. Remanſit mihi tamen dubium, ex qua planta hoc ludibrium artis & naturæ efformatum ſit, quanquam mox ſubiit cogitatio illud plantæ cuidam ex capillarium genere vulgo dicto adſcribendum eſſe, cujus varia habebam indicia, ab analogia mihi cognitarum quarundam exoticarum ſpecierum, nonnullarumq; ab Illuſtri Dº Hans Sloane & Rev. P. Carolo Plumier in ſplendidiſſimis ſuis Operibus deſcriptarum & delineatarum; utpote, quarum diverſæ caules ſcandentes ejuſmodi lanugine ferruginea ſ. rufeſcente muſco, ut vocant, tectos emittunt. Interim tamen, ex qua ſpecie præciſe, determinare non valui. Peculiarem itaq; forte nondum deſcriptam ſpeciem eſſe, quæ in Tartaria reperitur, arbitror, donec cum tempore certiora edoctus fuero.

VIII.

HANC meam ſententiam confirmant, quæ poſtmodum legi in Tranſactionibus Philoſophicis Anglicanis (m) ubi Illuſtris Ds. Hans Sloane, Collegii Med.

(m) Nº 287. *p.* 861. quod etiam reperitur *in the Philoſophical Tranſctions abridged by* John Lowthorp', *Vol.* 2. *p.* 646. *f.* 160.

Lond. nunc Præſes & Regiæ Societatis Vice-Præſes digniſſimus ſimilem Agnum Scythicum fictum ex India Orientali nactus, deſcripſit & delineavit, qui tamen longe minus Agni figuram refert, quàm meus memoratus. De ejus origine eandem modo laudatus Vir mecum fovet ſententiam; cæterùm ait, lanuginem ejus in uſu eſſe contra Hæmoptyſin, ad ſex grana pro doſi ter exhibitam, præterea ejuſdem lanuginis mentionem fieri à Merretto (*n*) titulo Poco Sempie, *ſ.* a Golden Moſs, ubi Virtus ipſi cordialis adſcribitur. Porro ſibi certò relatum, lanuginem hanc in China adhiberi pro ſiſtendo ſanguine in vulneribus recentibus, ut apud nos telæ araneorum, ibidemq; in tanta æſtimatione haberi, ut paucæ familiæ hac careant; dum ipſe ſæpius in ſputo ſanguinis uſurpatam meminerit, ubi tamen non infallibile remedium hoc, licet innocens, expertus eſt.

IX.

Credo autem ejuſmodi Agnos ex certis radicibus vel caulibus in Moſcovia & Tartaria effingi, ut aliquo modo Hiſtoriæ Agni Scythici Vegetabilis adſtruatur Veritas. Quis autem non videt hunc ab eo, qui a ſupra citatis Authoribus deſcribitur, plane diverſum, nec tanta admiratione dignum eſſe, cum etiam hic ex variis radicibus varia portenta, quæ rerum naturalium quarundam ſimilitudinem aliqualem habeant, effingi poſſint; quemadmodum ſupra de Mandragoræ radicibus retuli, dum interim hic non magis agnus dicendus, ſed radix vel caulis revera eſt & manet plantæ cujuſdam, ac Homunciones Allraunen, Belgice Pisse Diefjes (*o*) dicti, radices Mandragoræ.

II. *De*

(*n*) In Appendice ad Nehemiæ Grævii Muſ. Soc. Reg. *p.* 386.

(*o*) De quibus Vid. Deuſingius de Mandragoræ Mangoniis, quod extat in Ejuſdem Faſciculo Diſſertationum, *p.* 586. nec non Miſcellan. Natur. Curioſ. D. 1. A. 1. Obſ. 48.

II. *De Camphora.* *Auctore D^no^* Joh. Brown, *Chym. R. S. S.*

IN tractatu nuper à Domino *Neuman*, Germano, in Angliam miſſo, cui Titulus de Camphora, quique coram hâc Societate lectus fuit, inter alia Vir Doctus hæc aſſerit. "Acquiſivi ex noſtrati Thymo "Vulgari verum, denſum, cryſtalliformem, & in "omnibus qualitatibus conſpirantem, nec niſi odore "differentem Camphoram ; quæ cuncta experimenta "huc uſque cognita, & pro juſto examine excogita- "ta poſſibilia, æque ac orientalis ſuſtinet, atque ita "cum hoc in omnibus, ſolo odore excepto, convenit." .... Quibus verbis paulo poſt ſubjungit, "ſe hanc "Camphoram ex Oleo Thymi deſtillato obtinuiſſe."... Hæc ille : de quibus non inutile erit pauca obſervare.

Primo mihi animum ſubit, Quoddam apud nos eſſe eodem modo è noſtrati Thymo deſtillatum, quod hactenus improprio Salis Thymi Volatilis nomine dictum fuit : non tamen video quomodo Camphora Thymi verius dici poteſt. Equidem non in dubium voco quod ex propria experientia aſſeruit Vir Doctus de eo, quicquid demum ſit, quod è Thymo extraxit, quodque appellat Camphoram : Liceat tamen mihi, pace viri eruditi, paucis, notare differentias, quas ipſe obſervavi inter Camphoram proprie dictam, & illud quod è Thymo apud nos deſtillatum habemus.

Præparatio noſtra eſt hujus modi : Oleum Thymi coagulatum ſeu condenſatum, pellucidum, cryſtalli formâ indutum, variæ magnitudinis, variæque figuræ, poſt primam ſeparationem ſaccharo cando albo, mox vero rubro, ſimile ; valde Thymum ſpirans, pungentis vel etiam cauſtici ſaporis, non fragile ut Camphora. Hoc in quibuſdam experimentis à vulgari Camphorâ, qualis in officinis venditur, non multum differre fateor.

Eadem ſpecifica gravitate videntur eſſe.

Neutrum horum in aquâ ſolvi poteſt. In Spiritu Vini ambo facile ſolvuntur.

In aliis vero Phænomena adeo ſunt diverſa ut mea ſententia nomen Camphoræ utrique competere non poſſit.

(1.) Camphorâ in Spiritu Vini ſolutâ & poſt in aquam projectâ, ſubito coagulum album ſupernatat, quod reverâ apparet eſſe Camphora, odore, ſapore, & ſubſtantiâ. Quod ſi eadem in oleo noſtro coagulato experiri velis, videbis in aquæ ſuperficie guttas parvas olei liquidi innatare, quæ ſenſim in cryſtallos planos (cryſtallis Lunæ non multum abſimiles) formantur, etiamque ſaporem & odorem Thymi retinent. Diverſitas adhuc magis è ſequenti experimento comparuit. Utramque mixturam ſupradictam tota nocte in aere frigido expoſui: Mane coagulum, quod prius Camphoræ in Spiritu Vini ſolutæ innataſſe dixi, prorſus evanuerat, aquâ Camphoræ odorem adhuc retinente; (forte propter ſpiritum vini admixtum:) In altero vero, nempe Oleo Thymi coagulato, cryſtalli aquam ſupernatantes, in eodem ſtatu, quo ante fuerant, remanebant, nec quidquam erant deminuti.

(2.) Camphoram ſolidam per vim ſpiritus nitri in liquorem pellucidum mutatum iri, quis in re chymicâ vel minime verſatus non novit? At oleum noſtrum coagulatum, ſi eidem menſtruo adjiciatur, priſtinam quidem formam cryſtallinam penitus amittit, & ſubſtantiæ reſinoſæ ſeu gummoſæ formam induit, neque, quantumvis vaſculum, in quo continetur, validâ manu & ſæpe concutias, unquam in fluidum evadet

vadet. Preterea, ſi liquorem illum pellucidum, in quem Camphora redacta erat, in aquam poſtea effundas, redintegratur Camphora: contra, oleo noſtro coagulato eadem ſubſtantia reſinoſa, ſeu gummoſa, adhuc manet, & ſapor Thymi adhæret cum notabili amaritudine adjecta.

(3.) Oleum Vitrioli (ut docuit noſter Boyleus) Camphoram in totum diſſolvit: in Oleum vero Thymi coagulatum, quod ſupra deſcripſimus, nullam aliam mutationem inducit, niſi quod vi manûs concutientis cryſtalli in minores partes dividantur; & hoc modo cryſtalli olei, ſi microſcopio obſerventur, fragmentis Camphoræ ſe magis aſſimilant.

Inſuper Camphora Oleo Vitrioli ſoluta ſe ab hoc menſtruo recuperat, ſi poſtea in aquam fundatur: noſtrum vero eandem formam cryſtallorum, quam in Oleo Vitrioli habuit, etiamſi in aquam effundas, una cùm ſapore & odore Thymi, adhuc retinet.

(4.) Scribit D^nus^ *Neuman Camphoram conſtare rarefacto Phlogiſto* (hæc ſunt ejus verba) at minime quidem noſtrum: nam ſi igni admoveatur, extemplò colliqueſcit, & oleum liquidum fit. Fatendum quidem eſt, ſi goſſipium immergas, idque deinde igni admoveas, flammam ſtatim concipit: ſed hoc goſſipio deberi clarum eſt, idemque oleis omnibus commune.

(5.) Ignis in duo iſta corpora diverſos effectus habet. Nam qualiſcunque ignis gradus idoneus eſt ad ſublimandam Camphoram ſine ulla liquefactione, idem corpus hoc è thymo deſtillatum & condenſatum prorſus liquefaciet, & liquefactum tenebit cum nulla vel minimâ ſublimatione. Hactenus de differentiis.

Quibus non abs re fuerit teſtimonia quædam de oleis coagulatis, ſeu ſalibus ex oleis natis, huc pertinentia, ſubjungere.

Quidam ex noſtris ſociis, rei chymicæ admodum peritus, Dom. *Slare*, in Philoſoſophicis Actis, ab hinc pluſquam triginta annis editis, oleum noſtrum hiſce verbis deſcribit, Ex ſatis magnæ copiæ Thymi deſtillatione, ſeparatum eſt ſal volatile indolis ſingularis, (quod Peritiſſimus noſter Chymicus D. *Moult* primum mihi exhibuit) hoc eſt pellucidum, vel cryſtallinum, quoad gravitatem ſuam ſpecificam paulo aqua ponderoſius, idemque & ſal & oleum in uno corpore coagulatum eſſe videtur : in aqua non ſolvitur, ſed calefactum facile avolat.

Idem Vir Doctus ſe Oleum Cinnamomi viginti annos aſſervaſſe dicit, & inta ſex annos poſtremos, ſingulis annis quoddam ſalis præcipitatum fuiſſe, & hoc ſine ullo additamento aut arte ad ſalis formam inducendam. Sed quoad hujus ſalis naturam, an ſimile fuerit ei, quod ex Thymo deſtillatum oſtendit D. *Moult*, ſilet.

Helmontius equidem de ſale ex eodem Oleo, arte facto ſic locutus eſt. " At quando Oleum Cinna-
" momi ſuo ſali Alkali miſcetur, abſque omni aqua,
" trium menſium artificioſa occultaque circulatione,
" totum in ſalem volatilem commutatum eſt ;" etiamque in alio loco modum, quo Oleum Cedri ſalis naturam aſſumat, vi menſtrui ſui Alkaheſt, docet, additque hæc verba, " ſeſe permiſcebit aquæ.

Cl. *Boerhavius* (ſi modo Chemiæ inſtitutiones ejus nomine nuper editæ, genuinæ ſunt) ſic ſcribit, " Diu
" aſſervata olea pretioſiſſima aromatum orientalium,
" vaſis bene clauſis, mutantur in ſalem, ſeu ſaponem ;
" ſed experimentum non facile imitari poſſumus, nam
" docente *Hombergio*, demum fit per multos annos,
" & dicuntur tunc aquæ miſceri."

In alio loco cum Dom. *Neuman* meritò nunc in Societatem noſtram admiſſo, conſentire videtur *Boerhaavius*. " Camphora eſt nil niſi Oleum per deſtillationem concretum in glebam pellucidam; nam non eſt progenies ſolius arboris Camphoræ, ſed omnes plantæ aromaticæ Camphoram ſui generis producere poſſunt;" & paulo poſt "Oleum quod in Inſula Ceilon & alibi ex ſemine Cardamomi fit, fere totum in Camphoram mutatur, quod idem verum eſt de pluribus Oleis ibi nalis." At vero ante ſex vel quatuor dies Olea varia in India deſtillata, qualia ſunt Oleum ex cortice Caryophill. & Calam. Aromat. & radicibus Caſſumun. & ſimilia perluſtravi; atqui nullam in his materiam condenſatam ſive coagulatam reperire potui, quamvis pluſquam decem annis hic recondita fuiſſent. Neque dicere poſſum cum D. *Geoffroy* jun. (*Memoires de L'Académie Royale* 1721.) me unquam obſervaſſe Cryſtallos ex Ol. Terebinth. vel Majoranæ, parietibus vaſium, in quibus aſſervata fuerant, adhærentes, ſimiles Cryſtallis Camphoræ; quamvis vaſa multos annos his uſibus applicata diligenter inſpexi.

Quod ad hoc Sal, ſeu Oleum coagulatum Thymi attinet, idem aliis in hac urbe, in re chymica verſatis notum eſt; nam hujus parvam quantitatem (niſi quod colore ſit candidiore) noſtro ſimilem vidi: at in nullis aliis Oleis aliquid hujus generis me vidiſſe memini, niſi forte mihi perſuadere liceat, ineſſe hoc tempore in ſuis principiis, (ſi ita dicam) in Oleo Macis, à nobis ab hinc jam quindecim menſibus deſtillato, cujus olei eliciendi hæc eſt narratio.

Satis magnam quantitatem Macis pro una operatione recepi, totoque die deſtillavi; at minore olei quantitate elicita, quam expectabam, altero die, & etiam tertio, pro re nata, deſtillationem reiterare inſtitui: Quantum memini, quarto die, poſteaquam Oleum aquam ſupernatans ſeparaveram, viſus eſt mihi alius liquor, cum aqua non permiſtus, vaſis ſeparatorii fundum petiiſſe; hoc etiam educto, inveni eſſe Oleum Macis, aqua ponderoſius: Ita ut ex eadem planta, eademque operatione, Oleum diſpari gravitate extraxerim. In ſuperiore parte hujus Olei, aliquid cryſtallinæ formæ hodie fluctuare videtur; attamen cujus generis ſit ſubſtantia, ſive Camphora, ſive non, tempus docebit.

P. S.

Poſtquam differentias inter noſtrum Oleum Thymi coagulatum & Camphoram orientalem ſupra deſcripſeram, Dominus *Johannes Manel*, Chymicus, oſtendit mihi Oleum Majoranæ coagulatum, quod nuper, & inopinato in vaſe, (in quo Oleum Majoranæ quinque annos aſſervatum fuerat) invenit.

Hoc Oleum coagulatum Majoranæ etiam eadem experimenta, cum Oleo noſtro Thymi coagulato, ſubire volui, quod & factum eſt, & multum inter ſe convenire reperiebam, ita ut hoc nihilo magis, quam noſtrum, nomen Camphoræ mereri, ſatis ſciam.

III. *Two Letters on the Effects of* Lightning, *from the Reverend Mr.* Joſ. Waſſe, *Rector of* Aynho *in* Northamptonſhire, *to Dr.* Mead.

WE are told by Mr. *Jeſſop*, in the Tranſactions, that what the common People call *Fairy Circles*, are occaſioned by Lightning; but I think it has not yet been obſerved, that they continue viſible 50 Years, and that no Compoſition of Uſe in Fire-works will produce near ſo laſting an Effect, as I have experienc'd. There ſeems to be ſomething here, which

Sulphur and Nitre will hardly account for. Does it depend upon the great Quantity of the Matter diſcharged, or the Violence with which it is impell'd? The Ground is no way tore up, and the Graſs is only a little blaſted; which would make one think its Force well nigh ſpent: Whereas, when the Burſt is near us, the Effect is like that of a Petard, as appears from the following Inſtance.

On *Saturday July* 3, at *Mixbury*, three Miles Eaſt of this Place, about two in the Afternoon, *William Hall*, aged above Sixty, was found dead in a hard gravelly Field, together with five Sheep, which lay round him about 30 Yards Diſtance: Of the five, that only, which lay neareſt him, had a viſible Wound through the Head. The Shepherd lay partly upon his Side; the upper Part of his Head was terribly fractur'd, and his right Knee was out of joint: He had a Wound in the Sole of his Foot, towards the Heel; his right Ear was cut off, and beaten into his Skull, and Blood flow'd out of that Part upon the Ground. He is ſuppoſed to have been driving thoſe Sheep. All his Cloaths and Shirt were torn into ſmall Pieces, and hung about him; but from the Girdle downwards were carried away intirely, and ſcattered up and down the Field: Particularly the Soles of a new ſtrong Pair of Shoes were rent off. His Hat was driven to Pieces: I have a Hand breadth of it full of irregular Slits, and, in ſome few Places, cut as with a very ſharp Pen-knife, and a little ſinged in the upper Part. His Beard, and the Hair of his Head were, for the moſt Part, cloſe burnt off. The Iron Buckle of his Belt was thrown 40 Yards off, and a Knife in the right Side Pocket of his Breeches was broken in Pieces, not melted, and the Haft ſplit. Near each Foot appear two round Holes about a Yard deep, and five Inches Diameter, which ſhews the Force of the Blow. I have ſeen an Iron Ball ſhot out of a Mortar almoſt perpendicular, which, upon a like gravelly Soil, made not a greater Impreſſion. About the Time this Accident happen'd, a Tradeſman of the ſame Town obſerved a Sort of Fire-ball, as large as a Man's Head, to burſt in four Pieces near the Church. The Storm began here at 1^h 30′, and laſted, with Intermiſſions, to 2^h 30′, and we ſaw the Lightning towards *Ailesbury* all the Evening. Two Perſons at *Aynbo* were a little hurt at the ſame Time, and one of them ſtruck down to the Ground, and ſays, he thought he was felled with a Beetle. I myſelf heard the Hiſs of a Ball of Fire, almoſt as big as the Moon, which flew over my Garden, from S. *E.* to *N. W.*

I Thought I had been impertinently circumſtantial in the Account of the late Storm; but there ſtill remains a Particular or two to complete it. I order'd my Nephew, a Student of *Merton* a pretty good Philoſopher, to ſearch the Holes made by the Blaſt, which the Owner of the Ground ſaid, his Conſcience permitted him not to do. He prevailed upon him, at laſt, with Money, Ale, and other *rural* Arguments, to trace the Path-way of it. Both the Holes, at firſt, were almoſt perpendicular for half a Yard, and after that grew narrower; in both of them, the Matter divided into two Parts, and form'd horizontal Cavities about three Inches Diameter. In one he found a very hard glazed Stone, of about 10 Inches long, 6 wide, and 4 in Thickneſs, crack'd in two: Others it could not pierce, but was turned here and there out of its Courſe, but left not the leaſt Blackneſs, or other Diſcolouring any where. As to the Knife, it was not the Blade, but the Haft, and the

Hinge that goes into it, which was ſhiver'd in Pieces. Near the Sheep that was wounded, the Ground was tore up near two Yards round. It was very ſurprizing, that the Man's Body was not beat to Pieces, or Bones broke at leaſt. Had *Statius* obſerv'd a human Tabernacle withſtanding a Shock ſo terrible, when the very Stones, lodg'd in the Ground, gave Way, he would, no doubt, have put him upon a Level with *Capaneus*;

— — — toto Jove fulmen adactum
Corripuit: primæ fugere in nubila criſtæ
Et clypei niger umbo cadit; jamq; omnia lucent
Membra viri.

Exuiturq; animus; paulum ſi tardius artus
Ceſſiſſent, potuit fulmen meruiſſe ſecundum.

To make a groſs Eſtimate of the Force, I took a *Coborn* charg'd with three Quarters of a Pound of very good Powder, wadded with thick Paper, and fired it againſt a Stone of the ſame Dimenſions, but not ſo hard, which it ſhatter'd to Pieces at half an Inch Diſtance: But, in the other Blow, we have above treble the Effect, without any diſcoverable Particles at all; and yet it ſeems to fly like ſmall Shot; pierces only here and there, and leaves a good many Places quite untouch'd, as is evident from the Hat which I have by me. To confirm this, *James Marſhal* of this Town aſſures me, that in the Middle of the ſame Storm, he received a Blow upon his Hat, which rattled like Shot through the Branches of a Tree: It beat in the Crown a little without penetrating it: He ſtagger'd and was giddy for two Days afterward. Two of his Sons were, at the ſame Inſtant, both knock'd down to the Ground, and ſtunn'd a little, but preſently came to themſelves, and have no Wound:

They are about 20, and 23 Years old. *Qu.* whether this may not be accounted for, by ſuppoſing the Flame to rarify the Air, and make a Sort of *Vacuum* about one; into which when it returns again, it gives the Likeneſs of a Stroke with a Beetle, as he expreſſes it. I fancy a Wind-Gun, with compreſs'd Air, wou'd have the ſame Effect, and might eaſily be try'd upon a Dog, or ſuch like Animal.

IV. *De Viribus Magneticis. Epiſtola Viri Doctiſſimi* P. Muſchenbroek, *M. D. ad Rev*^{m}. J. Th. Deſaguliers, *LL. D. R. S. S.*

QUum motus, quos vocamus Attractiones, frequentiſſimi videantur in Natura, & plurimi effectus ab hiſce pendeant, ad eos animum advertere cæpi, ut intelligerem quales eſſent, quænam eorum cauſa, & quibuſnam proportionibus operarentur: cum vero in magnetibus videantur hi effectus fortiſſimi, ſuſpicari cæpi an ex iis non aliquantum plus lucis fænerarer, quam ex aliis corporibus; & an, quod in majori gradu obtinet inter magnetes, non obtineret in minori forſan inter quælibet alia corpora; ad experimenta cum magnetibus igitur veni, plura diverſa inſtitui, quæ infra deſcribam, ſed poſt plurimos labores videor illud tantum colligere poſſe; magnetes non agere in ſe, aut in ferrum, per effluvia quædam corporea, ſed cauſam eorum eſſe penitus ignotam, & forte non corpoream. Conjecturis non detinebo te, nec pretioſiſſimum, quod tibi ſupereſt à gravioribus negotiis, ſurripiam hiſce vanis cogitationibus tempus; credo enim nihil eſſe magis noxium promovendæ ſcientiæ, quam ſi conjecturas pro demonſtrationibus obtrudamus.

Primum igitur volui experiri an magnetes in ſe operarentur juxta certam proportionem in diverſis diſtantiis, vidique in Actis Britannicis, N°. 335. *pag.* 506. ſimile veniſſe in mentem experientiſſimo *Hauksbejo*, ſed ipſum inſtituiſſe experimenta cum magnete & acu modo tali, qui omnibus non ſatisfaceret accuratis rerum ſcrutatoribus, unde concludit tamen his verbis. *I ſee no Reaſon to doubt, but the Proportions of this Power will be regular, and agreeable to the ſeveral Diſtances.* Quæ verba non adeo placuiſſe omnibus Eruditis colligo, cum Nobiliſſimus *Taylor* eadem experimenta repetiit, N°. 344. *pag.* 294. & alia reliquit obſervata.

Rem eandem aggreſſus fui methodo prorſus diverſa, ſic meditatus, ſi ſumerem duos magnetes & unum Suſpenderem ex filo ſupra alium, ad diverſas diſtantias à ſe invicem, ſique fili extremum annecterem bilancei, me ponderare poſſe quantitatem virium, quibus magnetes in ſe agebant; neque ſucceſſu caruit meditatio. Sumpſi accuratiſſimam bilancem, qua melior nullibi forte exſtitit, & uni brachio annexui lancem, alteri filum longiſſimum plurimorum pedum, cujus parti inferiori adhærebat magnes nudus, filum longiſſimum feci, ne actio magnetis ulla in ferream libram turbaret experimentum; ideoq; ſelegi locum in quo ferri tam parum, ac in ædibus unquam ſeligi poteſt. Sumpſi præſtantiſſimos magnetes perfecte ſphæricos, terrellas veſter *Gilbertus* vocavit, horum poli erant accurate in extremo utroq; axeos ſphæræ, ita accuratiſſime diſtantias amborum polorum menſurare poteram, Gravitatem magnetis primo reduxi ope ponderis in altera lance in æquilibrium; dein ambos magnetes poſui infra ſe, & quia libra erat, ope funis, mobilis ſupra trochleam, eam demittebam ad diverſas diſtanſtias pro lubitu, & cum magnes ſuſpenſus ageretur deorſum attractus vi magnetis inferioris, ſemper imponebam tantundem ponderis alteri lanci, donec vis magnetis cum pondere faceret æquilibrium: hæ tamen diſtantiæ menſurari non poſſunt, niſi interponendo corpus cupreum tantæ longitudinis ac eſt diſtantia inter ambos magnetes, ob oſcillationes libræ, & quia in majoribus diſtantiis magnetes minus operantur, quam in minoribus diſtantiis, æquilibrium libræ obtineri non poteſt, niſi eo artificio. Ecce nunc tabulam continentem experimenta in diverſis diſtantiis pollicum & linearum, columna remotior continet grana quæ æquiponderant cum attractionibus in iis diſtantiis.

Diſtantia		Grana
Poll.	Lin.	attractionis.
13 —	6 —	0
12 —	0 —	0 $\frac{1}{10}$.
11 —	0 —	0 $\frac{1}{8}$.
10 —	0 —	0 $\frac{1}{4}$.
9 —	0 —	0 $\frac{1}{2}$.
7 —	6 —	1 $\frac{1}{8}$.
7 —	0 —	2 $\frac{1}{8}$.
	12 —	70 $\frac{5}{8}$.
	11 —	78 $\frac{1}{2}$.
	10 —	87.
	9 —	94.

	Lin.	Gran.
	8 —	106.
	7 —	114.
	6 —	131.
	5 —	146.
	4 —	172.
	3 —	190.
	2 —	215.
	1 —	250.
	$\frac{1}{2}$ —	290.
In ipſo contactu, ſive	0 —	340.

Pollices ſumſi Rhenolandicos, & grana ſunt pondera noſtra medica, quæ etiam ſumma acribeia prius examinavi, ut eſſent vera & æque gravia.

Hæc experimenta inſtitueram die 24 *Decemb.* 1724. & animo adeo ad omnia attento ne hallucinarer, ut vix ſperaverim melius fieri poſſe.

Sed an ex his colligere poſſumus ullo modo dari proportionem inter vires & diſtantias? ego non video; tu vero vir, qui oculatiſſimus es, & cui tantum conſiderem in judicio ferendo de rebus phyſicis, imprimis de experimentis, quantum ulli mortalium, perpende an quidem aliquid regularis adſit, excute omnes proportiones, invenies nullas; evolve curvas cognitas, nec felicior eruendo eris.

Poſtquam eo uſque perrexeram, ſuſpicabar an non forte ſuſpenſus magnes eſſet heterogeneus utcunque, & an alius ejus ſubſtitutus loco, eventum quoq; non daret magis proſperum, ſaltem ex quo plus lucis caperem, tædioſa enim nimis fuerant hæc experimenta quam ut inde tam parum emolumenti colligerem; ſed ecce quid cum alio magnete parvo, admodum præſtanti, obſervare datum fuit, dum magnes rotundus alius inferior idem maneret, firmiter in menſa poſitus: eodem autem modo experimenta inſtituta fuerunt.

Diſtantia		Grana
Poll.	Lin.	attractionis.
5 —	10 —	1 ¾.
4 —	6 —	2 ¼.
3 —	9 —	3.
2 —	4 —	9.
1 —	9 —	12.
1 —	0 —	23.
	11 —	23 ½.
	10 —	26 ¼.
	9 —	29.
	8 —	30 ¾.

Lin.	Gran.
7 —	33.
6 —	38 ½.
5 —	43 ½.
4 —	50 ½.
3 —	62.
2 —	79.
1 —	140.
½ —	186.
0 —	340.

Sed irregularitates hic iterum adſunt maximæ, ex quibus concludi poteſt nihil: id ſolum eſt mirandum, quod dum magnes pro hoc ſecundo experimento fuerat minor, quam qui primo inſerviverat, tamen in mutuo contactu viribus æqualibus attrahebatur, nempe 340 granorum; dum in aliis diſtantiis longe minus tamen attracta fuit, uti ex comparatis ambabus tabulis patet: ſed præterea hic minor magnes ſecundi experimenti multo generoſior fuit & præſtantior ad elevandum ferrum, quam magnes primi experimenti.

Hæc experimenta repetii cum aliis magnetibus & imprimis cum aliquo, cujus tanta vis ut acum magneticam inflectat, quæ diſtat ab ipſo 14 pedibus Rhenolandicis; neſcio an ſimilis deſcriptus ullibi habeatur: ſed ex omnibus id modo concludere poſſum experimentis, proportionem inter vires & diſtantias dari nullam.

Quum tam declinatio, quam inclinatio acus magneticæ variat ſingulis annis fere, ſubiit quoque deſiderium videndi, an vis magnetis omni die eſſet eadem, an minor vel major æſtate quam hyeme; ſed vim eſſe minorem æſtate quam hyeme, me docuerunt experimenta multa, ſaltem de hac æſtate loquor, an futuro anno idem obtinebit, explorandum erit.

Sumſi igitur magnetes binos, qui primo experimento inſervierant, & eodem prorſus modo inſtitui experimenta cum iis, ac ante, dies vero fuit 11 *Julii* 1725. cum Baroſcopium eſſet elevatum ad 29 $\frac{5}{34}$ pollic. Thermoſcopium *Fahrenheytii* ad 62 grad. & Ventus *Noordten Weſten* ſive Septentrionalis verſus Occaſum, cælum ſiccum, ſerenum, & in eodem loco mearum ædium.

Diſtant.

Diſtantia Poll.	Lin.	Grana æqualia attractioni.	Lin.	Gran.
12 —	0 —	0	7 —	106.
9 —	0 —	$1\frac{1}{3}$.	6 —	111.
8 —	0 —	$1\frac{2}{3}$.	5 —	132.
7 —	6 —	2.	4 —	149.
7 —	0 —	$2\frac{1}{2}$.	3 —	173.
	12 —	$70\frac{1}{2}$.	2 —	205.
	11 —	$75\frac{1}{2}$.	1 —	240.
	10 —	85.	$\frac{1}{2}$ —	270.
	9 —	92.	0 —	300.
	8 —	100.		

Conſtat quidem inter Philoſophos, magnetis utriuſque polos non agere æque fortiter, ſed polos boreales eſſe fortiores viribus quam auſtrales, ſed hoc aſſertum quidem, demonſtratum accurate fuit nullibi; quia vero noſtra methodus ponderandi vires magnetis ſatis facilis erat, at accurate ejus ope hoc determinari poſſe videbam, converti modo ambos polos amborum magnetum ita, ut iterum duo poli amici ſibi eſſent obverſi, & in magnetibus hujus ultimi experimenti hæc obſervavi.

Ad diſtant. Lin.	Grana æqual. attractioni.	Lin.	Gran.
12 —	57.	5 —	101.
11 —	63.	4 —	113.
10 —	66.	3 —	124.
9 —	70.	2 —	148.
8 —	79.	1 —	168.
7 —	83.	0 —	228.
6 —	90.		

Ex his patet manifeſto non ambos polos magnetis agere viribus iiſdem, quanta autem intercedat differentia, ex comparatione ambarum tabularum videre poteris: brevis eſſe cogor, qui tibi epiſtolam, non tractatum tranſmitto, & jam ante verebar, ne prolixo ſermone tædium creaverim.

Quum ab ineunte ætate, qua primum operam Philoſophiæ dare inceperam, in hunc uſque annum mihi perſuaſeram actionem magnetum ab effluviis, vel aliquo ſaltem fluido pellente extus magnetem, pendere neque videram præſtantiſſimos viros aliter ſenſiſſe experiri volui an ullo experimento hanc meam opinionem confirmare poſſem, affirmare enim effluvia vel aliquod fluidum premens extus, & non demonſtrare, mihi viſum fuit nimis temere conjecturis dare operam. Dum igitur experimenta priora cum magnetibus inſtituebam ad varias diſtantias à ſe mutuo, interpoſui fruſta craſſiſſima plumbi, ſtanni, argenti, cupri, mercurii maſſam inſignem, viſurus an hæc effluvia magnetica non impedirentur, & ſi non omnino, ſaltem aliquo modo; vitrum pellucidum eſt, lucem tranſmittit, tamen non adeo copioſam, quam ſi nullum vitrum adfuiſſet; eodem credidi modo effluvia magnetica, ſi non prohiberentur omnino, ſaltem impediri aliquo modo ne magnetes tam fortiter ad ſe traherent, ſi plumbi fruſtrum 1 pedis cubici interpoſitum foret, vel ſi plumbum craſſitiei 2 digitorum, & ſtannum ejuſdem craſſitiei, tum cuprum, tum maſſam magnam mercurii interponerem; ſed vidi, quæcunque interpoſueram corpora, ſemper vires magneticas eſſe eaſdem, ac ſi nullum corpus interlocaretur; hoc profecto mirandum exiſtimo, neque intellectum credo ab ullo mortalium: non enim fingere licet hæc corpora eſſe adeo poroſa ut nihil ſolidi in ſe habeant; quod ſi igitur ſolidum habent, ut habent plurimum, an hæ partes non impedient quominus fluidum extraneum adveniat, vel ex magnete exeat; non dico quod omne fluidum impedient, ſed ſaltem aliquid, experimenta tamen omnia docent, vires magneticas impediri nullo modo: vel an hæc effluvia erunt multo ſubtiliora luce? præterquam quod hæc eſſet iterum hypotheſis, difficultas ſuperior non tollitur; Ignis impeditur à corporibus, lux non penetrat illico per omnia corpora, & ita ſe habent fluida omnia ut à ſolidis reſiſtentiam experiantur, ſed effluvia magnetica ita ſe non habebunt, immunia erunt reſiſtentiæ à ſolido corporeo; hoc eſt, ad quod mens noſtra plane hebeſcit.

Sed argumentum fortiſſimum ex viribus repellentibus magnetum depromam, ſunt hæ longe debiliores, quam vires attrahentes, uti mox experimentis confirmabo, adeoque oportebit ut fluidum accedat ab exterioribus verſus magnetem, quod dum occurrit alteri magneti, unum pellit ad aliud, quodque magnetem ingreditur, & quia attractio magnetum eſt longe fortior quam repulſio, copioſius fluidum ingredietur magnetem, quam egreditur: unde fieri non poteſt quin brevi magnes adimpleatur hoc fluido, ut non amplius poroſus maneat; nec ſtatui poteſt quaſi undiquaque ex magnete fluidum hoc exiret, nam fit attractio in omni puncto magnetis, & fit tantum repulſio in locis polorum. Ut vero demonſtrem repulſionem magnetum eſſe minorem attractione eorundem, ecce tabulam continentem experimenta cum memoratis ultimis magnetibus facta.

Diſtantia Poll.	Lin.	Grana æqualia repulſioni.
13	0	0.
11	11	$\frac{1}{2}$.
10	9	$\frac{1}{4}$.
9	9	1.
9	0	1.
8	0	1$\frac{1}{4}$.
7	0	1$\frac{1}{4}$.
6	1	2.
5	1	3$\frac{1}{2}$.
4	0	6$\frac{1}{3}$.
2	9	11$\frac{1}{2}$.
2	3	13.

	Diſtantia Poll.	Lin.	Grana æqualia repulſioni.
	1	11	16.
	1	10	17.
	1	4	17.
	1	0	24.
		10	24.
		7	25.
		6	25$\frac{1}{2}$.
		5	27$\frac{1}{2}$.
		4	29.
		1	34.
In ipſo contactu.		0	44.

Ex his experimentis circa repulſum magnetum iterum videbis non poſſe deduci ullam proportionem, ſed profecto magnetes eſſe corpora admodum miranda, de quibus huc uſque pauca ſcimus, ignoramus plurima. Longe alia diverſaque inſtitui cum his corporibus tentamina, ſed quæ nimis prolixa forent, quam ut nunc adderem.

Trajecti ad *Rhenum*,
30 *Julii* 1725.

V. *A ſhort Account of the* Anomalous Epidemic Small-Pox, *beginning at* Plymouth *in* Auguſt 1724, *and continuing to the Month of* June 1725. *By the Learned and Ingenious Dr.* Huxham, *Phyſician at* Plymouth.

THE *Small-Pox* were preceded by the uſual Symptoms of that Diſtemper; but the Pains of the Limbs, and Back, were generally more ſevere than common, as were likewiſe the *Nauſea* and Vomiting. Abundance were ſeized with violent Colick Pains, which would leave them upon the Eruption, or after a Clyſter or two, with a gentle Anodyne: The Stools were commonly bilious.

It ſometimes happen'd, that the Symptoms would not ſeem very ſevere before, and at the Eruption, and yet the Pox would prove very confluent and fatal at the State.

The *Puſtules* were very ſmall, and did not regularly fill; but, in a Day or two after the Eruption, would flat and be depreſs'd in the Middle. I obſerv'd this even in the diſtinct Kind. In ſome Perſons they appeared in leſs than 24 Hours from the Seizure: When they broke out ſo very ſoon, they were always of the Flux Kind, as is commonly obſerved. The Eruption was attended with prodigious Sneezing, eſpecially in Children. I ſaw one * Child about five Years old, that ſneezed inceſſantly for more than 30 Hours, nor could it be allay'd, but by Anodynes. This Child had the *Confluent Pox*, and dy'd the 13th Day. In ſome, at, and after the Appearance of the

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* *Mr. Cockey's* Child.

Puſtules, they would itch moſt intolerably: This happen'd alſo to the Child now mentioned, and was generally a bad Symptom; as it was an Argument of the great Acrimony of the morbific Matter.

In ſome few, a Day or two after the Eruption ſeem'd to be compleated there would appear in the Interſtices of the Pox ſeveral miliary Puſtules, ſome of a dark Red, others filled with a limpid *Serum*: Theſe never came to Suppuration, as the ſecondary Crop of *Small-Pox*, which I have now and then obſerv'd, ſometimes do; nor were they as large. Tho' this is an ill Symptom in general, yet, in a * Girl of ſeven Years old, I perceiv'd her Fever and *Delirium* go totally off upon this Eruption, and the Urine immediately ſettled.

Some had abundance of Purple *Petechiæ* appear among the Pox at the Eruption, and the Puſtules would look of a lurid Hue: In others, the Purples would not diſcover themſelves, till the Maturation. I knew but † one Perſon that had theſe Spots, during this Conſtitution, that ſurviv'd the Diſtemper; but ſome dy'd the 5th or 6th Day, ſome dwindled on till the 10th or 11th.

During the Suppuration, the Pox would become very ſeſſile, and the coherent Kind would enlarge their Baſes exceedingly; ſo that, though they ſeem'd for ſome Time after the Eruption to be very diſtinct, they would now flux together. A Purple Speck would often appear in the Center of the Puſtules, which would ſpread and grow blacker and blacker by degrees. The Interſtices would alſo ſometimes turn pale, ſometimes livid. Symptoms of very ill Omen! The Puſtules, that had not the Purple Speck, did not incruſt yellow, but appear'd of a dead, aſh Colour, and by degrees grew into a dark black Cruſt.

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* Miſs *Spurrel*. † Mr. *Lang*'s Son.

The Salivation, which conſtantly ought to accompany the Maturation in the *Confluent Small-Pox*, was in ſeveral very inconſiderable, in ſome none at all, ſaving a very ſmall Quantity of extremely viſcid Matter, which was got off by ſyringing. I had two adult Perſons, and ſome Children, labouring under the confluent Sort, who neither ſalivated, nor purged, except when ſome lenient Cathartick were given them; and yet they got over the Diſtemper. Indeed, it was very rare, that we ſhould find Children have that gentle *Diarrhœa* which *Sydenham*, and others juſtly reckon, ſupplies the Salivation in Perſons of more advanced Age. Some very young Children, on the contrary, drivell'd exceedingly through the Courſe of the Diſtemper. In * two Children, one of five, the other of ſeven Years old, no Salivation came on till after the 13th Day, and then it was ſo profuſe, and continued ſo long, that it was with Difficulty I put a Stop to it by Purges firſt, and then by the Bark, Aſtringents, *&c.* To the younger of theſe, indeed, I had given *Calomel*, Gr. iv, but it was ſoon purged off.

Where the Swelling of the Face and Throat was very hard, painful, and tenſe, with a ſtrong Vibration of the carotid Arteries, and little or no Salivation, the Patients generally grew delirous at the State. Theſe Symptoms frequently prov'd fatal. The maxillary and parotid Glands, of thoſe that recover'd, would remain ſwoln and indurated for a conſiderable Time after the intire Deſquammation of the Pox, (though that was very ſlow) nor would theſe Tumors go off, but after repeated Purging, and that with *Calomel*, &c.

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* Mr. *Wallis*'s little Boy, Mr. *Collier*'s Daughter.

Thoſe Tumors were undoubtedly the Conſequence of a very viſcid Matter obſtructing thoſe Glands, which harden'd the Swelling of the Face, hinder'd the Salivation, and in ſome Meaſure, the Circulation through the external Carotids; by which Means, more Blood being forc'd through the Internal, an Inflammation of the Brain, and a *Delirium* might be partly brought on; and this happening too, when the Blood was fraught with acrimonious Matter abſorb'd from the Puſtules, render'd the *Delirium*, at that *Stadium* of the Diſeaſe, vaſtly more to be dreaded than in the *Apparatus*, when it happen'd almoſt of courſe. Under theſe Circumſtances, Bleeding, emollient Clyſters, Eccoproticks, plentiful Dilution, were abſolutely neceſſary.

On this Occaſion it may be ask'd, whether or no, the Salivation being very viſcid and defective, the Tumor of the Face hard and tenſe, ſome Mercurial (as a duly prepar'd *Calomel*) might not be given with Advantage, even in the State of Maturation? (I have frequently given *Cinnabar* to good Purpoſe). There are ſome Inſtances that would ſeem to juſtify ſuch a Practice; and I know but one material Objection to it, and that is, that the Weight of the Mercury would, by encreaſing the Moment of Motion of the Blood, augment the Fever; but ſurely we have given *Calomel* after the Incruſtation, when the ſecondary Fever hath ſubſiſted without any Manner of ill Conſequence, I might ſay, with great Succeſs.

Nothing ſo certainly fuſeth viſcous, tough Humors, being join'd with plentiful diluting Liquors, as this, and ſo prepares them to be diſcharg'd by proper Outlets. As to *Oxymel Scillit.* Syringing, and the like, in a defective Salivation; the former, indeed, by Puking, ſometimes irritates the Glands of the *Mem-*

brana Schneideriana to diſcharge their Contents; Syringing barely deterges the Mouths of the *Ductus Salivares*: Either have little certain Effect further; whereas the viſcous obſtructing Matter is lodg'd in the inmoſt Glands, and even in the Blood itſelf.

This Method ſeems peculiarly adapted to ſuch an *Epidemic Small-Pox*, as I am now deſcribing, in which we had all the Indications imaginable of a very viſcid State of Humors. The Blood, when drawn, was always exceſſively viſcous, eſpecially at the State of the Diſeaſe: Frequently there was little or no Salivation; generally it was extremely glutinous; ſo that the Nurſes were many times obliged to pull the Matter out of the Patient's Mouth with their Fingers; and without drinking very plentifully, it would ſoon ceaſe. A *Diarrhœa* very ſeldom happen'd to Children. The Bliſters ſoon dry'd up. I heard of no one, during this Conſtitution, that made bloody Urine. Where that dreadful Symptom happens, the *Craſis* of the Blood ſeems to be diſſolv'd, (as *Lyſter* well obſerves); on the contrary, the recited Symptoms argued a too compact and viſcous *Diathesis* of the Blood.

This State of the Humors, during this Conſtitution, might, in part, at leaſt, depend on the extraordinary Drineſs of the Seaſon, and the almoſt conſtant Northerly and Eaſterly Winds, which we had in the Months of *October*, *November*, *February* and *March* laſt. From the Middle of *January* to the Middle of *April*, was a drier Seaſon than ever was known in this Country, where we have certainly, in general, more continu'd Rain than in moſt Places in *England*, *Plymouth* being infamous for wet Weather.

This remarkable Change of the Temperature of the Air muſt undoubtedly have ſome conſiderable Effect on human Bodies: A very cold Wind ſuffering only the thinner Part of the Blood to paſs of by Perſpiration: Nor, in ſuch Seaſons, doth the Body imbibe ſo much of a diluting Humidity from the Air (as *Keil* obſerves). Hence the Neceſſity of drinking plentifully of thin diluting Liquors, which, as it is always proper in this Diſtemper, ſo, when it happens in ſuch a Seaſon, is highly neceſſary. And I am of Opinion, Monſ. *Andry*'s Method of bathing in warm Water and Milk, or warm Milk, before the Eruption, may, upon many Accounts, be proper in ſuch a Conſtitution of the Air. There can be no Objection againſt it, but its not being in Faſhion.

I took particular Notice, that while, and juſt after the Eaſterly Winds blew exceſſively ſtrong for ſeven or eight Days together in the Months of *October* and *November*, the Patients, I then ſaw in the *Small-Pox*, ſcarce ſalivated at all. Then particularly, * an *adult* Perſon, who had the *confluent Pox* very ſeverely, did not ſpit the leaſt through the whole Courſe of the Diſeaſe; She was ſeized with a violent *Pleuriſy* the 18th Day, but was reliev'd by Bleeding. The Blood was the moſt viſcid that ever I ſaw. 'Tis remark'd by *Lanciſi*, that People expectorate very little in Diſorders of the Breaſt, when cold, dry, Eaſterly Winds blow; and 'tis what I have frequently obſerved: And this may be one Reaſon, why ſome *Aſthmaticks* generally ſuffer a Paroxyſm at ſuch Seaſons.

The Swelling of the Hands did not ſo regularly ſucceed the *Detumeſcence* of the Face, during this Conſtitution, as I have obſerv'd in other *Epidemic Small-Pox*.

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* *Betty Boddy.*

Pox. Some had very ſmall, or rather no Tumors at all. 'Twas very rare the Legs and Feet ſwell'd, till after the Patients ſat up, and then they had much Pain in the Parts.

I have often reflected upon it, whether the Succeſſion of the Tumors of the Hands, to that of the Face, might not partly depend on the later Inflammation and Suppuration of the Puſtules of thoſe Parts: The Pain and Inflammation being a *Stimulus* determining the Humors to the pained Part: And 'tis particularly to be obſerv'd, that the greateſt Pain of the Hands and Arms commonly happens at the Time, when the Salivation begins to ceaſe: So that the Tumor of the Hands may, in ſome Meaſure, prove a *Succedaneum* to the Spitting. It is the common Obſervation, that the Puſtules of the Arms and Hands inflame and maturate a Day or two later than thoſe of the Face, and thoſe of the Legs and Feet lateſt, which may alſo be the Reaſon, that the Tumor of the Legs ſucceeds that of the Hands. I have been the rather inclin'd to this Opinion; inaſmuch as I have ſometimes obſerv'd a conſiderable Swelling of the Hands (the Puſtules being very painful and inflam'd) and that too in the diſtinct Kind, when there hath been little or none in the Face. Generally the more painful a Boil is, the greater the Tumor around it; and by conſequence, the Tumor of a Part is in proportion to the Painfulneſs of the Boils, and their Number.

From this, I would enforce the Uſe of Epiſpaſticks apply'd above the Wriſts, a little before the Time we expect the Tumor of the Hands ſhould ariſe, (eſpecially when Symptoms are threat'ning) as they are *ſtimuli* to be depended on, not only attenuating and deriving the Humors to the Parts, but alſo diſcharging them, and ſo proving a convenient Outlet to the morbifick

Matter,

Matter, which before was thrown off by the (now partly ſuppreſs'd) Salivation.

Bliſters apply'd to the Neck frequently relieve the extreme Pain of the Throat, and Difficulty of ſwallowing which are ſometimes exceeding troubleſome to the Patient in the third *Stadium* of the *Small-Pox*, by drawing the Humors another Way. Nay, in ſome, where Veſicatories have been early apply'd, and continu'd to run extreamly, there hath been leſs Swelling, and leſs Salivation, than ſeem'd proportionate to the Vehemence of the Diſtemper, but without any Diſadvantage to the Patients; the Running of the Bliſter ſupplying the Defect of the Spitting. It ſeems then but reaſonable, when we expect the Tranſlation of the noxious Humor to the Hands which is what Nature itſelf affects, to endeavour to promote its Flux thither, and give it Vent.

How advantageous Diſcharges of this Nature may be, I had Occaſion ſome Years ſince to obſerve in the Caſe of a Lady, for whom I was concern'd, together with that worthy and ingenious Phyſician Dr. *Seymour* of this Town; where, through the prodigious Diſcharge of Bliſters apply'd to her Neck, Ears, and Arms; as likewiſe a plentiful Flux of Urine, ſhe neither ſwell'd, nor ſalivated, through the whole Courſe of a very dangerous, *confluent Small-Pox*, and yet recover'd.

Any Perſon, that hath been converſant in Practice, cannot but have obſerv'd Tranſlations of the morbid Matter from one Part to another ſometimes of the greateſt Service, eſpecially where it hath had a Diſcharge. (Indeed, all critical Evacuations are of this Nature). But I mean, how often hath a Boil, an Impoſthume, or Swelling of the Limbs, been the evident Means of terminating a Fever? This I experienc'd particularly in myſelf, ſeveral Years ſince at *Paris*; when labouring under a violent, inflammatory Fever with *Delirium*, the 9th Day towards Night, I was ſeiz'd with exceſſive Pain in my Arms and Hands, upon which I bath'd my Hands a long Time in warm Water, by Perſuaſion of two worthy Gentlemen of the Faculty, now living, who were then my Fellow Students, and watched by me. In a little Time my Hands began to ſwell, and in 4 or 5 Hours my *Delirium* and Fever went off intirely, tho' my Hands remain'd ſwoln and pain'd for ſome Time.

If Nature, therefore, in ſome Caſes, take ſuch extraordinary Methods to free herſelf from Diſeaſes, how intent ought we to be in promoting her Operations, in a Diſtemper, where the *Metaſtaſis* of the morbific Matter to the Hands and Feet is generally regular and ſalutary. 'Tis, undoubtedly, upon this View that *Baglivi* orders Sponges ſoak'd in a warm emollient Decoction, to be apply'd to the Hands and Feet in the *Small-Pox*: And this, he ſaith, he hath done with great Succeſs. I have ſeen no leſs from Bliſters maturely apply'd to the Arms and Legs; but then I order'd the Patients to drink plentifully of a thin Whey or the like, which takes off, in great Meaſure, the Acrimony of the *Cantharides*.

Before I conclude this Paragraph, I cannot but obſerve, that the *Delirium*, attending the Eruption of the *Small-Pox*, is very much alleviated by the Application of emollient Cataplaſms to the Feet, in Children eſpecially. 'Tis, indeed, what I do commonly of courſe apply, when I am conſulted at the Beginning of the Diſtemper, and I think I have had Reaſon to imagine, in many Caſes, that it hath been a Means of deriving the variolous Matter that Way; and, by making the Eruptions more copious in the lower Parts, the Face and Breaſt have ſuffer'd leſs than, I had Reaſon to fear, otherwiſe might have happen'd. The great Tenderneſs of the Feet, which happens after their Application, is a trifling Diſadvantage, in Compariſon of the Benefit may be receiv'd by them; and ſo are thoſe ſhooting Pains, which often affect the Legs on the Uſe of thoſe Cataplaſms: Not to ſay, that theſe are rather an Argument of the Benefit ariſing from their Uſe.

In the *confluent* Kind, generally a *Micturition* and *Dyſury* came on about the 12th, or 13th Day; and that when there were no Bilſters apply'd. If a large Quantity of turbid Urine follow'd, it was ſoon ſucceeded by Urine, which depoſed a very large Sediment; but if it prov'd thin and limpid, and in ſmall Quantity, a *Delirium*, *Tremor*, *Subſultus Tendinum*, and other convulſive Symptoms, ſoon follow'd.

There were no Symptoms ſo certainly fatal at the Turn of theſe *Small-Pox*, as a *Delirium*, and (what is, I think, of conſtant ill Omen in all Kinds of eruptive Fevers at the State) a *Dyſpnœa*, or the *Anheloſa Reſpiratio*: If we bled upon the firſt Appearance of them, we frequently ſav'd our Patient; the Omiſſion of which a few Hours made the Caſe irrecoverable.

It was very common in Perſons afflicted with theſe *Pox*, that 8, or 10 Puſtules would run together, and form a large Veſication full of a limpid, crude Matter, which would continue ſo ſeveral Days after the Incruſtation. * In one that dy'd, I ſaw Mortifications under theſe Bladders. I thought it neceſſary to let out this Matter with a Lancet, or Needle, as ſoon as poſſible, leſt it ſhould, (as it did when left to itſelf) cauſe an Ulceration;

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* Miſs *Booſe*.

ceration; and I could not but believe it proper, inaſmuch as the Thinneſs of the Matter render'd it capable of being abſorb'd into the Maſs of Blood, and that the longer it lay, the more acrimonious it would grow, it not admitting of Concoction.

In two Patients, I ſaw ſeveral of the Puſtules fill'd with a bloody *Sanies*: I was ſurpriz'd to find * one of them get ſo eaſily over the Diſtemper, tho' ſhe labour'd alſo under the Flux Kind.

The Deſquammation was very ſlow, the black Cruſts adhering ſeveral Days, nay Weeks, after the Turn, while abundance of purulent Matter gleeted from under them. Theſe left very ugly *Cicatrices*. No Application ſeem'd to me to have a better Effect, in this Caſe, than frequently fomenting the Parts with warm Milk, or Milk and Water; this diluted the acrid Salts, waſh'd them off, and ſoften'd the Skin: Oily Liniments, by ſtopping the Pores, are frequently hurtful.

In a Caſe or two I obſerv'd a Repullulation of Puſtules under the Cruſts in the Face and Hands, when thrown off. This particularly in the Boy, that recover'd with Purple Spots. The latter were diſtinct, tho' the former were in the greateſt Degree confluent.

Nothing ſo certainly abated, and took off the ſecondary Fever after Bleeding, (if indicated) as gentle Cathartics; ſuch as *Rhubarb*, *Manna*, *Tartar Infuſ. Senn.* and the like. The hot, ſcammoniate, aloetic Purgers ſeem not ſo proper, at leaſt, to begin with. Theſe I gave the 10th, 11th, 12th, or 13th Days, if I found the Patient have a quick Pulſe, feveriſh Heat, dry Tongue Head-ach, reſtleſs Anxiety, and other Symptoms of the putrid Fever. Some one, or other of theſe, being once or twice repeated, I gave *Calomel*, and purg'd it off. This was our general Method, and the moſt ſucceſsful.

I can-

Mr. *Baſe's* Child

I cannot but think the World highly oblig'd to the moſt learned Dr. *Friend*, and the other noble Ornaments of our Faculty, that have introduc'd, and wrote in Favour of, this Method. In the Beginning of my Practice, relying upon the Authority of *Morton*, I gave the *Cortex* to check the ſecondary Fever; eſpecially when I found it (as is very frequent) evidently intermit; but I cannot ſay with a Succeſs any way anſwering my Expectation: Not but that after due Purging, the *Bark* is very proper to extinguiſh the hectical Diſpoſition of the Blood, which is frequently the Conſequence of the *Small-Pox*, to which, if a cool Regimen, and Aſſes Milk (where no *Idioſyncracy* forbids it) be ſubjoin'd, we have done, perhaps, as much as lies in the Power of Phyſick.

By this Method of early Purging, in the *coherent* and *confluent Pox*, I have ſeen ſeveral reſcued from the moſt imminent Danger, whoſe Recovery look'd rather like a Reſurrection.

This, ſurely, is the only Way of cleanſing the *Primæ Viæ* ſtuff'd with a Load of fœtid, acrid Impurities thrown off by the Glands of the Guts, which cannot be ſuppos'd to ceaſe from their Office, during the Courſe of this Diſtemper: And foraſmuch as the Pores of the Skin are at this Time very much conſtipated by the incruſted Puſtules, 'tis reaſonable to believe, the Glands of the Guts rather ſeparate more than uſual; it being an allow'd Maxim in Phyſick, That the Leſſening one Evacuation is the Encreaſe of another; eſpecially where there is ſuch a peculiar Conſent, as between the Skin and the Guts.

If ſo the Excrement, being retain'd for a Week, or more, by its Weight preſſing on the great Artery, hinders the Blood from paſſing freely to the lower Parts, and ſo deluges the Brain. Hence thoſe *Deliriums*,

riums, *Coma*'s, *&c.* ſo frequently threatening at this *Stadium* of the Diſeaſe. Further; can we imagine, that the putrid Recrement of the now putrid Blood, join'd, perhaps, with the *Pus* of the internal *Pox*, and having alſo the Addition of ſome Part of the morbifick Matter ſeparated by the Glands of the *Fauces*, which is accidentally ſwallowed, muſt not be greatly hurtful, by remaining in the Inteſtines? where growing more and more acrimonious, (as is the Nature even of our moſt balſamic Juices, when they are *extra Aream Circulationis*, and expos'd to the conſtant Heat of the Body) it contaminates the Chyle, or Liquors, that are drunk, is re-abſorb'd into the Maſs of Blood, and becomes a *Pabulum* to the very Fever, which Nature endeavours, even this Way, partly, at leaſt, to throw off.

And, in very Deed, what horribly offenſive, fœtid, large Stools do we obſerve in this Diſtemper on the Uſe of Clyſters, and more eſpecially after a Purgative? I mean at the State, or in the Declination: So that this very putrid Matter lying long in the Guts, and growing more and more ſo, becomes at laſt ſo virulent, as to corrode them, and brings on that very *Diarrhœa*, or Dyſentery, which, 'tis ſo vainly fear'd, would ariſe from a gentle Cathartic; ſeeing we are always eaſily able to check its Force (if Need be) with an Opiate, *&c.*

How often do we find feveriſh, hectic Heats proceed from a *Cacochymy* of the firſt Paſſages, in Children eſpecially? In which Caſe, a little Rhubarb, or a few gentle ſtomachick Purgers, ſhall do more to remove the feveriſh Diſpoſition than a Pound of the *Cortex*. Indeed, it's common enough to find the *Bark* itſelf purge gently upon its firſt Adminiſtration: And I'm confident, in ſome Caſes, it hath the better Effect. Have

The major Part of the *adult* Perſons, that had the Unhappineſs to be ſeiz'd with this Diſtemper, dy'd; among whom fell an old Gentlewoman of 72; a very uncommon Exit for a Perſon of her Years!

It was a remarkable Inſtance of the extraordinary Virulence of theſe *Small-Pox*, that the Women (tho' they had had the *Small-Pox* before, and ſome very ſeverely too) who conſtantly attended thoſe ill of the confluent Kind, whether Children, or grown Perſons, had generally ſeveral Puſtules broke out on their Face, Hands, and Breaſt, exactly reſembling the pocky Puſtules, which undoubtedly aroſe from the Matter of the cruſh'd Pox infecting the Skin in thoſe Parts. Thoſe Puſtules aroſe, maturated, and ſcabb'd off, intirely like the true Pox. † I knew one Woman, that had more than forty on one Side of her Face and Breaſt; the Child ſhe attended, frequently leaning on thoſe Parts on that Side. I obſerv'd, that thoſe, which had the tendereſt Skins, and who attended thoſe ill of the worſt Sort, had moſt of theſe Eruptions. We had abundance of Inſtances of this Nature.

In the Beginning of this Conſtitution, the *Small-Pox* were much more malignant than they have been for this Month, or two, laſt paſt. Indeed, when they raged moſt ſeverely, ſome Children had them very favourably, and requir'd no other Phyſick than to be duly purg'd after them.

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† Mr. *Hicks*'s Nurſe:

F I N I S.

LONDON, Printed for W. and J. Innys, Printers to
the *Royal Society*, at the Weſt End of *St. Paul*'s *Church-*
Yard.

Plate III.

Fig. 6.

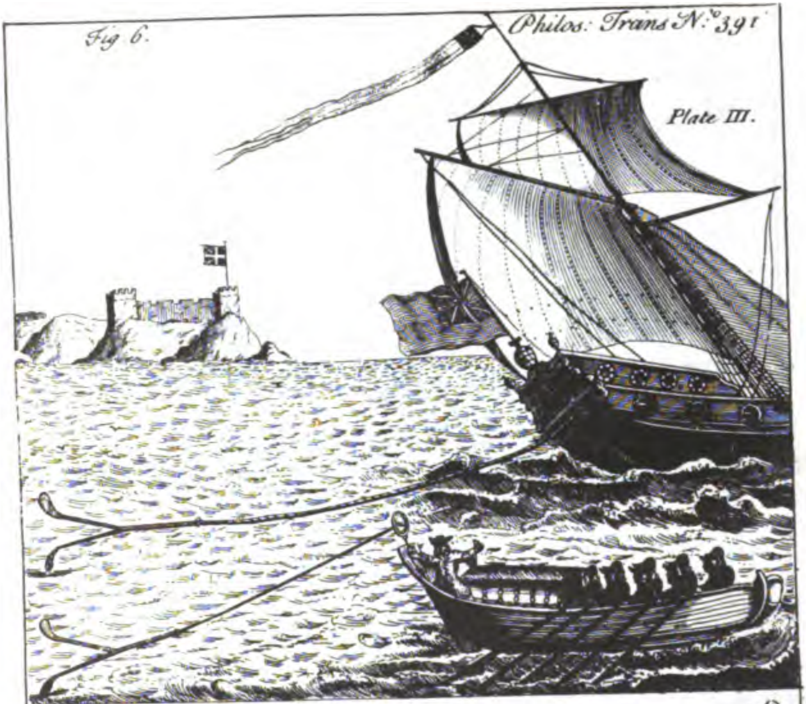


Fig: 5.

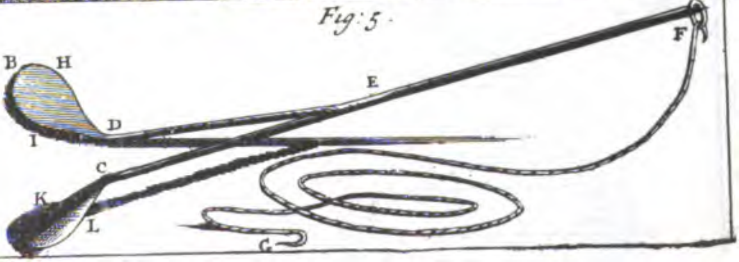


Plate. III.

Fig. 3.

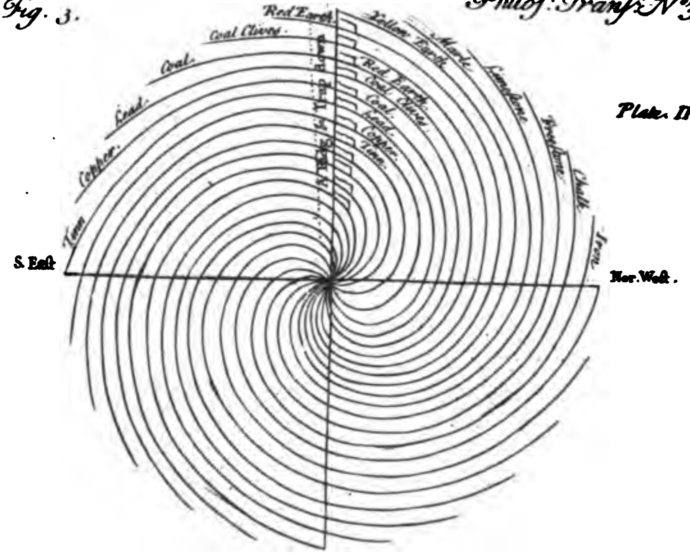
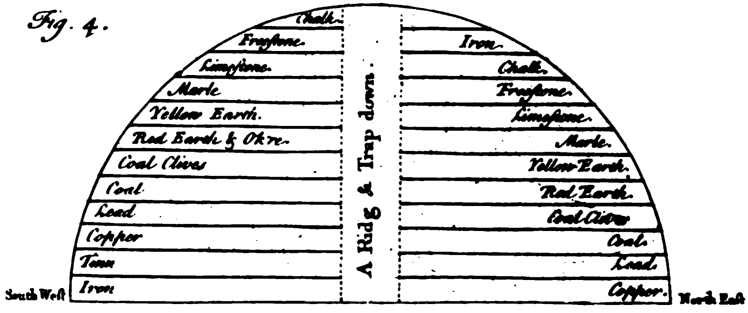


Fig. 4.

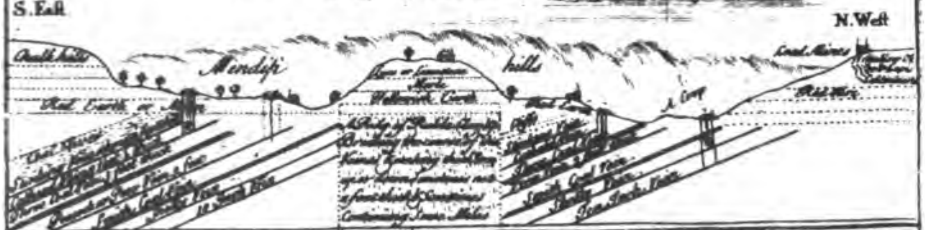


A Section of a Coal Country in Somersetshire about 20 Miles from South-East to North-West

Philos: Trans: N°

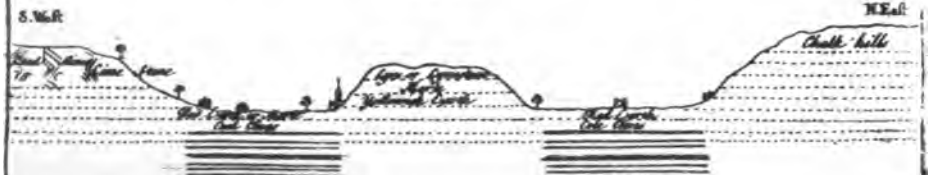
Fig: 1.

Plate I.



A Section of the same Country from North East to S: W: on ye Drift or Level of the Coal & at right Angles with ye former

Fig. 2



Numb. 391.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *November* and *December*, 1725.

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ERRATUM.

N° 390. *p.* 366. *l.* 12. *for* Manel *r.* Maud.

I. *An Account of the* Strata *in Coal-Mines*, &c.

By John Strachey, *Eſq*; *F. R. S.*

IT was ſome Time ſince, that in a Letter to one of the Members of this Society, I gave an Account of the ſeveral *Strata* of Earths and Minerals, found in ſome of the Coal-Works in *Somerſetſhire*, which was printed in *Phil. Tranſactions*, N° 360. But there is one great Error in the Print; for whereas I ſaid, that in thoſe Parts they never meet with *Freeſtone* over the Coal; the Printer, by miſtake, calls it *Fireſtone*; whereas *Fireſtone* is always found in thoſe Mines, contrary to the Works in *Staffordſhire*, *Newcaſtle*, and *Scotland*, where *Freeſtone* does, indeed, lie over the Coal. I have farther obſerved the *Strata* of Stone, Clay, and Marle, of the interjacent Hills, where, under the black Marle, lies a ſpongy yellowiſh Earth; all this lies above the red Soil, which I have ſaid is generally the Surface of the Vallies, where the Coal is found. And as this red Mould on the Surface degenerates into Marle or Loom, ſo, towards the North-Weſt, beyond or without the Veins of Coal, about *Winford*, in the ſame County, it turns to Ruddle, or Red-Okre, uſed chiefly for marking of Sheep, and for ground Colours or Priming, inſtead of *Spaniſh* Brown; and often counterfeits Bole Armoniack.

But as I never heard any Coal was found to the Weſt or South of *Mendip-hills*; ſo *Cotſwold*, to the North-Eaſt, and the Chalk-Hills of *Marlborough-Downs* and *Salisbury Plains*, ſeem to ſet Bounds to the Coal Country, to the Eaſt and South-Eaſt of which *Fig* 1.

may

may be ſuppoſed a Section from South-Eaſt to North-Weſt, *viz.* from the Dip to the Riſe; and *Fig.* 2. at right Angles, from South-Weſt to North-Eaſt, on the Drift or Level.

I mention this by way of Correction and Addition to my former Obſervations of the Coal-Works in *Somerſetſhire.* I have ſince had Opportunities to be underground, and view ſeveral Coal-Works in *Scotland* and *Northumberland*, and to obſerve the ſeveral *Strata* there. At *Widdrington* they have four Fathom Clay, then a Seam of Coal, about ſix Inches thick, not worth working; then a white Freeſtone; then an hard Stone, which they call a Whin; then two Fathom of Clay; then a white ſoft Stone; and under that a Vein of Coal three Feet nine Inches thick. This is a ſmall Coal of the ſame Nature, but not ſo good as the *Newcaſtle-Coal* which comes to *London* Market. Theſe Veins dip to the South-Eaſt, one Yard in twenty. Near *Tranent*, in *Eaſt-Lothian* in *Scotland*, the Coal dips alſo to the South-Eaſt, in the ſame Proportion; but at *Baldoe*, in the Pariſh of *Campſy*, three Miles from *Kylſith*, it dips to the North-Eaſt; and at *Madeſtone*, near *Falkirk*, to the ſame Point, and in the ſame Proportion. The *Strata* of Earths and Minerals, at theſe Places, agree very near: They have, as the Ground riſes or falls, one, two, or three Fathom of Clay; then eleven Fathom of Slate, or Coal-Clives; one Fathom of Limeſtone; under that two Fathom of Slate, Earth and Stone; and then Coal. And all theſe agree in this; that the Pits generally need no Timber, and have a good Roof, which is ſupported by Pillars of Coal, which they leave in the working. At *Baldoe*, the Coal is commonly forty-five Inches thick; and all along, for ſome Miles Eaſtward thence, on the Sides of the

Hills, are Crops of Coal and Limeſtone; and oftentimes the Tenants ſpit up as much as will ſerve their Turn for a Winter's burning, juſt under the Surface; for there wants a Market, and it is ſcarce worth working for Sale. And to the North-Weſt and North, in the Drift of the Coal in higher Ground, and, conſequently, lying over it, there appear, in the Sides of the Hills, Seams of Spar and Lead, the Drift of whch is North-Eaſt, and lies almoſt perpendicular; but what Obliquity there is, pitches to the South-Eaſt. At *Auchenclaugh*, ſix Miles Eaſt from *Kylſith*, there is a Coal eighteen Feet thick; this dips one Foot in three, and is not purſued by reaſon of Water; and, for want of a Market, will not quit the Coſt of draining. At *Madeſtone*, the Coal is four Feet and a half thick, above three Fathom and a half deep: They land it (as at many Coalhews in the Country) on Girls Backs. Near *Tranent* are three different Veins wrought; the undermoſt is about eighteen Fathom from the Surface, call'd the *Splenty Coal*, four Feet and a half thick; it's a hard but not large Coal; makes a clear and ſtrong Fire; lies ten Fathom under the *main Coal*, which is nine or ten Feet thick, and comes out very large. Its Roof is of Freeſtone, under which I walked backward and forward two Hours; but had no Opportunity to make any other Obſervations on the upper Vein, than that it is about four Feet thick, and neither ſo hard or large as the other.

As I have, *Fig*. 1. and 2. drawn the different *Strata* (which have come to my Obſervation) on a ſuppoſed Plane, as they there lie; in *Fig*. 3. and 4. I protract the ſame in a globular Projection, ſuppoſing the Maſs of the Terraqueous Globe to conſiſt of the foregoing, or, perhaps, of ten thouſand other different Minerals, all originally, whilſt in a ſoft and fluid State, tending towards the Center. It muſt mechanically, and almoſt neceſſarily follow, by the continual Revolution of the crude Maſs from Weſt to Eaſt, like the winding up of a Jack, or rolling up the Leaves of a Paper-Book, that every one of theſe *Strata*, tho' they each reach the Center, muſt, in ſome Place or other, appear to the Day; in which Caſe there needs no ſpecifick Gravitation to cauſe the lighteſt to be uppermoſt, *&c.* for every one in its Turn, in ſome Place of the Globe or other, will be uppermoſt; and, were it practicable to ſink to the Center of the Earth, all the *Strata*, that are, would be found in every Part, and according to the Poet, *Ponderibus librata ſuis.* Add to this that in all Places within my Knowledge, the Obſervation of * another Member of the Society has held good, that the Precipices of all Hills are to the Weſtward, whereas the Aſcent to the Eaſt is more gradual. The farther Enquiry into which I offer to the Curious, who have better Opportunity.

II. *De Experimento probandi Spiritum Vini Gallici, perquam uſitato, ſed revera falſo et fallaci. Auctore* C. Neuman, *Chym. Prof. Reg.* Berolin. *R. S. S.*

SI qua rerum naturalium eſt ſcientia, è qua multæ erroneæ opiniones, præjudicia, ſpeculationes, & inde ſecutæ falſæ hypotheſes atque experimenta extiterunt, Chymia in primis eſt; quæ tamen in ſe conſiderata, non niſi certis, in externos ſenſus incur-

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* Dr. *Stukely.*

rentibus, proinde facillimis, & tuto concludentibus rebus occupata eſt; quo magis mirandum eſt, quòd quam plurimi ea de re conſcripti libelli pauciſſima vera in ſe contineant, multo minus neceſſariæ ad rem experientiæ reſpondeant. Hinc etiam aliquantulum modo rei expertus non moratur magnificos & ingenioſe excogitatos librorum titulos, apparationes Chymicas, vanaſque arcanorum oſtentationes, dum protinus, quam fallacibus & incertis opinionibus fundata ſint, perſpicit. Hos flexus Labyrintheos pervadere, eorumque varias aberrationes attingere, non eſt inſtituti noſtri; ſed impræſentiarum tantummodo de experimento quodam Chymico, fortaſſe jam multis abhinc annis frequentato, imaginario, & reapſe falſo ac fallaci, quod tamen quaſi peculiare aliquod arcanum & ingenioſum inventum, adhuc dum occultatur, ſententiam exponemus, eademque opera demonſtrabimus, quod qui tali experimento fidunt, manifeſto decipiantur, & quod ad id negotii requiſita præparatio Chymica nullum prorſus ſit myſterium, uti venditatur, ſed res rudem ſimplicitatem redolens, admodum vulgaris & perquam notiſſima.

Quantum ad rem propoſitam: Nonnulli negotiatores in *Hollandia*, *Anglia*, *Hamburgi*, *Dantiſci*, &c. qui maxime in coemendo & divendendo ſpiritu vini Gallici occupati ſunt, certum aliquod experimentum probatorium crepant, idq; pro magno artificio ac ſingulari arcano venditant, firmiter perſuaſi, ſe ope hujus experimenti non ſolum ſpiritum vini Gallici a ſpiritu frumenti, ſed etiam genuinum ab adulterato diſcernere, atque ita in coemendo genuino ſpiritu vini Gallici, nequaquam defraudari poſſe. Hinc etiam tanquam indubitatum & infallibile probamentum, imo magnum aliquod arcanum, quod non omnes calleant, adhuc exiſtimatur, adeo, ut nemo hunc liquorem probatorium, nec olfaciendum, neque guſtandum mihi concedere unquam voluerit, tametſi fide interpoſita proteſtabar, me illorum commercio nec nociturum, nec artem hanc cum aliquo communicaturum. Quid quod nec ego ipſe, nec illi prænoſcere quibant, utrum ego nudo olfactu vel guſtu conjectando illico aſſecuturus eſſem, quidnam rei ſit, & qua ratione paranda eſſet ?

Subjectum liquor quidam eſt ſubfuſco-flaveſcens, quo adhibito mercatores experimentum iſtud ita peragunt. Complent primo ſcyphum vitreum ſpiritu vini Gallici probando, deque liquore iſto in ſcyphum illum vel unicam, vel duas treſve guttas, pro quantitate ſpiritus vini infuſi, inſtillant. Quod ſi bonus fuerit & genuinus ſpiritus vini Gallici, ſtatim in fundo vitri impleti exiſtit admodum cæruleus color pulcherrimus, qui ſi caule mundiore agitatur, cum reliquo ſpiritu permiſtus, totum vitrum colore cyaneo tingit; ſin autem ſpiritus frumenti ſit, ne quicquam cyanei coloris in vitro conſpicitur, ſed ſpiritus ille colorem ſuum primigenium retinet, etiamſi de liquore dicto vel vigecuplo plures guttæ ſcypho impleto inſtillentur. Sicuti igitur hic modus probandi generatim, putum frumenti ſpiritum a puto puro ſpiritu vini Gallici, ex opinione Mercatorum diſcernit: ita hi conſequenter hac hypotheſi fidentes de utriuſque ſpiritus vini & frumenti adulterinæ commiſcelæ gradibus diverſis judicant. Norunt enim ex conſpecto colore cæruleo, niſi hic obſcure talis, ſed ſubcæruleus modo, cæſius, glaucus, aut ex cyaneo virideſcens appareat, dijudicare, ſpiritum vini non ſolum adulteratum, ſed etiam quoad gradus tot pluſve minuſve partibus ſpiritus frumenti mixtum eſſe.

Equidem lubens fateor, hanc rationem probandi ſpiritus, ubi primum eam animadvertebam, admiratione quadam & delectatione me affeciſſe, ita ut eam omnino certam & falſi neſciam primo adſpectu crederem, quo etiam magis nunc talibus Mercatoribus & Oenopolis, rerum Chymicarum ignaris, inſcrutabilem ſibi errorem condonare poſſum. Quum autem animo mecum ſubinde agitarem, quod nullum adhuc, quantum ſciam, demonſtrativum experimentum proſtat, cujus ope peculiares puri ſpiritus vini Gallici & quidem rectificatiſſimi partes conſtitutivas a ſpiritu frumenti æque rectificatiſſimo diſtinctas oſtendere vel ſaltem ſuſpicari poſſimus, ſed quod uterque ſpirituum ex iiſdem partibus eſſentialibus conſtet, in quam communem qualitatem ſimpliciter neceſſaria fermentatione rediguntur, ideo argumentatus quoque mecum ſum, quod ſi qua notabilis diverſitas in dictis ſpiritibus eveniret, ea non a mixto intrinſeco tanquam mixto ſpiritus vini formante, vel a vini indole ex quo ſpiritus deſtillatus fuit, ſed neceſſario ab additamento quodam heterogeneo, ad conſtituendum ſpiritum vini, qua talem, nullo modo pertinente, ortum ducat, ſive ſub fermentatione aut deſtillatione peregrinum quid addatur, ſive tingendo, extrahendo, vel ipſa commiſcela aliorum liquidorum aut ſolubilium contingat.

Meum non eſt in ſolis ejuſmodi cogitatis, præcipitatis ut plurimum & fallacibus, acquieſcere; quare varia, ad rem pertinentia, experimenta inſtitui, quibus etiam facile impetravi conjecturæ meæ confirmationem, quod ſcilicet in iſta per dictum experimentum apparenter proveniente diverſitate, nulla ſit rationc ſpiritus ardentis vera & eſſentialis diſtinctio, ſed quod productio coloris cærulei, ex quo differentia judicatur, heterogeneo cuidam, & ad ſpecialem conſtitutionem ſpiritus vini plane non requiſiti additamento debeatur, atque ita totum experimentum, quacunque demum veriſimilitudine ſeſe commendans, falſum ſit, fallax, & inutile, id quod jam ulterius comprobabo.

Præmonendum autem mihi eſt quod, cum de additamento hoc ſpiritus vini, liquore ad experimentum pertinente deſtitutus, certior fieri non æque poſſem, operam dederim ſedulam, dictum modo liquorem, utpote ipſummet artificium laudatum, adinveniendi, quamvis ab initio parum in id concitatus, quod eum nunquam, ceu dixi, neque guſtu, neque odoratu ſentire, nec niſi ſub inſtitutis experimentis, eminus intueri licuerit, neque etiam difficultate ſua caruiſſent eum explorandi tentamina (quamvis, ut apparebit, ſimplex ille ſit & ſatis notus) niſi ab amico, qui *Gedani* eum quondam guſtaverat, percepiſſem, quod ſtyptico ſapore ſe exhibeat, quorſum cum omnia mea experimenta dirigerem, tandem reperi, quod arcanum illud tanto in Pretio habitum aliud nihil ſit, quam mera ſolutio Martis in acido vitriolico, ſive ex Marte cum ſpiritu vitrioli ſoluto, & cum aqua diluto, ſive ex vitriolo Anglico, ſive ex vitriolo quodam Venereo-Martiali, præcipitando parato, ſive ex mineræ cujuſdam Martis, qualis Haſſiaca eſt, extractione conſtet, quamvis color cæruleus omnium elegantiſſimus cum ultima, nempe cum ſic dicto liquore terræ Martis ſolaris appareat: quo ſaturatior ſolutio eſt, eo minus ejus pro experimento faciendo requiritur.

Quamvis autem liquoris hujus, utpote jam dudum cogniti, nova non ſit inventio quouſque tamen pro experimento illo probatorio adhibetur, res mihi nova omnino fuit, & perquam grata, tum quod antea cogitando in eam nunquam ſponte inciderim, tum quod in ſecretis haberetur a Mercatoribus, & pro arcano ſollicite ſervaretur, tum etiam & quidem potiſſimum ideo quod per experimenta mea acceſſorium illud, ſpiritui vini, qua tali, peregrinum & minus eſſentiale, colorem vero cæruleum proprie cum liquore dicto producens, quid ſit, & quanta fallacia, imo falſitate manifeſta laboret experimentum probatorium detegere & propalare potuerim, quæ præcipua mihi cauſa fuit, ob quam obſervationem hanc inclytæ Societati exhibere geſtiam.

Additamentum autem illud flavedinem primum ſpiritui vini Gallici concilians, hinc & ſub experimento cæruleum colorem provocans, lignum quercinum eſt, ſive ſegmenta aut ramenta ejus cum ſpiritu vini infundantur, ſive in dolio quercino novo ſervetur ſpiritus, uſque dum flavedinem ex ligno extraxerit; quo magis autem flaveſcit ille, eo magis cæruleus quoque cum liquore martiali evadit ſub experimento, niſi croco vel alia quadam re flava tinctus fuerit.

Quod autem totum negotium non niſi ligno quercino debeatur, experimento ſequenti, in contrarium facto, confirmo: Aſſumo nempe ſpiritum frumenti, eundem, qui cum liquore neque cæruleum colorem exhibet, neque ullam aliam mutationem ſubit, atque ideo a mercatoribus pro puro frumenti ſpiritu judicatur, huic indo ramenta quercina, & infuſa relinquo, donec ſpiritus frumenti flavedinem ſpiritus vini Gallici fere contraxerit, filtrato huic ſpiritui dum liquorem vitriolicum, ſicuti ſpiritui vini Francici, inſtillo, eundem colorem cæruleum & æque elegantem acquiro, nulla ne leviſſima quidem diverſitate, aut diſſimilitudinem indicante phænomeno interveniente; quod abunde teſtatur, omnem mutationem coloris ex ligno quercino originem ducere.

Poſſumus quoque ſpiritum frumenti cum alio quodam, analogia ad lignum quercinum accedente, cum gallis, *v. g* infundere, ſequente ſimiliter experimento. Succedit quoque res quodammodo cum cortice granati & aliis vegetabilibus adſtringentibus, omnium tamen optime cum ligno quercino, cui cortices granatorum longe ſunt inferiores, violaceum potius, quam cæruleum colorem, & cum agitatur, viroris aliquid trahentem, exhibentes.

Notatu dignum eſt, quod perparum liquoris ex ligno quercino extracti ſufficiat ad qualitatem ſpiritui frumenti bene multo inducendam, pro cæruleo colore, cum dicto ſæpius liquore, exhibendo, ſiquidem cum gutta unica infuſi ligni quercini dimidiam ſpiritus frumenti unciam pro experimento ſubeundo aptam reddidi.

Liquorem ſive ſolutionem ex puro vitriolo Martiali, nullatenus Venereo, conſtare & paratam eſſe debere, exinde apparet, quod experimentum 1. cum Goſslarienſe, Ungarico, Dantiſcano, omnibuſque ſimiliter mixtis vitriolis, quæ qualitercunque venerea vel cuprea ſunt, pro copia cupri commixti plus minus male ſuccedat, & colorem valde dilute cœruleum, ſeu cæſium producat. 2. Quod cum puro vitriolo Veneris plane non ſuccedat, nec ullum colorem cæruleum provocet, ſicuti id variis experimentis, infra in ejus teſtimonium allatis, tentando didici.

Dicendum adhuc reſtaret ut, quibus de cauſis & quomodo color iſte cæruleus oriatur, explicarem; quia vero rem pro incipiente, vel diluto, atramentorio ſcriptorio habeo, cujus ingredientia, colorem ſuppeditantia, præcipua ex iiſdem, nempe ex vitriolo Martis & adſtringente vegetabili conſtant, de quibus *D. Lemery* ſcite & copioſe diſſeruit (*v.* Hiſtoire de l'Academie des Sciences l'annee 1707.) hunc potius allegare volui illis, qui talibus delectantur, quam obſervationem hanc meam allegatis hujuſmodi complere & extendere. Qui opticis aliiſque Philoſophicis rationibus capiuntur, D. *Boyle*, D. *Newton*, & alios legant, qui de coloribus ſcripſerunt.

Sub finem diſcurſus mei paucis commemorabo, quod inter Gallici vini & frumenti ſpiritum, purum nempe hunc & cum cura deſtillatum, non alia quam peculiaris odoris & ſaporis in Francici ſpiritu vini a vinaceis, petiolis & acinis uvarum oriundi differentia indagari debeat, licet idem quoque odor & ſapor variis modis ſpiritui frumenti conciliari atque hic ita in Francicum adulterari queat, ut eam vel peritiſſimus pro genuino Francico aut ad minimum pro frumenti ſpiritu plane non habuerit, unde apparet; quod uti primo allegatum, ita & alia experimenta ſpirituum ardentium probatoria nullius, vel ſaltem inſufficientis uſus ſint.

E L E N C H U S.

Experimentorum in rem præſentem factorum.

I. Primum octuplas paravi ſolutiones vitriolicas, pro qualibet drachmas duas vitrioli & unciam unam cum dimidia aquæ communis deſtillatæ recipiendo. Habui autem ſolutiones vitrioli. 1. Goſlarienſis, 2. Gedanenſis, 3. Hungarici, 4. De Cypro, 5. Anglici, 6. Martis cum oleo vitrioli parati, 7. Martis ex vitriolo venereo martiali per præcipitationem facti, & 8. ſolutionem mineræ Martis Haſſiacæ, vel ſic dictæ ſolaris, cujus itidem binas tantum drachmas ad dictam aquæ quantitatem aſſumpſi.

II. Deinde triplices feci adſtringentium vegetabilium extractiones per infuſionem, ad quamvis unciam vegetabilis libram unam medicam ſpiritus frumenti, qui à liquore probatorio antea inſtillato nihil plane cœrulei oſtenderat, accipiendo; fuerunt autem extractiones 1. Ligni quercini, 2. Gallar. Turcic, 3. Granatorum corticum.

III. Tertio: Cum tribus hiſce admodum ſaturatis extractionibus, triplicem quoque Gallici vini ſpiritum adulterinum, colorem ſaltem & flavedinem imitando, ex frumenti ſpiritu effinxi, & quibuſvis octo ſpiritus frumenti unciis unam extractionis unciam ſingulatim commiſcui.

IV. Denique: Ordinarium ſumpſi ſpiritum vini Gallicum flaveſcentem, atque etiam ordinarium recens deſtillatum ſpiritum frumenti, & de utroque, ſecundum ordinem modo dictum cum ſolutionibus vitriolicis, experimenta cepi ac ſucceſſive inſtitui.

V. Infudi nempe quavis vice unciam dimidiam ſpiritus vini Francici in vitrum mundum infra acuminatum, & ſolutionis vitriolicæ unicam, vel ſi non ſufficeret ad cæruleum colorem proferendum, duas, tres uſque ad quatuor guttas inſtillavi, vidique diligenter attendendo, quod ſpiritus vini Gallici dictus 1. cum ſolutione vitrioli Goſlarienſis pallide cæruleum, 2. cum ſolutione vitrioli Gedanenſis, & 3. vitrioli Hungarici itidem ſubcæruleum, 4. cum ſolutione vitrioli de Cypro autem nullum plane cæruleum ſed ſubvirideſcentem colorem induerit, 5. cum ſolutione vitrioli Anglici, atque 6. cum ſolutione vitrioli Martis, cum oleo vitrioli parati, itidemque 7. cum ſolutione vitrioli Martis per præcipitationem facti, valde elegantem, ſed cum liquore Martis ſolari ſic dicto, ſeu ſolutione mineræ Martis Haſſiacæ, amœniſſimum colorem cæruleum acquiſiverit. Cauſam diverſitatis hujus ſupra jam dedimus.

VI. Cepi porro pro quovis experimento probatorio dimidiam itidem ſpiritus frumenti unciam, & cujuſvis ſolutionis vitriolicæ unam, duas, tres, imo decem & plures guttas inſtillavi, ſed à nullius neque etiam ipſius liquoris ſæpius dicti commiſcela, quamvis plus ad hunc, quam ad Gallici vini ſpiritum additum fuerit, ullam ne leviſſimam quidem livedinem cœrulei coloris obſervare licuit.

VII. Poſtmodum aſſumpſi dicti ſub numero III. & de eodem ſpiritu frumenti, triplici modo imitando efficti ſpiritus vini Gallici unciam dimidiam pro quovis experimento, eandemque cum ordinario ſpiritu vini Francici factam, ac ſub numero V. commemoratam inſtillationem cujuſvis ſolutionis vitriolicæ inſtitui, & ecce conveniebant experimenta cum genuino, & cum adulterato hoc ſpiritu vini Gallici facta, per omnia phænomena; niſi quod propter combinatas variorum vegetabilium extractiones aliqua hinc inde, quamvis admodum levis differentia interveniret, uti ex ſequentibus apparebit.

VIII. Spiritus frumenti cum extractione gallarum ab inſtillata ſolutione vitrioli 1. Goſlarienſis, itidemque 2. Gedanenſis nigreſcebat quodammodo, 3. cum ſolutione vitrioli Hungarici ab initio, ſub inſtillatione, dabat quidem aliquid cærulei, ſed inter commiſcendum perdebat penitus, 4. cum ſolutione vitrioli de Cypro plane non tingebatur, 5. cum ſolutione vitrioli Anglici eleganter cæruleſcebat, & 6. cum ſolutione Martis utroque modo parati, ab initio quidem cæruleo, ſed commiſcela vero & interagitatione cum caule pennæ, violaceo colore tingebatur; denique 7. cum ſolutione mineræ Martis, & quidem una ſaltem ejus gutta, mixtus hic ſpiritus frumenti egregium acquiſivit colorem cæruleum.

IX. Spiritus frumenti extractione corticum granatorum, ceu dictum, imbutus, cum ſolutionibus vitriolicis, juxta ordinem præcedentem confuſus, parum vel nihil cærulei provocavit, & circa priora quatuor puncta, eodem fere modo, ſe exhibuit, atque ſupradictus frumenti ſpiritus gallarum extractione imprægnatus, in poſterioribus quoque experimentis mox viridiuſculi quid, mox atramento æmulum oſtendebat.

X. E contrario ſpiritus frumenti extractione ligni quercini in ſimilitudinem quandam Gallici infucatus,

cum ſolutionibus vitriolorum pure martialium, & præ cæteris cum liquore Martis ſolari, omnium pulcherrimo colore cæruleo inficiebatur. Cum primis quatuor ſolutionibus vitriolicis vero eodem ſe modo habuit, ac cœteri ſpiritus frumenti fucati, imo non ſecus ac ipſe ſpiritus vini Gallici.

XI. Tandem cum ſatis eſſem confirmatus, rem cum ramentis ligni quercini optime ſuccedere experiri libuit, quantum infuſi vel extractionis ligni quercini, ad minimum pro aliquali ſaltem cœruleato colore producendo requireretur.

Varias ergo quantitates minuendo ſemper parciores, à dimidia drachma ad viginti, quindecim & pauciores guttas deſcendendo, probavi, adeo ut unica gutta ſufficeret totam ſemiunciam ſpiritus frumenti qualitate imprægnando, quæ ad colorem cœruleum, licet non adeo ſaturatum, unica ſaltem gutta liquoris mineræ martis accedente, producendum requiritur.

III. *An Account of a Fork put up the* Anus, *that was afterwards drawn out through the Buttock; communicated in a Letter to the Publiſher, by Mr.* Robert Payne, *Surgeon at* Loweſtofft.

James Biſhop, an Apprentice to a Ship-Carpenter in *Great-Yarmouth*, about 19 Years of Age, had violent Pains in the lower Part of the *Abdomen*, for 6 or 7 Months; it did not appear to be any Species of the Colick; he ſometimes made bloody Urine, which induced me to believe it might be a Stone in the Bladder. He was very little relieved by Phyſick; at length a hard Tumor appeared in the Left Buttock, on or near the *Glutæus Maximus*, 2 or 3 Inches from the

the Verge of the *Anus*, a little ſloping upwards. A ſhort time after he voided purulent Matter by way of the *Anus*, every Day for ſome Time; the Tumor broke; I ſuſpected a Fiſtula *in Ano*, but could not get the Probe, by the Orifice of the Sore, into the *Rectum*. Shortly after the Prongs of a Fork appeared through the Orifice of the Sore, above half an Inch beyond the Skin. As ſoon as the Prongs appeared, his violent Pains ceaſed; I divided the Fleſh between the Prongs, according to the beſt of my Judgment; and after that made a circular Inciſion about the Prongs, and ſo with a ſtrong Pair of Pincers extracted it, not without great Difficulty, Handle and all entire; the End of the Handle was beſmeared with the Excrement, when I drew it out; it is 6 Inches and a half long, a large Pocket-Fork; the Handle is Ivory, but is dyed of a very dark-brown Colour; the Iron Part is very black and ſmooth, but not ruſty. He is likely to do well in a ſhort Time, the Operation being performed the 11th of *Octóber* laſt; he would not own how it came, till a Relation of his, a Gentleman in this Neighbourhood, who ſent him to me to be under my Care, the Reverend Mr. *Gregory Clark*, Rector of *Blundeſton*, on whom, in a great Meaſure, his Dependence is, threatened never to look upon him more, unleſs he would give him an Account how it came; and he told him, that, being coſtive, he put the ſaid Fork up his Fundament, thinking by that Means to help himſelf, but unfortunately it ſlipt up ſo far, that he could not recover it again.

Loweſtofft, 7 *Miles from*
Great-Yarmouth, 5*th*
Novemb. 1725:

Rob^t^ Payne.

P. S. He ſays he had no Trouble or Pain, till a Month, or more, after it was put up.

IV. *Two*

IV. *Two Caſes of Inſects voided by the urinary Paſſage, communicated in a Letter to the Publiſher, by Dr.* Daniel Turner, *Coll. Med Lond. Lic.*

Dec. 6. 1725.

ON the 24th of *November* laſt, a poor Woman, in my Neighbourhood, came to crave my Advice for her Grand-child, an Infant of about 16 Months old, bringing with her a Worm, which ſhe told me the Apothecary had juſt then drawn out of the Child's *Penis*, who had for ſeveral Days before labour'd under great Uneaſineſs; which (as uſually term'd by theſe People) ſhe call'd Convulſions of the Bowels. The Infant was continually drawing up the lower Limbs, and ſtraining at both Sphincters; the Urine ſeemed to paſs with Difficulty for ſome Days, till at laſt there came on a total Suppreſſion, and the Worm, advancing, ſhew'd itſelf at the Extremity of the *Urethra*, when the Apothecary was called up to help them. I examined the Inſect, and found it meaſur'd above 4 Inches, reſembling the Worms uſually excern'd *per Podicem*, of the Earth-worm Kind, but whiter; which made me think at firſt they had been miſtaken, and that the ſame had been that Way excreted. Having directed what I thought proper, I went to the Apothecary, who has the Worm now in his Cuſtody, to be better inform'd. He aſſures me, that, when he came to the Child, he ſaw a preternatural Body, which at firſt he knew not what to make of, hanging half an Inch out of the *Glans* and lying double in the Paſſage: Perceiving it farther advancing, he took hold of it, and with little Difficulty drew forth the ſame.

The

The Summer before the laſt, a Woman ſhow'd me an Inſect of the Maggot Species, with a cruſty red *Galea* over the Snout, and a creſcent or forked Tail, which ſhe had juſt then voided by the urinary Paſſage.

V. *An Account of a new Machine, called the* Marine Surveyor, *contrived for the Menſuration of the Way of a Ship in the Sea, more correctly than by the* Log, *or any other Method hitherto uſed for that Purpoſe; together with ſeveral Teſtimonials, ſetting forth the Uſefulneſs of this Invention. By Mr.* Henry de Saumarez *of the Iſland of* Guernſey.

HAving for ſeveral Years applied my Studies to the Improvement of Navigation, I have fallen on various Projects, as well for the better clearing a Ship off a Lee-ſhore, as for her Steering, Tacking, or Waring, *&c.* which I may probably ſoon publiſh to the World: But what has moſt of all employ'd my Thoughts, has been to contrive ſomething to aſcertain the Way of a Ship in the Sea, more correctly than by the Log, (at preſent in Uſe in our Navigation;) the Errors of which are ſuch, that I am fully perſuaded the unhappy Fate of the brave Sir *Cloudſley Shovell*, was more owing thereto, than to any Errors in Judgment. The melancholy Reflections I made on that national Loſs, was what ſpurr'd me on to find out ſomething that would not only have a regular Motion under Water, but might communicate the ſame to a Dial, or Piece of Clock-Work, within the Ship.

After Variety of Experiments, I was at laſt ſo happy as to anſwer my Purpoſe in ſome Meaſure; and as in *Guernſey* we are at a Loſs for curious Workmen, in the Year 1715. I came to *London*, and furniſhed my ſelf with a Boat, and all the Materials neceſſary for my Deſign. Daily was I on the River *Thames* making Experiments, and was frequently honoured with the Company of ſeveral of the *Literati*, who were greatly pleaſed with my Invention; but, not being fully ſatisfied of the Certainty thereof, they adviſed me to try it on a Standing-Water. According to their Deſire I did ſo, for I obtained a Liberty ſeveral Times of having a Boat on the Canal in St. *James*'s *Park*, where I demonſtrated, to ſuch curious Perſons as favoured me with their Company, the Uſefulneſs of ſeveral Inſtruments; for we there fixed two Poles for Marks, and notwithſtanding I many Times alter'd the Motion of my Boat, by ſometimes rowing faſt, and at other Times ſlow,. yet had my Machine under Water the ſame Number of Revolutions between the Marks. It may, perhaps, be ask'd, how I came to be aſſured that the Revolutions of the Engine under Water are regular, let the Motion be ſwift or ſlow, and that they anſwer the ſame Diſtance? I will not yet ſay, that this admits of ſo clear a mathematical Proof, as any Propoſition in *Euclid*; however, I have mechanically found it ſo in a great Variety of Experiments; and that every Turn or Revolution of the Engine under Water juſt meaſures 10 Feet, which I call the decimal hydraulical Circumference thereof.

The *Primum Mobile*, or Soul of this Machine, is in the Form of the Letter Y, and is made in Iron, or any other Metal: At each End of the Lines, which conſtitute the Angle, or upper Part of that Letter, are two Pallets not much unlike the Figure of the

Log; one of which falls in the ſame Proportion as the other riſes. The falling or pendent Pallet meeting a Reſiſtance from the Water, as the Ship moves, has, by that Means, a circular Motion under Water, which is faſter or ſlower, according as the Veſſel moves. This Motion is communicated to a Dial within the Ship (which is fix'd either in the Maſter's Cabbin, or any other proper Place) by means of a Rope (of any convenient Length) faſten'd to the Tail of the Y, and carried to the Dial. The Motion being thus communicated to this Dial, which has a Bell in it, it ſtrikes exactly the geometrical Paces, Miles, or Leagues, which the Ship has run. Thus is the Ship's Diſtance attained; and with equal Eaſe may the Forces of Tides and Currents be diſcovered by this Inſtrument.

The Figures adjoin'd more fully explain the firſt Movement of my Machine.

In Figure 5. A K C L and B H D I are the Pallets, which are work'd from the Legs D E and C E into the Form they appear, to a Breadth of about 4 Inches and a half. The Length of the Pallets (B D and A C) are 8 Inches. The Branches or Legs, D E and C E, are each 15 Inches and a half long, and 2 in Circumference, the Diameter of which is about two Thirds of an Inch; and the Angle C E D, which is contained between them, is 45 Degrees.

The Shank E F is of the ſame Thickneſs or Circumference with C E and D E, and is 27 Inches long. At the Point F there is a Ring, where one End of the Rope F G is hook'd to the Machine, the other End G being fixed to the Dial within the Ship or Veſſel. This Rope may be about 5 Fathoms, more or leſs, according as the Dial is fixed high or low, in reſpect to the Surface of the Water.

In

In the Figure afore-mentioned, this Machine has but two Branches; however, it may be form'd of three, if not four, and adjuſted to the ſame Standard or Meaſure: But as three or four Branches would be more ſubject to entangle themſelves in Sea-Weeds, and thereby prevent the regular Motion of the Inſtrument, if not in ſome Meaſure impede the Ship's Way, I cannot but recommend their being made only of two Branches, in the Manner I have laid down; for, in my own Experiment at Sea, I have obſerv'd thoſe made in this Form have been ſo far from being choak'd by Weeds, that if they encountered any at any Time, they have always cleared themſelves of them, without the Trouble of hauling the Engine into the Ship to do it.

To regulate this Inſtrument, it may be done ſeveral Ways; as firſt, by opening or cloſing the Angle CED; ſecondly, by lengthening or ſhortening the Branches, or turning or bending more or leſs the Pallets AKCL and BHDI; and ſo in this Manner the Machine is brought to what Standard or Meaſure you pleaſe, to make the hydraulical Revolution to anſwer either to a geometrical Pace of 5 Feet, or to 10, 12, 14 Feet, *&c.*

The Machines of this Kind, which I have tried at Sea in all Sorts of Weather, did weigh ſome 4, others 5, and others 6 Pounds; the Weight of them not at all affecting the peculiar Property of the Inſtrument, or hindering the Regulation thereof according to the Methods I have laid down.

Theſe Machines may be made of Tin as well as Iron, and ſo light as not to weigh above two or three Pounds, which may ſerve for any Boat, Wherry, Barge, *&c.* without any Hindrance to their Rowing or Sailing. The Manner of fixing them to a Ship, or Boat, is repreſented in *Fig.* 6.

I

I come now to the Explanation of three ſeveral Dials, any one of which may be uſed with this Machine.

The firſt Dial had three Indexes, one of which mark'd 10 Revolutions of the Engine, each Revolution 10 Feet; ſo that of conſequence the whole Round of the Circle was 100 Feet. As five of theſe Revolutions make 50 Feet, which I reckon to be (or at leaſt ſhould be) the Diſtance marked between each Knot on the Log-Line now in Uſe at Sea; by holding the Half-minute Glaſs in one's Hand (which is always uſed with the Log-Line) one may, by Inſpection, ſee how many Times 50 Feet ſhe runs in half a Minute, and of courſe how many Miles in an Hour, without the Trouble of employing four or five Hands, as there generally is, in heaving the Log. My ſecond Index on this Dial marked 100 Revolutions, which makes 1000 Feet, as the third Index did 1000 Revolutions, which is equal to 10,000 Feet; and then a little Bell ſtruck, ſignifying when the Ship had ſailed that Diſtance, which may be alſo fitted to ſtrike to any other Meaſure.

My ſecond Dial had the Circle on its Plate divided into twelve Parts, ſo that as the Index paſt each Diviſion, the Ship had run one Mile, and conſequently twelve Miles, when it had meaſured the Circumference. On one Side of this Dial, I had fixed another Plate, which was graduated in ſuch Manner, that by the Half-minute Glaſs I could alſo, by Inſpection, tell what the Veſſel run in that Space of Time, *&c.*

On my third Dial I had three Circles; the firſt was ſo divided, as to ſhew when the Ship had run 60 Leagues; the ſecond was ſo contrived, as to ſhew when the Ship had run the ſame Diſtance in Miles; and on the third was mark'd 120 Knots; ſo that, computing each Knot at 50 Feet, the Circumference was 6000 Feet, which I take to be the Standard of an *Engliſh* Maritime Mile, or the $\frac{1}{60}$ Part of a Degree upon the Equator; in running which Length, my Inſtrument has juſt 600 Revolutions; to which Diſtance a little Bell ſtrikes to give Notice, to the Man at the Helm, of the Diſtance ſailed in that Time.

Beſides the ſeveral Circles on this Dial (graduated as I have mentioned) I had alſo two Plates on each Side, having two Circles; one divided into 100 Leagues, and the other into 300 Miles; ſo that, without hearing the Bell ſtrike to every Mile or League, one might at any Time ſee by them, what Number of Miles or Leagues the Ship had run, from the Time ſhe had left her Port.

As to the Materials within the Dial, there is little more than common Clock-work.

As by this Machine I undertake to correct the Errors of the Log, I flatter my ſelf that a Compariſon between that Inſtrument, and my Invention, will not be unacceptable to the Curious; I therefore preſent you with

A Comparative Diſcourſe between the Log *and my Inſtrument, which I chuſe to call the* Marine Surveyor.

1ſt. "THE firſt Error I chuſe to touch on, in relation to the *Log*, is in the half and quarter Minute Glaſſes; I think I may well affirm, that they are ſeldom or never true, in regard it rarely happens that we can find two to finiſh their Courſe in the ſame Space of Time; yet, if they did run their Sand out equally, it is no Demonſtration of their Truth, ſince two, that are falſe, may do the

" ſame, as well as two that are true. But, admitting " they were never ſo truly made, they are notwith- " ſtanding ſubject to Error, ſince it is but too well " known, that dry and wet Weather have a great In- " fluence on them. Should the Half-minute Glaſs " lack but two Seconds, or be two Seconds too long, " it makes an Error of ſome Miles in 24 Hours. If " the *Log* be hove by Quarter-minute Glaſſes, in like " manner defective, (which is the general Practice, " when the Ship has great Way) in doubling the " Knots, the Error is alſo doubled. Beſides, when the " Ship runs after the Rate of 8 or 9 Miles an Hour, " (and the Line is left to run off of the Reel) it " rarely happens but ſome Fathoms are out, before " the Line can be ſtopp'd; though this may be ſmall " in the Courſe of 24 Hours, and therefore diſregard- " ed; yet in a long Voyage it will make a great " Addition to the many Errors in the Diſtance (which " we gain by the *Log*) which, added to thoſe of our " Judgment, occaſions ſo many that keep Journals " at Sea, to be a Shore, when they have reck- " oned themſelves 50, 60, or more Leagues from " the Land; and others to be as many Leagues " from their Port, at the Time when they have " expected to make it.

" In the *Marine-Surveyor* it is not ſo; for this In- " ſtrument requires no Glaſſes of any Kind: Let the " Ship run faſt or ſlow, it is the ſame, for it works in " Proportion, and the Bell ſtrikes to every Mile ac- " cordingly. To evidence the Truth of this, I take " Leave to mention an Inſtance, *viz.* When I was " making my Experiments on the Canal, the Reverend " Dr. *Deſaguliers*, one of the Members of this honou- " rable Society, and ſeveral other Mathematicians, " at Times, were with me, and we meaſur'd out a

" certain Diſtance there; upon which I fitted my " Machine to ſtrike to that Diſtance and accordingly " it did ſo. We then alter'd the Motion of the Boat, " and row'd much faſter to the Mark than we had " done before; however, the Bell ſtruck, when we " came up to it, to the greateſt Exactneſs: And ſuch " is the Property of this Inſtrument, that it may be " fitted to ſtrike to Miles, Leagues, *&c.* as ſhall be " thought proper. This Machine is made of Materi- " als ſo durable that one of them ſhall laſt 50 or 60 " Years; and ſuch is the Price, that they will prove " as cheap or cheaper to the Government, than the " *Log*, which is attended with an Expence of ſo ma- " ny Lines, Glaſſes, *&c.* As for the making a Trial " of this Inſtrument, it may be as fully done in the " Channel, as in an *Eaſt-India* Voyage; for if it an- " ſwers to 20, 30, or 40 Leagues, the Reaſon holds " good for as many Thouſand.

2*d.* " The chief Property of the *Log* is to have it " ſwim upright, or perpendicular to the Plane of the " Horizon. This is too often wanting in *Logs*, be- " cauſe but few Seamen examine whether it is ſo or " no, and generally take it upon Truſt, being ſatis- " fied, if it weigh a little more at the Stern than the " Head. What erroneous Reckonings flow from hence " is but too evident; for if the *Log* does not ſwim " upright, it will not hold Water, neither remain " ſteady in the Place where it is heav'd, ſince the " leaſt Check of the Hand, in veering the Line, will " make it come up ſeveral Feet. This repeated, the " Errors become Fathoms, and perhaps Knots, which, " how inſignificant ſoever they may ſeem, are Miles " and Parts of Miles, and amount to much in a long " Voyage.

“ In-anſwer to this, the *Marine-Surveyor* is of ſuch “ a Property, that there is no Neceſſity to take Care “ about its ſwimming; and it is a conſtant Truth, “ peculiar to this Inſtrument, that be the Ship's Moti- “ on on the Water what it will, whether ſhe runs one “ Mile faſter or ſlower than another, yet all ſhe runs, is “ exactly mark'd on the ſaid Inſtrument, as appears “ plainly from ſome Tables of Experiments made by “ me in the River *Thames*, for obtaining the gradu- “ al Increaſe and Decreaſe of both Ebb and Flood.

3. “ The ſtretching and ſhrinking of the *Log-Line*, “ is another great Error in the Uſe of the *Log*; for “ when a new Line is firſt us'd, let it be ever ſo well “ ſtretched upon Deck, and meaſured as true as poſ- “ ſible, it ſhrinks after wetting conſiderably; and “ therefore if we rely on the Line run out for the “ Ship's Diſtance, we ought to meaſure and alter the “ Knots on it every Hour before we uſe it; but I “ am well aſſured that this is ſeldom done oftner than “ once a Week, and ſometimes not above once or “ twice in a Voyage. What great Dependance then is “ there on a Reckoning kept by the *Log*? Since in “ this Caſe the Line will ſhrink ſo, as to add Miles to “ the other Miſtakes of every 24 Hours. Again, “ when the Line is meaſured to its greateſt Degree “ of Shrinking, it is generally left there; and when, “ by much Uſe, it comes to ſtretch again, it is ſel- “ dom or never mended, although it will ſtretch be- “ yond what it firſt ſhrunk. In ſhort, ſuch are the “ Errors incident to the *Log*, that I don't wonder at “ our Neighbours the *Dutch* for preferring their “ *Chips* or an irregular Pulſe to it; which conjec- “ tural Reckoning of theirs is obtained after the fol- “ lowing Manner. They fix two Marks on the “ Side of the Ship at a certain Diſtance, when an ex-

“ perienced

" perienced Perſon, ſtanding at the foremoſt Mark, " throws a Chip over-board, and counts the ſeveral " Beats of his Pulſe, during the Chip's Paſſage from " one Mark to the other; and from thence it is they " compute the Number of Miles that the Ship runs " in an Hour.

" As for the *Marine-Surveyor*, it is not hove with " a Line, but is tow'd a Stern by a Rope; and let " that Rope ſtretch or ſhrink (be long or ſhort) it is " all one, for the Inſtrument will have the ſame true " Revolutions. Should it be objected, that it holds " Water, I affirm, from my own Experiments of it, " that the *Log* haul'd in from 5 or 6 Knots, is much " heavier upon the Hand; and that the faſter the " Ship runs, the leſs Water this Inſtrument of mine " holds, becauſe it gives Way to the Water and turns " quicker; nay, I can venture to ſay, that it is ſo far " from being any conſiderable Impediment to the " Ship's Way that ſhe does not loſe one Mile in an " hundred by it. But ſhould this Inſtrument be intro" duced into the Navy, in caſe of chaſing an Enemy, " or the like, it may be taken in at any Time, and " let down again at Pleaſure.

4. " I appeal to all Seamen, if in a moderate Gale, " when the Ship runs 5 or 6 Knots, two diffe" rent Perſons (every way qualified) were to heave " the *Log* immediately after one another, whether " they would exactly agree. Surely no. Since 'tis " but Chance if they do ſo, and is what may not " happen in an hundred Trials. I therefore affirm " the *Log* to be very erroneous on this Account and " that the Error frequently increaſes with the Wind; " for in a ſtiff Gale, when a Ship has run about 8 or " 9 Knots before the Wind, it has been known that " two expert Seamen have hove the *Log* in this Man-

" ner, and on their comparing Notes, they have found
" a Knot Difference; ſometimes it has been more, and
" at others leſs, which muſt certainly make a ſtrange
" Confuſion in the Reckoning. Under this Head I
" take leave to obſerve, that when the *Log* is hove,
" it is ſometimes in ſo ſtrong a Gale, that the Ship
" runs 9 Knots; but before it is hove again, there
" may be ſuch a Decreaſe of the Wind, that for half
" of the Hour ſhe may not run above 5 Knots. Her
" true Diſtance ſailed then, is the Mean between the
" Extremes of 9 and 5; but this has been ſo far from
" being conſider'd by ſome *Chalkers of the Log-board*,
" that it is but too well known, the Extremes have
" been put for the Mean, and the contrary. Were
" there Truth in the *Log*, two Ships in Company
" would nearly have the ſame Account; but it is
" otherwiſe; for we too often find many Leagues Dif-
" ference in Reckonings, even on board the ſame
" Ship. In a word, ſuch Errors have been found in
" the *Log* by ſome of my Acquaintance, that when
" they have ſailed between a Meridian and a Paral-
" lel, the whole Difference on the *Log-board* has not
" prov'd *Difference of Latitude* enough to agree with
" their *Obſervation*, although each Day they had a
" good obſerv'd Latitude, and no Currents.

" In the *Marine-Surveyor* we are ſo aſſured of the
" Ship's Diſtance, that all Ships ſhall agree which are
" in Company, as to their Reckonings, ſave that ſome
" Allowance be made for Difference of Judgment in
" the ſeveral Perſons who keep Journals.

There are ſeveral other Caſes equally, if not more momentous than what I offer here, wherein the *Marine Surveyor* will be found to have the Preference of the *Log*; but I forbear to mention them, chuſing rather to entertain you with undeniable Proofs of the

Uſefulneſs of my Invention, which I perſwade my ſelf will be found to be of ſingular Advantage to the Trade and Navigation of my Country.

The following are the Subſtance of two Affidavits, taken under the Seal of the Royal Court at *Guernſey*, by ſome expect Seamen, who have had Trial of my Inſtrument, *viz.*

“ KNOW ALL MEN BY THESE PRESENTS, that
“ on the 30th of *November* 1720, there perſonally
“ appeared before *William Le Marchant* Eſq. (Judge
“ Delegate in the Iſland of *Guernſey*, &c.) Meſſieurs
“ *Jean Andros*, and *Eleazar Le Marchant* (Jurats of
“ the Royal Court of the ſaid Iſland)

“ *William Abier*, aged about 40 Years, who com-
“ manded ſeveral Privateers in the late War, (and
“ particularly that call'd *La Chaſſe*, of about 150
“ Tuns, 16 Guns, and 140 Men) and is now Maſter
“ of the Ship call'd the *Eagle*, of which Veſſel he is
“ the only Proprietor, who voluntarily makes Oath,
“ that on *Sunday* the 9th of *October* 1720, he parted
“ from *Southampton* with ſeveral Gentlemen Paſſen-
“ gers on board for *Guernſey*; that he had fix'd at the
“ Stern of his Ship a new Invention call'd the *Ma-*
“ *rine-Surveyor*, projected, to the beſt of his Know-
“ lege, by Mr. *Henry de Saumarez*, a Gentleman of
“ the Iſland of *Guernſey*, for correcting the *Log*, &c.
“ That after they had left the *Needles*, they had a
“ ſtiff Gale of Wind, attended with a rolling Sea,
“ notwithſtanding which, the Machine work'd as re-
“ gularly as if it had been ſmooth Water, the little
“ Bell of it ſtriking to every Mile the Ship run with
“ great Exactneſs. And this Deponent further de-
“ clares, that having thoroughly view'd and examined
“ the Experiment of this new Invention, he finds it
“ to be not only practicable, but preferable to the
“ common

“ common Methods us'd at Sea for attaining the “ Ship's Diſtance ſail'd; that therefore, for the pub- “ lick Good, he doth atteſt the Truth of the above- “ mentioned Particulars. In witneſs whereof, the “ Seal of the Royal Court of *Guernſey* is hereunto “ affix'd by us the under-written,

William Le Marchant, Judge Delegate.
Jean Andros, } Jurats.
Eleazar Le Marchant, }

The other Affidavits runs as follow, *viz.*

“ KNOW ALL MEN BY THESE PRESENTS, That “ on the 30th of *November* 1720, there perſonally “ appear'd before *William Le Marchant* Eſq; Judge “ Delegate in the Iſland of *Guernſey*, &c. Meſſieurs “ *Jean Andros* and *Eleazar Le Marchant*, Jurats of “ the Royal Court of the ſaid Iſland.

The following Perſons, *viz.*

“ *Abraham Le Meſurier*, of about 48 Years of Age, “ formerly Captain of ſeveral Ships,

“ *Peter Bonamy*, of about 58 Years of Age, for- “ merly Captain of ſeveral Ships, and who has uſed “ the Sea above 40 Years,

“ *John Hardy*, of about 38 Years of Age, formerly Cap- “ tain of ſeveral Ships, *William Abier*, about 40 Years “ of Age, and formerly Captain of ſeveral Ships; and “ *James Hubert*, of about 27 Years of Age, who has “ alſo been Maſter of ſeveral Veſſels, who volunta- “ rily make Oath, that on the 19th of *October* 1720, “ they ſet Sail in the Morning out of *Guernſey Pier*, “ with a freſh Gale of Wind, in a Sloop call'd the “ *Dolphin*, in Company with ſeveral Gentlemen of

" the ſaid Iſland, in order to make an Experiment at
" Sea of a Machine call'd the *Marine-Surveyor*, pro-
" jected, to the beſt of their Knowledge, by Mr.
" *Henry de Saumarez* of *Guernſey*; which Invention
" is intended to correct the many Errors of the *Log*,
" *&c.* And they further declare, that they have not
" only throughly view'd, conſider'd, and examin'd the
" ſaid Machine, but have alſo made ſeveral Experi-
" ments of it in a rough Sea, ſometimes ſailing right
" before the Wind, then quartering; at other Times
" turning to Windward, and then lying by to know
" the Drift of the Ship both with and againſt the
" Tide: That having tried the ſame Invention all
" Manner of Ways, they find it much preferable to
" the *Log*, or any of the Methods in uſe for ob-
" taining the Ship's Diſtance run, having nothing to
" object againſt it, as to its being a *Clog* or Hindrance
" to the ſailing of the Ship *&c.* That being fully
" ſatisfied of the great Uſefulneſs of this Invention
" for the Improvement of Navigation, and the Service
" it may be of to all the Maritime Powers, they
" publickly atteſt the Truth of the above-mentioned
" Particulars, to the End the Author thereof may
" make ſuch Uſe of it, as he ſhall think moſt proper.
" In witneſs whereof, the Seal of the Royal Court of
" *Guernſey* is hereunto affix'd by us the underwritten,

" Signed by the Judge Delegate and Jurats, as
" above-mentioned.

Here you have ſome Proof of the Uſefulneſs of this new Invention, and that from Seamen of long Standing and Practice: But, notwithſtanding theſe Teſtimonials, I was yet determined to have it tried further: Accordingly I made a Preſent of one of my Machines to a Friend of mine, Captain *John Thoumes*, who beſides his Knowledge in the Theory and Practice of Navigation, was the better qualified to make Trial of it, in regard he had ſometimes accompanied me in my Experiment on the Canal in St. *James's Park*, and in the River *Thames*. As he was then going a Voyage, I intreated him to act impartially with me, and to loſe no Opportunity in letting me know how far, and with what Certainty, my Invention might be depended on. Agreeable to my Requeſt, he wrote twice to me on this Occaſion: His firſt Letter was dated at *Nantes* the 20th of *October* 1724, and the following is an Extract of it, *viz.*

" According to my Promiſe, I am to acquaint you,
" that I have had as favourable an Opportunity as I
" could have wiſh'd for, to try your *Marine-Surveyor*;
" for ſome Part of my Voyage being from St. *George's*
" Channel to the Bay of *Biſcay*, I paſs'd cloſe to the
" Land's-End of *England*, with a moderate Gale of
" Wind at North our Courſe *S.* by *E.* When I had the
" Land's-End Eaſt of me about 3 Miles, I began to
" reckon, and the next Morning, when *Uſhant* bore
" Weſt, about 5 Miles Diſtance, the *Surveyor* had
" made juſt 37 Leagues. Theſe two noted Headlands,
" which are very near under the ſame Meridian, dif-
" fer in Latitude about 33 or 34 Leagues. As for
" the Tides, we croſs'd them having in this Run two
" Floods and two Ebbs; and as the Wind blew croſs
" the Channel one Tide was no more influenced by
" it than the other, nor could the Current be any
" Impediment to the Trial. Now as to our having
" 3 or 4 Leagues more than the true Diſtance, the
" Reaſon is very plain, ſince it cannot be expected
" but that a Ship before the Wind will deviate from
" her true Courſe, ſometimes one Way, ſometimes
" another, in her *Yaws* and *Sheers*. Of this all Sea-

" men are ſenſible. What I would remark from hence
" is, that the *Surveyor* meaſures all the little Traverſes
" exactly; 'tis therefore the Buſineſs of the Navigator
" to allow for this, when he works the Ship's
" Run. But I cannot help obſerving here, that a good
" Effect is produced from theſe little Traverſes being
" ſo meaſur'd; for ſhould we be running boldly on
" the Land in a dark Night, it forewarns us to look
" out in time, by marking ſomewhat more than the
" true Diſtance ſail'd upon a ſtreight Line.

" Many are the Advantages which accrue to Navigation
" by this Invention, which I ſhall not take
" upon me to enumerate: In ſhort, the Sailors are in
" love with it, and when at the Helm, they value
" themſelves on chalking more Miles than thoſe who
" went before them. For my own Part, I am ſo
" pleas'd with it, that I have done with the *Log*.
" One excellent Quality I obſerve in it, which I
" cannot omit mentioning, *viz.* That in plying to
" Windward along Shore in a dark Night, our uſual
" Way, by the *Log*, is to ſtand two or three Hours
" out, and ſo many in; and here we may be a ſhore
" before we are aware, becauſe in running out we
" may not have had ſo much Wind as in running in;
" or we may have reef'd Topſails, ſhorten'd Sail,
" hanker'd in the Wind, or have met with many
" other Impediments, which, by being drouſy in the
" Night, a Man may ſometimes not take Notice of;
" but it is otherwiſe with the *Surveyor*; for if the
" Ship is hindered in her Way, it will not mark more
" Miles than ſhe has run.

" I have ſhew'd it to ſome curious Perſons at
" *Nantes*, who are greatly delighted with it. They
" wanted to ſee the Movement within, but I ſhall
" never grant that to a Stranger. I have been offer'd

" fifty Piſtoles for it, and might have had more, would
" I have parted with it; but I value the worthy Donor
" of it too much, to do any ſuch Thing.

P. S. " When I ſaid my Courſe from the *Lands-End*
" to *Uſhant* was *S. b E.* it muſt be underſtood
" that I did not go on the Outſide, but paſs'd within,
" between *Uſhant* and the Main: For in the other
" Caſe, to paſs to the Weſtward, the Courſe had been
" about *S. b W.* to go clear of all.

The ſecond Letter, which I receiv'd from Captain *Thoumes*, in relation to my Inſtrument, was dated at *Guernſey* the 2d of *September* 1725; and what follows is the Subſtance of it, ſo far as it relates to the *Marine-Surveyor*, viz.

" I am now fully confirm'd of the Uſefulneſs of
" your *Marine-Surveyor*, having tried it, this laſt
" Voyage to *Marſeilles* and *Toulon*, ſufficiently to
" perſuade me, that it is greatly preferable to the *Log*.

" Having in two former Voyages in the Bay of
" *Biſcay* been apprized, that the Ship's Diſtance ſail'd,
" as obtain'd by the *Marine-Surveyor*, was really true,
" yet I was oblig'd every 24 Hours to ſhorten the
" Diſtance by a certain Proportion, that I gueſt to be
" near one ſeventh Part of the Whole; which, from
" the Bearings of Headlands, *&c.* I found conſtantly
" ſo. However, to be better ſatisfied of this Allowance,
" I wanted a long Run, near, or upon a Meridian,
" with good Obſervations, which could not be
" had in the Bay or our Channels; therefore, when
" I ſail'd for the *Mediterranean*, which was in *January*
" laſt, I continued to make the ſame Allowance, and
" caution'd my Mate to make it alſo. It happen'd,
" that for the firſt eight Days, we had hard Gales of
" ſoutherly Winds, attended with violent Squalls of
" Rain, and a diſtracted Sea, inſomuch that we try'd

“ under a double reef'd Main-ſail, great Part of the
“ Time, and drove to the Weſtward, without the Be-
“ nefit of celeſtial Obſervations; yet all the While the
“ *Marine-Surveyor* ſtruck the Miles of our Drift,
“ which are to be ſeen upon our Journals for every
“ Hour; and ſo far did I depend on it, that I did not
“ order the *Log* to be once hove.

“ After the bad Weather, the Wind chang'd with
“ the new Moon, to *N. N. E.* and *N. E.* with a briſk
“ Gale, which gave us a fair Run for five Days, near
“ 50 Leagues every 24 Hours. We had daily Obſer-
“ vations, and our Courſe was near South. Here it
“ was, that I found the one ſeventh of the Ship's Di-
“ ſtance was to be deducted from the whole, and that
“ it was for *Yaws* and *Sheers*, which the *Marine Sur-*
“ *veyor* marks exactly. After this Allowance was
“ made, ſo well did my Reckoning agree with my
“ Obſervation, that when there was 2 or 3 Miles dif-
“ ference, I rather imputed it to the Want of Exact-
“ neſs in my obſerving, or a Fault in the Quadrant,
“ than to the *Marine Surveyor*, in regard my Mate
“ alſo found it to agree to a ſurprizing Exactneſs.

“ Three Weeks after our Departure, I had the
“ Misfortune to loſe the Fork of the Machine, and
“ therefore was afterwards without the Help of the
“ *Surveyor*, till our Arrival at *Toulon*; which Place
“ being one of the chief Nurſeries for Navigators
“ that ſerve the *French* King, I was the more con-
“ cerned for my Loſs; but I in ſome meaſure repaired
“ it, by ordering a Smith to make two ſuch Forks, of
“ nearly the ſame Dimenſions and Turns in the Fins,
“ as I could remember the other had, which ſerv'd
“ there ſo well as to gain the Admiration of all who
“ ſaw me try it. My Merchant was ſo taken with it,
“ that he deſir'd me to ſhew it to a Friend of his, a

" noted Profeſſor of the Mathematicks in the College " of Jeſuits there. He was all Surprize at the regular Motion of the Machine under Water, and more " that it ſhould ſo nicely determine the Diſtance " ſail'd of any Ship or Boat. I ſhould ſwell my Letter to too great a Bulk, ſhould I repeat the Converſation I had with this Jeſuit, who importun'd me " much to ſee the Inſide of the Clock-Work, offering me what I pleas'd for a Sight of it. In a word, " I was deaf to him, and many other Gentlemen of " the Town, who crouded to me every Day on the " ſame Account, and who were all greatly pleaſed " with the Invention.

" The Machine made by my Directions at *Toulon*, " I us'd in my Way home, and found it to anſwer " very well in the Ocean; from whence ariſes this " Remark, which ſufficiently ſhews the Uſefulneſs of " your Invention, *viz.* That even rough ones, made " by a meer Cobler of a Smith, and turn'd by the " Directions of a ſhort Memory which I dare not " truſt in many Things, are capable of anſwering the " End for which you invented them.

" It muſt be noted, that though I allow one ſeventh " of the Ship's Diſtance for her Deviation from her " Courſe, yet ſome Ships are ſo built, that they will " ſteer much truer, and others worſe than ours did; " and in this Caſe the *Marine-Surveyor* ſhews its " Worth; for if two Ships are in Company, the one ' ſteering well, the other ill, the Latter ſhall have " more Miles than the Former on comparing their " Run, although they ſet out from the ſame Port, and " never part Company.

I ſhould be wanting to my ſelf, if I did not produce the moſt convincing Proof of the Uſefulneſs of this new Invention; I ſhall therefore add to the foregoing

Teſtimonials, ſome other Certificates whereby it will further appear, that the *Marine Surveyor* has the Preference of the *Log*, *viz.*

" We the underwritten Maſters of Ships, *&c.* do
" certify all whom it may concern, That this 21ſt of
" *October* 1725, we accompanied Mr. *Henry de Sau-*
" *marez* on board the *Richard* Yatch, in order to
" make an Experiment of an Inſtrument invented by
" him, call'd the *Marine-Surveyor*; and as by it he
" propos'd to aſcertain the Way of a Ship in the Sea,
" much more correctly than by any Thing hitherto
" invented for that Purpoſe, we tried it between *Lon-*
" *don* and *Graveſend* with the *Log* (which we hove
" ſeveral Times) to which it appears to us to have
" the Preference; for by its conſtant and regular Mo-
" tion, the Ship's Diſtance ſail'd muſt be more exactly
" attain'd than by the *Log*; which being hove but
" once in an Hour or two, cannot be ſo correct, in
" regard the Wind may increaſe or leſſen ſoon af-
" ter the *Log* is hove, in ſuch Manner, that it entire-
" ly depends on him who chalks the *Log-board* to
" allow for it. As therefore very conſiderable Errors
" muſt ariſe from thence if a proper Allowance is
" not made for an Increaſe and Decreaſe of Wind; and
" as the *Marine-Surveyor* is not ſubject to this, but
" keeps a regular Motion, according as the Wind is
" more or leſs: We are therefore of Opinion, that
" this new Invention is not only an ingenious Con-
" trivance in its Kind, but is exactly calculated for
" the Ends propoſed. As witneſs our Hands this 21ſt
" of *October* 1725.

" Sign'd in the Original
Michael Hales,
Benjamin Hutchinſon,
Joſiah Hales,
Peter Perchard,
Robert Gamble.

"Theſe

" Theſe are to certify all whom it may concern,
" that I *John Harris*, who have us'd the Sea for thir-
" ty Years paſt and who was lately Mate of the
" *William* and *Thomas*, bound from *London* to *Canſo*
" in *America*, was preſent, when Mr. *Henry de Sau-*
" *marez* came on board our Veſſel and fix'd an In-
" ſtrument at the Stern of her, call'd the *Marine-*
" *Surveyor*, invented by him for aſcertaining the
" Way of a Ship in the Sea, much more correctly
" that by the *Log*, or any Method hitherto in Uſe
" for that Purpoſe: And as he deſir'd us to try it
" with the *Log*, and to make an impartial Report
" whether we found it preferable to the *Log* or not;
" I do hereby in Juſtice to that Gentleman, certify,
" That we kept our Reckoning both by the *Log* and
" this Inſtrument, and do find it much preferable to
" the *Log*, or any Thing that has yet appeared to me
" for attaining the Ship's Diſtance ſail'd; the Truth
" of which I am ready to teſtify on Oath, if call'd on
" to do it. In witneſs whereof, I have hereunto ſet
" my Hand this 15th of *November*, 1725

" Sign'd in the } *Robert Gamble*,
" Preſence of } *Eliſha Dobree*,
John Harris.

It may perhaps be asked, how I came to produce a Certificate from the Mate, and not from the Captain of the *William* and *Thomas*? To which I anſwer, that the Mate left the Ship at *Plymouth*, and came to Town, ſo that I had an Opportunity of obtaining his Opinion of it, without the Captain's, who ſoon after his Arrival in *England*, made the beſt of his Way to the Iſland of *Guernſey*: However, as I had deſir'd him to try my Inſtrument with the *Log*, and impartially report to me, whether he found it preferable, or not, to that Method of obtaining the Ship's Diſtance ſail'd;

he favour'd me with a Letter from thence: His Name is *Thomas Fitot*, and his Letter bears Date the 16th of *November* 1725; it is in *French*, and the Subſtance of it in *Engliſh* is as follows, *viz.*

‘ That he had made uſe of the *Marine-Surveyor* in his Voyage to *Canſo* in *America*, and had been more than ordinarily careful therein, in order to make a juſt Report of it; that he had tried it upon a *Meridian* with good Obſervations, and found it to anſwer his Expectation and to be preferable to the *Log*, particularly in rough and ſtormy Weather; that it had been much admir'd by ſeveral Maſters of Ships, and particularly by Captain *St. Loe*, of his Majeſty's Ship the *Ludlow-Caſtle*, who expreſs'd a great Liking to it. He concludes his Letter with wiſhing I had an Opportunity to peruſe his Journals, whereby it would fully appear how much my Invention is preferable to the *Log*.

Being inform'd, that Captain *Henry Daniell* had come over as a Paſſenger from *Canſo* in *America*, to *England*, in the aforeſaid Veſſel *William* and *Thomas*; and being willing to obviate every Objection that might be brought againſt the *Marine-Surveyor*, I applied my ſelf to that Gentleman for his Opinion of it, who was pleaſed to ſend me the following Certificate.

“ *THESE are to certify all whom it may concern, that I* Henry Daniell, *who have been at Sea upwards of twelve Years, firſt as a Volunteer, and afterwards as a Midſhipman, did lately come over as a Paſſenger in the* William *and* Thomas, *from* Canſo *to* Plymouth, *in which Veſſel there was an Inſtrument fix'd at the Stern of her, call'd the* Marine-Surveyor, *invented by Mr.* Henry de Saumarez, *for aſcertaining the Way of a Ship in the Sea; and as that Gentleman has applied to me for my Opinion of it, I do hereby certify, that we found it much more correct than the* Log; *and that in a Gale of Wind, our Reckoning by it agreed with our Obſervation, which the Reckoning by the* Log *ſeldom did. And I muſt, in Juſtice to that Gentleman, ſay, that we kept our Reckoning both by his Inſtrument and the* Log, *and found it much preferable thereto, or to any other Method for obtaining the Ship's Diſtance. In witneſs whereof, I have hereunto ſet my Hand this 4th of* December 1725.

H. Daniell.

F I N I S.

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